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Chapter 24

Final Epilegomenon: *Noumena* and the Critical Method in Science

> *The truths which are ultimately accepted as the first principles of a science are really the last results of metaphysical analysis practiced on the elementary notions with which the science is conversant.*
>
> Mill

§ 1. The Need for Critical Discipline

In deciding how to best bring this treatise to a close I have been guided by two considerations. The first of these is to prepare for a transition to an applied metaphysic for the not-yet-born science of mental physics. My vision for this is that this science will unite the works of neuroscience, psychology, mathematics, system theory, and philosophy in carrying forward with the grand goal of understanding the reciprocal phenomena of mind and the central nervous system. John Stuart Mill touched upon an important observation in his quote with which this Chapter begins. I think there is very little chance of uniting the divers works of the different specialties so long as “the elementary notions with which the science is conversant” are not shared by all the practitioners of the science. Here I think philosophy, and, more particularly, metaphysics, has a duty to step up and earn a seat at the council of science. *Proper* metaphysics is the science of the general and ought to be re-made into what it once claimed to be: the parent of the special sciences.

That metaphysics has not yet earned its place is a blot on its record but is not a sufficient reason for it to withdraw from the effort. Bloom’s scathing criticism of the state of philosophy in the 1950s and 1960s is well deserved. Fortunately, I think I can detect the faint signs of a new renaissance in philosophy and, if I am not wrong, it is a hopeful sign for the future. Yet it must be admitted that philosophy today is greatly hampered by the fact that many, perhaps most, professional philosophers are too much lacking in an adequate science education. If it is ironic that so many Ph.D.s in the sciences are untrained in philosophy, it is more ironic still that so many practitioners of what Aristotle called “the first science” are untrained in any one of the special sciences. This treatise has brought up again and again how crucial *context* is for ideas; would it not then seem that at least a minor in one of the special sciences would be to the wholesome benefit of philosophy students? But I digress too much so let us get back on topic.

The second consideration has relatively little to do directly with the future of mental physics but perhaps much greater importance to the future of science and science education as wholes. The one benefit of positivism in the 19th century was that it imposed a *discipline* on scientific speculation. Positivism is dead now, and good riddance, but I perceive that the discipline it once enforced is showing signs of decay. There is a rising tide of Platonism starting to appear in the sciences and appearing most obviously in a most unexpected place, namely physics. It is true that our most fundamental understanding of physics is mathematical, but this does not and cannot mean that every mathematically conceivable fantasy should be aired to the public and taught in the high schools. I do, of course, overstate the current situation since it is largely in cosmology and the various offspring of the efforts toward a "theory of everything" that the Platonic tendency appears most pronounced. I also grant that most university and government public relations operations are more to blame than its scientists for overstating scientific findings; if the science were really as depicted by university press releases, many more of us would have given our Nobel lectures by now.

But despite this I think it is becoming increasingly clear that *enthusiasm* is claiming too prominent a place in science today. I think it likely that relatively few of today's scientists have read Bacon's treatise on the harm of "idols" in the pursuit of knowledge. I find this somewhat strange when juxtaposed with the fact that the Royal Society, the founding of which was inspired by Bacon, is the most respected scientific institution in the world and the model that all other nations try to copy. Not the least undesirable consequence of Platonic enthusiasm in science is that it opens the door to and practically invites attempted invasions by religious fundamentalists into the science classroom. It is not without some foundation that these unscientific zealots lay the rationalization of their "right to be heard" to claims that mainstream science is also based on a "faith" they usually equate with and denounce as so-called "secularism," a word they usually pervert to mean "atheism." If humankind ever comes to know another dark age of ignorance, superstition, persecution, prejudice, and inquisition, history warns us that these are the people most likely to bring it on.

The root of the enthusiasm problem, as well as the source of its solution, is metaphysics. Every science practices under some umbrella or umbrellas of metaphysical presuppositions. Today this is best characterized as pseudo-metaphysics because there is no agreed-to system of metaphysics embraced by the scientific community, and because metaphysical prejudices form by accident of personal experience. Furthermore, the most common forms of pseudo-metaphysics are characterized by a presupposition of the priority of some one or another ontology over all other parts of metaphysics. Kant's revolution came from placing first priority in epistemology over ontology. This has been for many scholars often the most difficult thing about the Critical Philosophy to keep in a firm grasp. Even among professional philosophers the ontology-first habit is hard to break, and Kant's epistemology-first priority proves slippery to the fingers[1]. It is worthwhile for us to note that the term "epistemology" was not coined until the 19th century[2]; thus it was not a word used by Kant. Instead he used the word "critique" and applied it to the term "ontology" to re-write its meaning, a redefinition that a fair-minded reviewer of the history of philosophy since Kant's day would have to say failed to catch on.

Although this treatise has not dealt very much with that section of *Critique of Pure Reason* called the Transcendental Dialectic, the most important lesson found therein, and re-emphasized in Kant's Doctrine of Method at the end of this work, is that human reasoning is inherently dialectical. Without a consistent appreciation of the need for *discipline* in speculation even scientific reasoning too easily slips over the dividing line between the transcendental (that which can be known with objective validity) and the transcendent (that which can be thought but never with more than a subjectively sufficient reason and without any basis for objective validity). All transcendent speculation is mere persuasion. When this persuasion clings to modern mathematics for its only foundation and support, it is scientific Platonism.

Thus one purpose of this final epilegomenon is to discuss Critical discipline in theorizing. Our primary topic is: How does one recognize the signpost marking the transition between what one can know with objective validity and what one cannot? In a rare bit of colorful poetry Kant wrote:

> We have now not only passed through the land of pure understanding and taken careful inspection of every part of it, but likewise traveled it from end to end and determined the place for each thing in it. But this land is an island and through nature itself enclosed in unalterable boundaries. It is the land of truth (a charming name) surrounded by a broad and stormy ocean, the true seat of illusion, where many a fog bank and rapidly melting iceberg pretend to be new lands and, ceaselessly deceiving with empty hopes the seafarer looking around for new discoveries, entwine him in adventures from which he can never escape and yet also never bring to an end [KANT1a: 354 (B: 294-5].

Our task at hand is to discuss how to anchor science so that it may plumb the depths of the pools in which knowledge lies hidden without being swept out to sea.

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[1] There are many examples of essays or longer works on Kant where the essayist starts off by pointing out Kant's prioritization and later ends up forgetting it and re-prioritizing ontology in his interpretations of what Kant was claiming. The usual result is to have Kant look like he is contradicting himself.

[2] by J.F. Ferrier in 1854 (*Institutes of Metaphysic*).

§ 2. ‘Reality’ and Experience

Ask a typical scientist for the definition of “science” and you are likely to get some version of the definition given in *The Penguin Dictionary of Science*: Science is the on-going search for knowledge of the universe. Ask what is meant by “knowledge of the universe” and you are likely to get a rather more diverse collection of answers from different individuals. Nonetheless, most of these answers will tend to reflect attitudes Marias described in the following way:

> Men feel an urgent need to concern themselves with the things, with reality itself, to divorce themselves from mental constructions in order to come to terms with reality as such . . . Physics, biology and history come to represent the exemplary modes of knowledge. This attitude gives rise to *positivism*.
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> The initial proposition – to concern oneself with reality – is irreproachable and constitutes a permanent philosophical imperative. But this is precisely where the problem begins: What is reality? . . . With excessive haste, the nineteenth century thinks that it can suppress this question, and affirms that reality consists of *sensible facts*. This is the error that invalidates positivism . . .
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> However the error committed at the beginning of the nineteenth century is more serious because it defines reality – it formulates a metaphysical thesis – and at the same time is so unaware of this fact that it denies the possibility of [metaphysics’] existence; that is, it does not understand its interpretation of reality as sensible facts for what it is, an interpretation, but takes it to be reality itself [MARI: 342].

All of present day science, and most systems of philosophy other than Kant’s, either consciously or implicitly give primacy of place to *ontology* (knowledge of things). In present day science especially, little or no place at all is given to epistemology. This is precisely what Kant’s philosophy reverses, for how can one claim to have knowledge *of* something without a clear understanding of what it is to have knowledge of anything? Because ontology-first thinking is not only the norm for most people but is also reinforced by a lifetime of habits of thinking, it cannot be said too often: Epistemology is prior to ontology. Understanding the nature of human knowledge, i.e. Critique, is crucial to understanding one’s knowledge of everything else.

Most scientists distrust idealism and reject dualism in science. Both are regarded as incompatible with scientific materialism. Because even empiricism as a philosophy inevitably finds itself committed to either idealism or Hume’s skepticism, science and philosophy have rarely been partners for the past two centuries. Professional philosophers are rarely materialists and physical scientists claim to not be idealists (or, at least, tend to deny all idealist tendencies in their work). Joad observed,

> Materialism . . . has not been maintained by any philosopher of the first rank. . . Materialism has historically been professed by amateur philosophers rather than by professionals. While professional philosophers have on the whole tended to be idealists, Materialism is, as we shall shortly see, the natural, indeed it is the inevitable, creed of men of science. Scientists, that is to say, in so far as they

> confine themselves to the assertion of those conclusions and only those conclusions which science justifies, inevitably adopt a materialist outlook. One of the tenets of Materialism is that every event must have as its cause a preceding event or set of events, such that a complete knowledge of the latter would enable an observer with absolute certainty to predict the former. This belief is, it is obvious, a necessary presupposition of scientific method. The laboratory worker, if he is to adopt the method of experiment and verification, must assume not only that every event has its cause, but that like causes will produce like effects. He must, that is to say, proceed experimentally as if Materialism is true, at any rate in the particular department of research with which he is concerned. Not only do most scientists take materialist views in their own departments, but they are apt to extend their application into spheres such as ethics, æsthetics and psychology, whose subject matter does not easily lend itself to materialist interpretations [JOAD: 495-496].

Some scientists, especially many physicists, will take objection to Joad's remark about being able to predict outcomes "with absolute certainty" and call it an outdated characterization that does not apply to modern science. They will likely also quibble about his remarks on causality and his later remarks concerning 'determinism'. The basis of these objections will be laid to the statistical nature of the quantum theory and, perhaps in some cases, to the discovery in the latter half of the twentieth century of 'chaotic' dynamical systems (systems which, although described by deterministic equations, have dynamics so sensitive to initial conditions that in appearance the system dynamics are 'random'). We must grant them the debating point regarding 'certainty and uncertainty'; but as for 'causality' and 'determinism' the practices belie the objections inasmuch as *at the least* 'expected' or 'average' outcomes *are* expected to be predictable through physical theories. There would be very little point to conducting an experiment or developing a theory if one did not expect to learn from the experiment and comprehend through the theory.

Joad goes on to say,

> Mr. Chapman Cohen, whose short book, *Materialism Restated*, contains one of the best modern accounts of Materialism with which I am acquainted, distinguishes three doctrines as characteristic of and essential to Materialism. First, on the negative side, Materialism may be defined as the denial of Supernaturalism. It holds, therefore, that what happens in the world is never the result of the agency of independent spiritual or mental powers, but is always explicable even when, owing to the lack of sufficient knowledge, it cannot at the time be explained "as a consequent of the composition of natural forces."
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> Secondly, Materialism makes a positive assertion as to the way in which things happen. It involves, in Mr. Cohen's words, "the belief that the state of the world, or of any portion of it, at any given time, is the exact consequence of the distribution and conjunction of forces preceding that moment." It involves, further, the view that no new force or new factor arises or has at any time arisen in the universe; nor has any such force or factor been introduced into the universe as it were from the outside. Hence "every new phenomenon is the equivalent of a new arrangement of existing forces." . . .
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> Thirdly, while Materialism insists that whatever exists in the universe is ultimately of the same sort as matter, it is committed to no particular theory of matter. The conception of matter current at different periods vary with and depend upon the state of physical science at different periods. Now whatever the ultimate analysis of matter may be – whether matter is fundamentally wave-like or projectile-like, whether it consists of charges of positive and negative electricity, of homogeneous atomic units, of electronic "mushes," or of "humps in space-time" – this too must, if Materialism is maintained, be accepted as the ultimate analysis of whatever exists [JOAD: 496-497].

To this I would add that the scientist regards the world (or, if one prefers, ‘the universe’ or ‘the universes’) as something whose character and properties are utterly indifferent to whatever the individual human being thinks they ought to be like, and that its nature would still *fundamentally* be ‘the way that it is’ if humankind had never existed (hence the scientist’s distrust of idealism). Dialectical quibbling aside, Joad’s description of materialism as quoted above is by and large an accurate description of materialism in science.

Scientific materialism is an ontology-based attitude. Although ‘what matter is really like’ is a question for which most scientists are willing to accept changing answers as scientific paradigms change, the ontological presupposition of ‘matter-based nature’ (or, if one prefers, ‘matter/energy based nature’) is constant in science and has been since the days of Boyle’s ‘corpuscles.’ Only the professional mathematician can legitimately make the denial, “I do not presuppose this,” and this denial on his part is rooted in *professional indifference*. As far as presuppositions are concerned, the physicists could change their views of ‘how the universe is made’ every alternate Tuesday yet the mathematician can rest assured that all these shifting paradigms will still rely upon mathematics. The physical scientist’s indifference to ‘philosophical questions’ concerning ontology stems from the rather pragmatic attitude that ‘stuff is still stuff no matter what.’ Fifty or a hundred or a thousand years from now human understanding of ‘what stuff is’ could be very different from how we understand ‘stuff’ today, but for all that it will still be ‘stuff.’ *That* is the ontological presupposition of scientific materialism. It is a tight little tautology, logically unassailable.

Even so, this does not mean that scientific indifference to metaphysics is not short-sighted. Science is in the business of comprehending ‘nature’ and therefore does have need to call upon whatever the prevailing ontology (more accurately, pseudo-ontology) of the day might be for the formulation of scientific theories. Theories are intended to describe and explain ‘the stuff of nature,’ and this cannot be accomplished if the ‘stuff’ is not modeled. It is not inaccurate to call a theory ‘knowledge compilation,’ and it is here where epistemology enters the picture. Scientists rarely talk about epistemology; if anything, they are more pragmatically indifferent to issues of epistemology than they are to ontology. ‘Standard scientific practices’ are presupposed to provide all that is needed for ‘the pursuit of scientific truth’ and, again, the attitude is at root pragmatic. If current scientific practices do not obviously result in our being seriously misled then they are *trusted*. ‘Philosophical’ issues of epistemology slumber during the long intervals when the practice of science piles up one victory after another. Only the gravest of circumstances, such as the stunning experimental refutation of classical physics at the beginning of the twentieth century, re-awaken interest in issues of knowledge-theory. When the crisis passes epistemology goes to sleep again.

I will not call the attitude arrogant, but I will say the practice ignores the lessons of history. I suspect no scientist likes to think that someday a paradigm shift will relegate his life's work to at best some dusty footnote in a history book, or, worse, to the fate of being utterly forgotten, or, worst of all, to remembrance only in the pages of a history lesson depicting how foolishly naive scientists of earlier times were. But these are the fates of all but a very few of all the scientists who have ever lived. Whatever modest comfort one might take from thinking one has at least placed a brick in the wall of human knowledge, and however selfless and noble this may feel, it comes to naught if the wall is torn down in a later epoch. Aside from a few scattered historians, how many today know or care who wrote the most brilliant and insightful foundational work on caloric or what it said? How many young scientists even know what caloric was?

Metaphysical ideas and prejudices affect how scientific theories take shape. Scientists regarded phlogiston and caloric as being just as established in the 18th century as we regard quarks in the 21st. Indifference to questions of metaphysics *makes a difference* in science. Hardly anyone would argue it is not important for us to understand what we are talking about. But indifference to metaphysics goes against this precept.

> I endeavor as much as I can to deliver myself from those fallacies which we are apt to put upon ourselves by taking words for things. It helps not our ignorance to feign a knowledge where we have none, by making a noise with sounds, without clear and distinct significations. Names made at pleasure neither alter the nature of things, nor make us understand them, but as they are signs of and stand for determined ideas. . .
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> They who first ran into the notion of *accidents*, as a sort of real beings that needed something to inhere in, were forced to find out the word *substance* to support them. Had the poor Indian philosopher (who imagined that the earth also wanted something to bear it up) but thought of this word substance, he needed not to have been at the trouble to find an elephant to support it, and a tortoise to support his elephant; the word substance would have done it effectually. And he that inquired might have taken it for as good an answer from an Indian philosopher, – that substance, without knowing what it is, is that which supports the earth, as we take it for a sufficient answer and good doctrine from our European philosophers, – that substance, without knowing what it is, is that which supports accidents. So that of substance, we have no idea of what it is, but only a confused, obscure one of what it does [LOCK: BK II, Ch. XIII, §18-19].

Ideas and notions taken as 'primitives' in science affect theory because theories must always be ultimately expressed in terms of them. But 'primitives' always belong to a metaphysic, whether this metaphysic is systematic or merely a pseudo-metaphysic of the accidents of one's early life experience. At root they are *practical*, which is to say we understand primitives for 'what they do' rather than 'for what they are' (as things). Locke went on to say,

> So that if any one will examine himself concerning his notion of pure substance in general, he will find he has no other idea of it at all, but only a supposition of he knows not what *support* of such qualities which are capable of producing simple ideas in us; which qualities are commonly called

> accidents. If any one should be asked, what is the subject wherein color or weight inheres, he would have nothing to say but the solid extended parts; and if he were demanded, what is it that solidity and extension adhere in, he would not be in a much better case than the Indian before mentioned . . . And thus here, as in all other cases where we use words without having clear and distinct ideas, we talk like children: who, being questioned what such a thing is, which they know not, readily give this satisfactory answer, that it is *something*: which in truth signifies no more, when so used, either by children or men, but that they know not what; and that the thing they pretend to know, and talk of, is what they have no distinct idea of at all, and so are perfectly ignorant of it, and in the dark. The idea then we have, to which we give the *general* name substance, being nothing but the supposed, but unknown, support of those qualities we find existing, which we imagine cannot subsist *sine re substante*, without something to support them, we call that support *substantia*; which, according to the true import of the word, is, in plain English, standing under or upholding. . .
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> Hence, when we talk or think of any particular sort of corporeal substances, as horse, stone, &c., though the idea we have of either of them be but the complication or collection of those several simple ideas of sensible qualities, which we used to find united in the thing called horse or stone; yet *because we cannot conceive how they should subsist alone, nor one in another*, we suppose them existing in and supported by some common subject; which support we denote by the name substance, though it be certain we have no clear or distinct idea of that thing we suppose a support [LOCK BK II, Ch. XXIII, §1-4].

Even the staunchest materialist, if he be not a committed skeptic, should find it difficult to disagree with Locke's analysis of 'substance' if we should give priority in our prejudices over to ontology. Were it not for our direct, sensible experience of 'accidents' science would indeed have nothing to talk about. An ontology-first prejudice merely places 'substance' as the problematical end-of-the-line in a chain of reasoned concepts.

One might argue that 17th century philosophy – even if it is empiricism – has little to offer us in the 21st century. But this line of argument is blind to the philosophical crisis the birth of the quantum theory posed in the 20th century. In 1950 physicist Henry Margenau wrote,

> In the center of our discussion stands the query as to what is immediately given in experience.
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> There is an important need for returning to such questions despite their unpopularity at the present time. Twenty years ago the physicist was disposed to consider them academic and useless, as inviting idle speculations among philosophers. Meanwhile, however, he himself has disinterred the bones of old disputes; his quantum theories have raised again the very issues he thought academic. Quantum theory is meaningless without a clear understanding of what, precisely, is immediately given. For if the physical investigator were undeniably *given* such facts as the position and velocity of particles – to cite a famous example – how can the uncertainty principle deny the observability under any circumstances? If time is given immediately in sensation, how can the physicist make theories that fashion time after abstract mathematical patterns? Modern natural science presents many such challenges to unsettle the complacency of those who thought they had been emancipated from the debates of "school philosophy."
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> In the vein of these convictions we have examined the mechanistic view of nature, in which the observer or possibly his mind is exposed to the spectacle of external events. It is found that the spectator-spectacle relation is difficult to maintain in the face of the newer knowledge of science, primarily because the knowing subject intrudes itself unpreventably into the objective scheme of things. The theory of auxiliary concepts . . . will not exonerate the spurious spectator-spectacle distinction, and it becomes apparent that a new start must be made. An analysis of *all* experience is suggested as the correct point of departure. The simplest type of experience, *i.e.*, immediate experience, or sense data, is then superficially examined, and it is shown . . . how it functions as a terminus for cognition [MARG: 51-52].

Although not a Kantian – indeed, Margenau quite badly misunderstood Kant's philosophy – he nonetheless came to the view that an epistemology-centered metaphysic had been made necessary by the startling discoveries of the twentieth century. Margenau's analysis led him to a metaphysic we can justly call a *theory of constructs*. Margenau uses the word "Nature" to mean "the immediately given" (a term that refers to direct sensible effects, i.e., "data of the senses"). Scientific knowledge, he tells us, is knowledge of constructs – concepts the scientist develops that serve to unify objective experience. As we will see, Margenau comes within a stone's throw of the road leading to the Critical theory.

Margenau speaks of the "passage from data to orderly knowledge" by means of "rules of correspondence." A couple examples will serve to illustrate his thesis.

> The simplest application of the rules has been discussed; it is the act of postulating a *thing* in the face of certain *sensory evidence*. This was called reification. That this thing be an external object cannot be certified by the "rules" alone but requires documentation of another sort, documentation which refers to the coherence of our entire experience. We must thus distinguish between a rule that reifies and the larger part of experience that objectifies. . .
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> Another passage from Nature to concepts, which is a little less direct than the foregoing, occurs whenever the postulated thing is endowed with specific qualities of its own, qualities not "read from data." As a case in point we mention the assignment of *mass* to bodies, the act which set the science of mechanics going. . . Mass, though not part of Nature, has some intuitable aspects; but it lies somewhat farther from Nature than does the apple [MARG: 64-65].

He follows these examples with several others, leading up to one of his central ideas, namely the *construct*.

> The examples of the foregoing section were selected with a purpose in mind. They illustrate in their chosen order a progressive widening of the gap which is spanned by the rules of correspondence. In reification we take but a small step toward concepts, in assigning mass we move a greater distance, until finally, in defining a state function[3], we make a flight of considerable magnitude into the very abstract. We have spoken of *one* rule in each of these instances, but this should not prejudice us against admitting the resolvability of any rule into an arbitrary number of simpler rules. In theoretical formalisms, including logic, it is dangerous to count entities for there is rarely a way of determining what entities are basic. Ideas, concepts, relations do not satisfy the axioms of arithmetic. . .
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> A rule of correspondence links what has here been called Nature to entities which we have vaguely termed concepts, ideas, reflective elements, and so forth. For these, too, we should now introduce a special name. A particular tree or a particular electron is hardly a concept because of the generic implications of that word. And yet our rules lead to particular trees and particular electrons. Nor can these entities be termed ideas unless one wishes to open the floodgates to misunderstanding. On the other hand they do partake of the character of concepts and ideas by being rational, by submitting themselves to logical procedures in a much fuller measure than do the data of Nature.
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> To indicate that they are not mere gleanings from the field of sensory perception, that they come into their own through what are felt to be creative processes in our experience rather than through

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[3] The term 'state function' refers to what is otherwise known in physics as 'the wave function' of quantum mechanics.

> passive contemplation; to emphasize their rational pliability and yet to distinguish them from shadowy concepts, the writer has previously called them *constructs*. . .
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> A construct is not found ready-made. It has many of the qualities of an invention. Here perhaps even the sympathetic reader will shake his head, for it is obvious that trees, mass, electrons are more than inventions and hence more than constructs. But we do not deny they are more than mere constructs. They are *useful*, or, to be much more precise, they are *valid* constructs, valid in a sense to be more precisely defined later. . . It is the principal business of a methodology of science, and indeed of all epistemology, to investigate what requirements a construct must satisfy to be admitted as valid, or, as we shall later say, to become a *verifact* [MARG: 69-70].

What Margenau termed a 'construct' is for all intents and purposes the same as what Kant called an Object. It would not be inaccurate to say that Margenau's thesis focused attention on the outcomes of sensibility, thinking, judgmentation, and reasoning while Kant focused on the processes and methods by which the Organized Being *produces* these outcomes. The works of Margenau and Kant make a complementary pair; understanding the one helps in comprehension of the other, and vice versa.

Margenau supplied us with a diagram illustrating the structure of his construct system. We will shortly see that a counterpart illustration can be drawn for Kant's system. Margenau's construct structure is depicted in Figure 24.2.1. C, C', and C" denote constructs. The thick line is

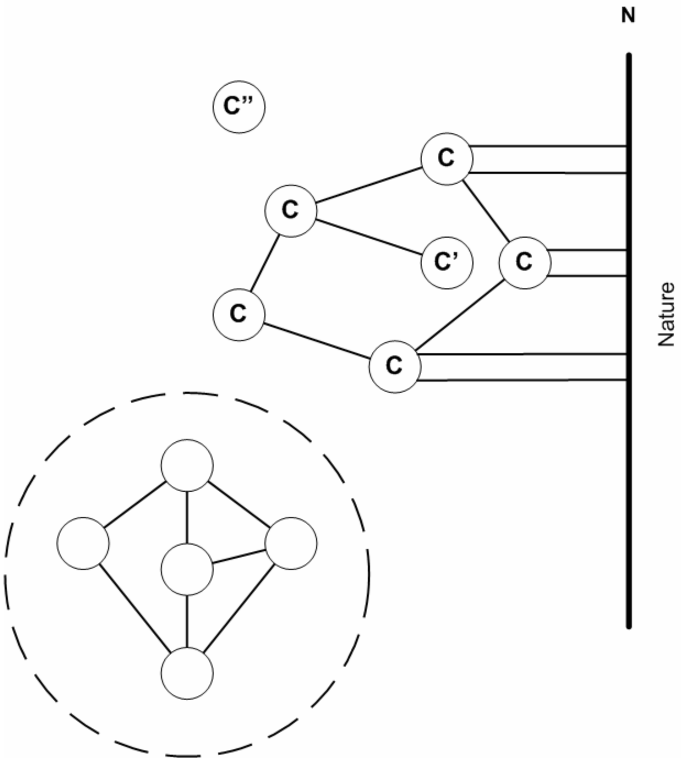

Figure 24.2.1: Margenau's construct diagram. Nature (the 'immediately given') is represented by the heavy black line. Formal connections are represented by single lines; double lines are 'epistemic connections'. Constructs are denoted by C, C', and C". The figure is adapted from [MARG: 85].

meant to represent the ‘plane of the immediately given’ (Margenau’s ‘Nature’). The thinner single lines are formal ‘connections’ (relationships) among the constructs. The double lines represent ‘epistemic connections’ – Margenau’s term for a rule of correspondence that links the construction to the data of the senses. Margenau explains this illustration as follows:

> Those [constructs] without primes stretch out two or more arms, either toward other constructs or toward Nature; they may be described as *multiply connected*. Every construct used in natural science will be seen to be of this type, permitting passage elsewhere in at least two ways. For example, from the idea of an electron one can pass to its mass, its charge, its field. From its electric field one can go in one direction to the idea of potential, in other directions to certain observable aspects of experience (along double lines). . .
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> But we also note in the figure a certain construct, labeled C’, which is peninsular, possessing only one connection with a normal construct. It hangs loosely within the system and obtains what meaning it has only from a coherent set of others. An example of a peninsular construct is the color of an electron[4]. No harm is done if color is assigned to it, but there is no way of substantiating this attribute, for it leads to no other significant knowledge by any formal route, nor does it allow verification by any possible rule of correspondence. The only egress is along the line which constituted it originally, that is, by repeating the categorical statement: The electron has color.
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> The construct C” has no connection whatever with others, nor with Nature; it is insular. Its insertion in a theoretical system makes no difference whatever. An example of an insular construct is the God of deism, which has no place in science. There can also be a group of isolated constructs, mutually connected but without epistemic connections, such as those surrounded by the dotted circle. They may be said to form an island universe, consistent in itself though unverifiable. Science sometimes generates such tantalizing curiosities, then looks for possible rules of correspondence that might give them significance. But they are dropped again unless such rules are found. . .
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> The metaphysical requirement here under examination may now be stated briefly: Constructs admissible in science must be multiply connected; they may not be insular or peninsular; sets forming an island universe must be excluded.
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> This axiom is not meant to settle all possible future contingencies. It should be admitted that situations can arise which render the established methodology of science powerless, where new and more precise directives are required. Science then has to feel its way and modify its metaphysics while proceeding. Indeed this question may have occurred to the reader: What if a *single line* could be drawn between one of the normal Cs in the figure and one of the constructs of the island? Frankly, the state of affairs would then be embarrassing, not only to the philosopher of science who attempts to formulate its metaphysics, but also to science itself. We happen to be confronted with just that situation in physics today.
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> According to Dirac’s theory, an electron can be in states of positive and in states of negative kinetic energy. The latter states have never been observed. At first they were thought to form an island universe and were forthwith dropped from consideration. But alas, the same theory showed that an electron, residing inoffensively in a state of positive energy, can pass without notice into one of the objectionable states! The line in question had been drawn. Much confusion in scientific quarters was the result; even now physicists are withholding final judgment regarding the “reality” of negative-energy electrons in abeyance, hoping for further clues. Very few regard the state of the theory as satisfactory [MARG: 85-88].

The “line that had been drawn” mentioned in the last paragraph above was the discovery by Carl Anderson in 1932 of the positron (the antiparticle of the electron), an accomplishment for which he was awarded the Nobel Prize in physics in 1936. The “unsatisfactory state of the theory” to

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[4] Margenau means ‘color’ in the ordinary every-day sense of that word. Quantum chromodynamics, which defined a property of quarks called ‘color,’ had not yet been developed when Margenau wrote this.

which Margenau refers was resolved later by Feynman, Schwinger, and Tomonaga through their development of quantum electrodynamics. Rather than implicating the ‘sea of negative energy’ envisioned by Dirac’s theory, which gave rise to some fundamental paradoxes confronting physics, QED holds that the positron is an electron ‘moving backwards through time.’ This is a nice illustration of what Margenau meant by science having to ‘modify its metaphysics while proceeding.’

But it is precisely this ‘need to modify the metaphysic’ one would prefer to avoid in order that science might actually be able to carry on ‘laying down knowledge brick by brick’ as the popular myth likes to describe scientific advances. The case of the positron illustrates very well the type of ‘modification’ that has taken place many times in the history of science. In this particular case what had to be modified was physics’ *ontological construct of objective time*. Rather than a single ‘current of time’, physics now had two. This is a model that was not foreseen by Einstein’s ‘operational’ definition of time, but it is also a model not actually *forbidden* by Einstein’s theory. Indeed, one could say that Einstein’s theory ‘caused’ this model in the sense that Dirac’s ‘sea of negative energy’ was a consequence of his unification of quantum mechanics and the special theory of relativity. QED unites these theories in a different way, and in doing so cleared away the problems inherent in Dirac’s theory.

§ 3. The Verification Problem

Margenau’s “multiply connected construct axiom” is a necessary condition for a construct to stand as a “verifact.” He coined the latter term to mean “valid construct” and explained it as a construct from which *predictions* are made *and verified*. “Prediction” in this usage means the theory implicated by the construct requires the actuality of some effect to be sought “in the plane of the immediately given” (Margenau’s ‘Nature’). “Verified” meant the looked-for correspondence in ‘Nature’ is found.

> If the challenge is met, the theory is said to be confirmed or verified in this instance. And the theory is *valid* if it is confirmed in a sufficient number of instances. Furthermore, the constructs which form organic parts of a valid theory will themselves be called valid constructs or verifacts. *Processes of validation, when conjoined with the metaphysical requirements discussed in the previous chapter, create scientific knowledge. It is this purgatory of validation which removes from constructs the figmentary stigma which their epistemological genesis first attaches to them* [MARG: 105].

A single confirming fact of ‘Nature’ all by itself is not, according to Margenau, a sufficient amount of evidence to declare a theory and its constructs ‘valid’. Coincidence is, after all, no stranger to experience. For an item of knowledge to earn the title *scientific* knowledge the predictable consequences of a construct must be manifold in Margenau's 'Nature' and a construct must lead to 'Nature' through multiple distinct routes. As an illustration let us consider construct C' ("the color of an electron") in figure 24.2.1. Although this construct can trace multiple paths to the plane of the immediately given, C' itself has only a single 'peninsular' path to the construct C to which it is immediately connected. The multiplicity of 'points' on N confirm this C construct but so far as C' is concerned these are not independent verifications. In Margenau's terminology the C construct is a verifact but C' is not. This is Margenau's prescription for uniting "rationalism" (concepts of constructs) with empiricism (confirmation at the plane of 'Nature').

> The realistic reader, who suspected from the beginning that we were telling a rather subjective story of the affairs of science, may now find his suspicions confirmed. He will perhaps take issue with the basic premise which marks the concepts of science as something close to inventions. In fact the position outlined in this book thus far can, without much shift in emphasis, be interpreted as idealistic. If the certainties of scientific experience are mere *constructs*, the critic will ask, where does science get the stability which it obviously possesses? Why does it lay claim to possessing *facts* in a more solid sense than other disciplines? Without an anchor to "reality," he will conclude, our epistemology, itself amorphous, floats within experience like jelly in an ocean.
>
> To answer him, we would first object to the seemingly innocent bias introduced into the argument by the simple word "mere": generically the elements of scientific theories are undeniably constructs. But they are not *mere* constructs, as idle inventions would be. They do not owe their existence to accidents or caprice but stand in uniform correlation with immediate experience; and after their birth they are subjected to a most rigorous regime of methodological principles. These constraints alone eliminate any mereness from the nature of scientific constructs. Their scientific validity, their complete trustworthiness, however, is conferred upon them by further, even more limiting and more exacting procedures: by a continual test against immediate experience, called *confirmation* [MARG: 98-99].

This is why valid constructs are required to minimally have more than one connection to other constructs and to connect, either immediately or mediately, to the plane of 'Nature'.

Even so, Margenau's theory does not escape the charge that constructs of science are contingent rather than necessary. He commits enthusiasm when he refers to "the certainties of scientific experience" and the "complete trustworthiness" of their scientific validity. We can perhaps generously overlook his comment about "the certainties" because we have already seen that "certainty" has a degree and Margenau did not say "absolute certainties." But "complete trustworthiness" is another matter because "complete" implicates "perfect." The degree of holding-to-be-true accorded a construct may be great, but it is never absolute. Indeed, the mandate of an on-going "continual test against immediate experience" tells us that science does keep one eye open to spot inconsistencies and "problems" that might attach to a theory. History tells us this vigilance is warranted. It seems not unlikely that Margenau might have had the contingency of constructs in mind when he commented earlier that, "This axiom is not meant to settle all possible future contingencies." Confirmation is required in science,

> But this leaves us with two important questions: (1) When is the number of instances of confirmation sufficient? (2) What constitutes agreement between theory and observation? The second question is particularly troublesome because, as has been pointed out, theoretical prediction is always definite, whereas the immediately given is of necessity surrounded by a haze of uncertainty. To answer it requires the exposition of slightly technical considerations . . . Question 1 can be treated at once.
>
> Though it may seem strange to the logician, scientists are none too meticulous in their demands for sufficiency in the number of validating instances. They have never evolved a pretentious formalism for deciding when they may stop testing a theory. A few crucial confirmations often satisfy their quest for certainty, perhaps to the consternation of staunch empiricists. But the reason for their modesty should now be apparent: It is to be found in the orderly relations which exist among constructs before they are put to test. Reliance upon the logical coherence of his conceptions exempts the scientist from the need of exhaustive verification. There is an important sense in which a theory is more than the class of sentences it can generate, and the awareness of this transcendency inclines scientists to unhesitating acceptance of a theory after it has been subjected to a number of tests which are wholly inadequate statistically. This pervading rational nexus also allows them to decide when experiments are crucial [MARG: 106-107].

I agree that a formula (dogma) for 'deciding when one may stop testing a theory' would be pretentious. But should the attitude of science be called 'modest'? Or is it pretentious? Here I must regretfully note we can spot the difference between a professional philosopher and a very gifted amateur. At issue is not Margenau's all-too-accurate description of how scientists behave in terms of theory-acceptance. The issue is Margenau's more or less obvious endorsement of it. The attentive reader will note that the quote above does not go to Margenau's construct structure theory; instead it goes to the inclinations of scientists in choosing the point at which they will accept a theory and determine to defend it against later challenges. This is a subjective judgment call and speaks to *persuasion* rather than knowledge. Margenau was a capable physicist and a member-in-good-standing of the culture of science. But one of my younger colleagues over in our philosophy department, who is a warm-hearted and kindly person, would tear to shreds Margenau's 'reliance' argument without much effort and with clinical dispassion. Because Dr. Margenau is no longer here to defend his position in a debate with my colleague, I will not unleash a polemic upon Margenau's analysis in this treatise. But the issue is nonetheless on the table and we must address it.

The reliance argument, while a reasonably accurate description of the psychology of scientists, is nonetheless an excuse, not a justification. It is right at this point where Margenau's epistemological analysis stops short of sufficiency. We will discuss the Critical resolution of this problem in the following section, but here it is appropriate to review the contemporary thinking that has gone into the verification issue. For this review we will begin with Lakatos.

If we were to think, as Margenau's remarks above might seem to imply, that all scientists spoke with one voice in deciding when a theory is ‘verified’ or ‘falsified’ we would be naive. There were and are a number of conflicting views on this issue. Lakatos reviewed and criticized the most common set of these in [LAKA2] with his usual fiery and polemical style. In our review we will overlook the slashing cuts he made at various individuals and groups and focus instead on the key points in his arguments. He begins by provoking us with this question: Is science reason or religion?

> For centuries knowledge meant proven knowledge – proven either by the power of the intellect or by the evidence of the senses. Wisdom and intellectual integrity demanded that one must desist from unproven utterances and minimize, even in thought, the gap between speculation and established knowledge. The proving power of the intellect or the senses was questioned by the skeptics more than two thousand years ago; but they were browbeaten into confusion by the glory of Newtonian physics. Einstein’s results again turned the tables and now very few philosophers or scientists still think that scientific knowledge is, or can be, proven knowledge. But few realize that with this the whole classical structure of intellectual values falls in ruins and has to be replaced: one cannot simply water down the ideal of proven truth – as some logical empiricists do – to the ideal of ‘probable truth’ or – as some sociologists of knowledge do – to ‘truth by consensus’.
>
> Popper’s distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation. In his view virtue lies not in caution in avoiding errors, but in ruthlessness in eliminating them. Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper’s recipe. Intellectual honesty does not consist in trying to entrench or establish one’s position by proving (or ‘probabilifying’) it – intellectual honesty consists rather in specifying precisely the conditions under which one is willing to give up one’s position. . . *Belief* may be a regrettably unavoidable biological weakness to be kept under the control of criticism: but *commitment* is for Popper an outright crime [LAKA2: 8-9].

It is perhaps clear from this opening remark that Lakatos is well on the road to skepticism. His own position, which we will come to soon, is a curious ‘new breed’ of skepticism[5] which, however unlovely it may appear when it is brought into the light, is for all that not so far from the way that 21st century science at least appears to behave when it comes to theory-commitment. Lakatos buys into the “unprovability” argument and turns instead to ‘refutability’ as his central focus.

Lakatos’ basic pessimism likely stems from the Hegel-like attitude that comes through in the remarks above concerning “the ideal of proven truth.” Such an ideal is absolute Truth in a very Hegelian sense. For Lakatos “to prove” is “to establish truth” and this is precisely what he and Popper think cannot actually be accomplished. Taking Newtonian mechanics as an example, there is certainly “much truth in it” (else it could not be said to be “the best-corroborated scientific theory of all time”) but it is not Absolutely True (and, hence, “collapsed”). This may seem an odd

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[5] I am reasonably sure Lakatos would have strongly objected to my characterization of his position as one of skepticism. On the other hand, most supporters of the positions he attacks would call his presentation of their positions one-sided caricatures and say he sets up a straw man to knock down. Lakatos calls himself a ‘demarcationist.’

way to characterize the branch of science that was used to design the car you drive, the bridges you drive over, the power system that lights your house, etc. I will go farther and dare to say *it is* an odd way to characterize it. We have previously discussed and disposed of the myth of Hegel's absolute Truth and I need not repeat the Critical definition of truth yet again. That the truth of an idea has limitations on its scope of application is well recognized in science. Wigner said,

> Physics does not endeavor to explain nature. In fact, the great success of physics is due to a restrictions of its objectives; it endeavors to explain the regularities in the behavior of objects. This renunciation of the broader aim, and the specification of the domain for which an explanation can be sought, now appears to us an obvious necessity. In fact, the specification of the explainable may have been the greatest discovery of physics so far.
>
> The regularities in the phenomena which physical science endeavors to uncover are called the laws of nature. The name is actually very appropriate. Just as legal laws regulate actions and behavior under certain conditions but do not try to regulate all actions and behavior, the laws of physics also determine the behavior of objects of interest only under certain well-defined conditions but leave much freedom otherwise.[6]

One may use Newtonian physics confidently so long as the situation to which it is applied does not involve velocities very close to the speed of light, objects of atomic dimensions, or extraordinarily high levels of gravity. Astrophysicists also tell us that we can use it for phenomena of astronomical dimension, although this claim seems more problematical given developments in astronomy that have come to light over the past few years.[7]

But how does one define the "certain well-defined conditions" under which laws of physics "determine the behavior of objects"? If we say that these conditions are only those under which the laws have been tested and verified then the science loses its ability to predict and, with this, loses much of its power and fecundity. In effect we would again be ancient Egyptians, who had much practical know-how but no encompassing theory to tie together their divers crafts. Instead science takes the attitude that a theory shown to be true (in the Critical sense of that word) over some scope of conditions is to be presumed to be true under *all* conditions until and unless a situation arises where the theory 'unquestionably' breaks down. But what does or does not constitute such a 'breakdown'? This is the crucial and central question. Lakatos claims that this attitude introduces what he calls a "*ceteris paribus* clause" into science, and he uses this to argue that no theory is provable. Consequently, he tells us, *demarcation* becomes the central problem

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[6] Eugene P. Wigner, Nobel Prize Acceptance Speech, Dec. 10, 1963. Reprinted in *Science*, **145**, No. 3636, 995 (1964).

[7] Some astronomical observations in recent years have been at variance with Newtonian predictions. These are the empirical factors giving rise to the speculation of the existence of 'dark matter.' Introducing 'dark matter' makes Newtonian dynamics 'work' once again on the astronomical scale. However, not all physicists are convinced that the observations establish the existence of dark matter. For an accessible discussion of this see Mordehai Milgrom, "Does dark matter really exist?" *Scientific American*, vol. 287, no. 2, August 2002, pp. 42-52.

for a philosophy of science. He calls his prescription for demarcation “methodological falsificationism.”

> The methodological falsificationist realizes that in the experimental techniques of the scientist fallible theories are involved, in the ‘light’ of which he interprets the facts. In spite of this he applies these theories, he regards them in the given context not as theories under test but as *unproblematic background knowledge*, which we accept (tentatively) as unproblematic while we are testing the theory. He may call these theories – and the statements whose truth-value he decides in their light – ‘observational’: but this is only a manner of speech which he inherited from naturalistic falsificationism. The methodological falsificationist *uses our most successful theories as extensions of our senses* and widens the range of theories which can be applied in testing far beyond the dogmatic falsificationist’s range of strictly observational theories. . .
>
> This consideration shows the conventional element in granting – in a given context – (methodologically) ‘observational’ status to a theory. Similarly, there is a considerable conventional element in the decision concerning the actual truth-value of a basic statement which we take after we have decided which ‘observational theory’ to apply. One single observation may be the stray result of some trivial error: in order to reduce such risks, methodological falsificationists prescribe some safety control. The simplest such control is to repeat the experiment . . . thus fortifying the potential falsifier by a ‘well-corroborated falsifying hypothesis’.
>
> The methodological falsificationist also points out that, as a matter of fact, these conventions are institutionalized and endorsed by the scientific community; the list of ‘accepted’ falsifiers is provided by the verdict of the experimental scientists.
>
> This is how the methodological falsificationist establishes his ‘empirical basis’. . . This ‘basis’ can hardly be called a ‘basis’ by justificationist standards: there is nothing proven about it – it denotes ‘piles driven into a swamp’. Indeed, if this ‘empirical basis’ clashes with a theory, the theory may be *called* ‘falsified’, but it is not falsified in the sense that it is disproved. Methodological ‘falsification’ is very different from dogmatic falsification. . .
>
> *The methodological falsificationist separates rejection and disproof*, which the dogmatic falsificationist had conflated. He is a fallibilist but his fallibilism does not weaken his critical stance: he turns fallible propositions into a ‘basis’ for a hard-line policy. On these grounds he proposes a *new demarcation criterion*: only those theories . . . which forbid certain ‘observable’ states of affairs, and therefore may be falsified and rejected, are ‘scientific’: or, briefly, *a theory is scientific* (*or ‘acceptable’*) *if it has an ‘empirical basis*’. [LAKA2: 23-25].

If the reader feels that this ‘new demarcation criterion’ and the ‘methodological falsification’ program advanced by Lakatos seems nonetheless to contain a certain amount of subjectivity in the decision-making process . . . well, so do I. So, it would also appear, does Lakatos.

> The term ‘demarcationism’ stems from the problem of demarcating science from non-science or from pseudoscience. But I use it in a more general sense. A (generalized) demarcation criterion, a methodology or appraisal criterion, demarcates better from worse knowledge, defines progress and degeneration. . .
>
> According to demarcationists, the products of knowledge can be appraised and compared on the basis of certain *universal* criteria. Theories about these criteria constitute ‘methodological’ knowledge . . .
>
> There are many differences *within* the demarcation school. These stem from two basic differences. First, different demarcationists may differ in their claims about what the most appropriate *unit* of appraisal is. . . Secondly, demarcationists may agree on the unit of appraisal but still differ over the criterion of appraisal. . .
>
> What advice do demarcationists give to the scientists? Inductivists forbid them to speculate; probabilists to utter a hypothesis without specifying the probability lent to them by the available evidence; for falsificationists *scientific* honesty forbids one *either* to speculate without specifying potentially refuting evidence *or* to neglect the results of severe tests. My methodology of scientific

> research programs does not have any such stern code: it *allows people to do their own thing but only so long as they publicly admit what the score is between them and their rivals* [LAKA3: 108-110].

In practice individual scientists use some one or another mix of the various "demarcations" coming out of Lakatos' various descriptions. There is no codified "rules of demarcation" set down by the scientific community as a whole, no document preserved in some vault in Paris or elsewhere. Pragmatically, the "code of demarcation" at any given moment lies in the hands of journal editors and peer reviewers. Anyone with more than a little experience with writing and publishing scientific papers or submitting funding proposals has likely experienced the shifting sands of "scientific appraisal" and "demarcation criteria." In that fiery style of his we have seen exhibited already, Lakatos calls this practice (perhaps better put, "practices") *élitism*.

> Among scientists the most influential tradition in the approach to scientific theories is élitism. Unlike the skeptics – but like the demarcationists – élitists claim that good science *can* be distinguished from bad or pseudoscience, better science from worse science. . . They claim, however, that there is, and there can be, no statute law to serve as an explicit, universal criterion (or finite set of norms) for progress or degeneration. In their view, science can only be judged by case law, and the only judges are the scientists themselves. . .
>
> According to the demarcationist one theory is better than another if it satisfies certain objective criteria. According to the élitist one theory is better than another if the scientific élitist prefers it. But then it is vital to know *who* belongs to the scientific élite. While élitists claim that no universal criteria for appraising scientific *achievements* are possible they may admit the possibility of universal criteria for deciding whether *persons or communities* belong to the élite [LAKA3: 112-113].

While Lakatos' polemics do not win him many friends, there is nonetheless a germ of truth in his characterization just quoted. The United States government does maintain a list of "top scientists" to advise it; the U.S. National Academy of Science and National Academy of Engineering do select their own memberships. Scientists *do* maintain that science is 'objective' but, when all is said and done, *individuals* make their own decisions on what is 'objective' and what is not. To win its way into a journal paper or a textbook a theory must gain *acceptance* and when all is said and done "acceptance" is a choice and choices are inherently based on subjective (affective) factors, regardless of any protest raised up by ontology-centered pseudo-metaphysics. So far as 'objectivity' is concerned Lakatos' program may be no better founded than what we find in normal scientific practice, but at least his stinging remarks do serve to point out the one crucial fact: We are at present adrift when it comes to ascertaining *objective validity* in appraising scientific theories or determining when a theory is 'verified' vs. 'falsified.'

§ 4. *Noumena* and the Horizon of Experience

Can something be done to improve on this state of scientific affairs? I think it is safe to say that many scientists would rebut Lakatos' assertion by saying 'demarcationists' are as much lacking in possession of universal *objective* criteria for solving the verification problem than he claims they are. Many, including your author, do not find Lakatos' claims credible in this regard. But is it true, as Popper and Lakatos held and as actual scientific practice appears to confirm, that no universal objective criteria are to be had? If one adopts an ontology-centered metaphysic (as Lakatos does, and as most people – not only scientists – fall back upon as their 'default') then skepticism – even skepticism calling itself 'demarcationism' – is the end product. If nothing else, the entire history of ontology-centered philosophy bears this up time and time again.

What, then, is the Critical answer to this issue? Does it have one? Many people having only a passing familiarity with Kant's theory presume its doctrine holding that there are *limits* to human knowledge means there is no *real* knowledge of nature. Lakatos, who was very ignorant of the Critical Philosophy, apparently thought so:

> The idea that we live and die in the prison of our 'conceptual framework' was developed primarily by Kant: pessimistic Kantians thought that the real world is forever unknowable because of this prison, while optimistic Kantians thought that God created our conceptual framework to fit the world [LAKA2: 20].

I would very much like to ask Lakatos to explain to me what exactly he means by "the real world" and how exactly his definition of it is anything other than an ontological presupposition, but, sadly, it is no longer possible to do this. One need not travel far along the highways of philosophy to find that ontology-centered philosophers do not or cannot explain this term. In the *Oxford Dictionary of Philosophy* we find:

> **real** The term is most straightforwardly used when qualifying another adjective: a real *x* may be contrasted with a fake *x*, a failed *x*, a near *x*, and so on. To treat something as real, without qualification, is to suppose it to be part of the actual world. To reify something is to suppose that we are committed by some doctrine to treating it as a thing. The central error in thinking of reality and existence is to think of the unreal as a separate domain of things, perhaps unfairly deprived of the benefits of existence.
>
> **reality** That which there is. The question of how much of it there is forms the dispute between realists and anti-realists. Does it include: numbers, possibilities, the future, the past, other minds, colors, tastes, the external world, mind as well as matter, or matter as well as experience?

Is it any wonder why most scientists have such a low patience threshold with philosophers? According to the *Oxford* "real" seems at least to mean "to exist" and at most to be "regarded as a thing." Things apparently are something we "treat as real." To "treat as real" is to "suppose it to be part of the actual world." Okay; then I ask: Does "actual world" mean the same thing as "real world"? If not, what is the difference? If so, then what about

> **actual** in modal logic the actual world is the world as it is, contrasted with other possible worlds, representing ways it might have been. The central problem is to understand how the actual state of the world is to be characterized, except in terms that themselves make reference to alternative possibilities.[8]

I can't speak for you, but to me this doesn't make much sense and sounds circular. The real world is the actual world and the actual world is (merely logically) the world as it is? Being, existence, and reality: the three troublesome words that entangle ontology-centered metaphysics in a fishing net tied inside a gunny sack. According to the *Oxford* even "meaning" is ontological:

> **meaning** Whatever it is that makes what would otherwise be mere sounds and inscriptions into instruments of communication and understanding. The philosophical problem is to demystify this power, and to relate it to what we know of ourselves and the world. Contributions to this study include the theory of speech acts, and the investigation of communication and the relationship between words and ideas, and words and the world. For particular problems *see* content, ideas, indeterminacy of translation, inscrutability of reference, language, predication, reference, rule following, semantics, translation, and the topics referred to under headings associated with logic. The loss of confidence in determinate meaning ('every decoding is another encoding') is an element common both to postmodernist uncertainties in the theory of criticism, and to the analytic tradition that follows writers such as Quine.

Poppycock.

Presumably, after the previous twenty-three chapters of this treatise, you, the reader, have gained an understanding of the explanations given for 'real', 'meaning', etc. by the Critical Philosophy. (If this has proven to be hard to keep track of – well that's what the glossary is for). The defining condition of "reality" is sensation (Margenau's 'plane of the immediately given'). The defining condition of a real thing is satisfied when I have a concept of the object containing sensation and connected with other concepts that provide my understanding of the object with a context and meanings. A transcendental idealist is also and always an empirical realist.

The usual ontology-centered objection to all 'idealist' metaphysics is: How do I know it is not true the only thing that really exists is me (or 'my consciousness' or 'my mind' or some other variation on this theme)? But truth is the congruence of the object and its concept. Human infants begin life in a frame of mind that could easily be called solipsism, as Piaget has shown, but we grow out of it. The division one draws between 'me' and 'not-me' is drawn as a real division grounded in *practical* distinctions. One might make idle intellectual arguments pretending to claim a solipsist position but one's actions belie the argument and reveal it as a mere playful and impractical skepticism[9]. Kant wrote:

> Practical belief is decided and completely certain, so that its affirmation of something as true is

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[8] *The Oxford Dictionary of Philosophy*.

[9] Bertrand Russell reported meeting someone who claimed she was a solipsist and was surprised more people were not.

> complete *in sensu practico* and cannot obtain any supplement even through the grandest grounds of speculation [AK16: 374].
>
> All certainty finally resolves itself into a sensuous certainty [AK16: 379].
>
> Experience is perception understood [AK17: 664].

Let us deal with the suspicion scientists harbor in regard to idealist metaphysics. In an ontology-centered metaphysic a skeptical position is possible. But the situation is otherwise in Kant's epistemology-centered metaphysics. Here the idealist position – the problematical argument that because we can 'really' only 'experience our own existence' and therefore any inference from a change in our own state to an outside thing that caused this change is hypothetical and uncertain – runs counter to the character of empirical consciousness and the pure intuitions of sense. Put another way, if we postulate the non-existence of objects of outer sense this is the same as postulating the non-existence of outer sense and its pure intuition. But this postulate leads to a contradiction. As Kant put it,

> One must here distinguish well between transcendental and empirical consciousness; the former is the consciousness "I think" and precedes all experience, first making it possible. But this transcendental consciousness affords us no cognition of our self; for cognition of our self is the determination of our *Dasein* in time[10], and for this to happen I must affect my inner sense. . .
>
> In our inner sense our *Dasein* is determined in time and thus presupposes the representation of time itself; in time, however, the representation of change is contained; change presupposes something that persists in which it changes and which makes it come to be that the change is perceived. To be sure, time itself persists but it alone cannot be perceived; consequently something that persists must be granted in which one can perceive the change in time. This that persists cannot be our own self, for as object of inner sense we are likewise determined through time; that which persists can therefore only be placed in that which is given through outer sense. Thus all possibility of inner experience presupposes the reality of outer sense [AK18: 610-611].

'Change' has no meaning except in relationship to its opposition to persistence. But the cognitive *determination* of that-which-persists is not a capacity of the pure intuition of time since we know time only through changes that distinguish one moment in time from another. Consequently we must grant that in addition to the pure intuition of inner sense (time) we also possess the pure intuition of outer sense (space). But the pure intuition of space is a topological structuring of sensation and, therefore, the idea of the pure intuition of space has no meaning without reference to the actuality of sensation. Sensation is objective (as opposed to feeling, which is affective and can never become part of the representation of an object). But the matter of sensation is precisely that which cannot be given *a priori* in any mere synthesis of imagination. Thus it follows that we must grant the actuality of our senses (that is, outer sense). But outer sense is the capacity for representations of receptivity and receptivity is the ability for the Organized Being to be affected

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[10] Recall that 'determination of my *Dasein* in time' means a cognitive determination of Self-*Existenz*.

by *objects* of outer sense. The matter of an object of outer sense is that which corresponds to sensation in perception. We must grant the actuality of outer sense and, in doing so, we must also grant the actuality of objects of outer sense.

> Outer sense thus has reality because without it inner sense is not possible [AK18: 612].

So much for problematical idealism.

We thus see that Margenau's 'plane of the immediately given' fits into the Critical framework in regard to objective validity. Now let us deal with ontological doubts regarding Margenau's 'constructs' idea. Margenau argued that "constructs are not *mere* constructs" and attempted to escape the charge of problematical idealism by an appeal to verification. This, however, proves to be a flimsy shield to defend against the suspicions with which idealist theories are viewed. On the other hand, the Critical metaphysic provides a ground for the objective validity of Margenau's 'constructs.' The construct of Margenau corresponds to the Object of the Critical Philosophy, and Object is the unity of the object and its representation. The *empirical realism* of Critical epistemology bestows *objective realism* upon Margenau's constructs.

Yet it remains for us to deal with the issue of *objective validity* in Margenau's construct structure. Specifically, what we must determine is how far this objective validity extends away from 'the plane of the immediately given.' Margenau argued, again, from a position of verification (hence his 'verifacts'). This much we must in all fairness grant Lakatos: He is pretty effective in demolishing the 'verification' argument and reducing it to a skeptical position[11]. What is Kant's answer to this issue?

The Critical weakness and short-coming in Margenau's epistemology is that it provides no 'stopping rule' to guard us from slipping past constructs with *real* objective validity into dialectic inferences of transcendent constructs. It might seem that his epistemological rule requiring two independent pathways from construct to the plane of the immediately given serves this function but it does not. 'Verification' is a necessary but not a *sufficient* condition for ensuring real objective validity in our concepts of supersensible objects (ideas of *noumena*). But, fortunately, Lakatos' "demarcationists" and "élitists" are wrong in holding that *no epistemological nor ontological rule* can be given for this. To understand this we must first clearly understand the Critical *Realerklärung* of the meaning of *noumenon*.

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[11] As I noted earlier, Lakatos would object to his position being called 'skeptical.' However, Lakatos had a relatively narrow definition of 'skepticism': "Skepticism regards scientific theories as just one family of beliefs which rank equal, epistemologically, with the thousands of other families of belief. One belief-system is no more right than any other belief-system although some have more *might* than others" [LAKA3: 107]. Under this usage of the term Lakatos is not a skeptic. Under Hume's usage of it he is.

§ 4.1 The *Noumenon* and Reproductive Imagination

A *noumenon* in general is the supersensible object of an idea. To be 'supersensible' means that *in* the concept of the object there is contained no representation of sensation (the "real in perception"). There is more than one way by which thinking can produce such a representation. Many such concepts lack objective validity altogether. Some retain the property of being objectively valid. Our concern lies with how to distinguish between the one case and the other because only those that retain the property of real objective validity can be called Objects of scientific knowledge and only these satisfy the intent of Margenau's term 'verifact'. All others are Objects of dialectical speculation or Objects of fantasy and their concepts are transcendent.

Given what was said previously it may at first seem a contradiction in terms to say that an object whose concept lacks representation of sensation can have *real* objective validity. However, as we will soon see, there is no actual contradiction in this idea. We begin by reminding ourselves that all thinking involves the free play of imagination and determining judgment under the regulation of reflective judgment. Now imagination is either *reproductive* imagination or *productive* imagination. We deal first with the case of reproductive imagination because it is only this *modus* of the synthesis of imagination from which we can obtain concepts of *noumena* that stand with real objective validity. Furthermore, we must restrict the scope of this consideration to only those concepts arising under an *inference of ideation* by reflective judgment. After this we will be able to examine the situation we face when the act of reflective judgment produces an inference of induction or an inference of analogy.

With these limitations in place we next recall that the final act of the synthesis in sensibility in producing the intuition of an appearance is the *Verstandes Actus* of abstraction. Under an inference of ideation the subsequent free play of imagination and determining judgment delivers up a concept that understands at least two lower concepts, to which it is connected in a determinant judgment. These lower concepts are those that were made comparates under the synthesis of reproductive imagination and are said to both "stand under" the higher concept. The lower concepts are said to be "contained under" the higher concept and the higher concept is said to be "contained in" the lower concepts. Figure 24.4.1 illustrates these relationships.

Root concepts in the manifold of concepts originate from an inference of ideation, and as rules for the reproduction of intuitions they possess the information required to reproduce the matter of sensation in the synthesis of reproductive imagination. These concepts are made distinct through the thinking of coordinate marks, these marks being higher concepts of what two or more concepts (of which they are marks) have in common. The *Verstandes Actus* of abstraction removes factors that are not shared in common by the lower concepts and consequently the higher

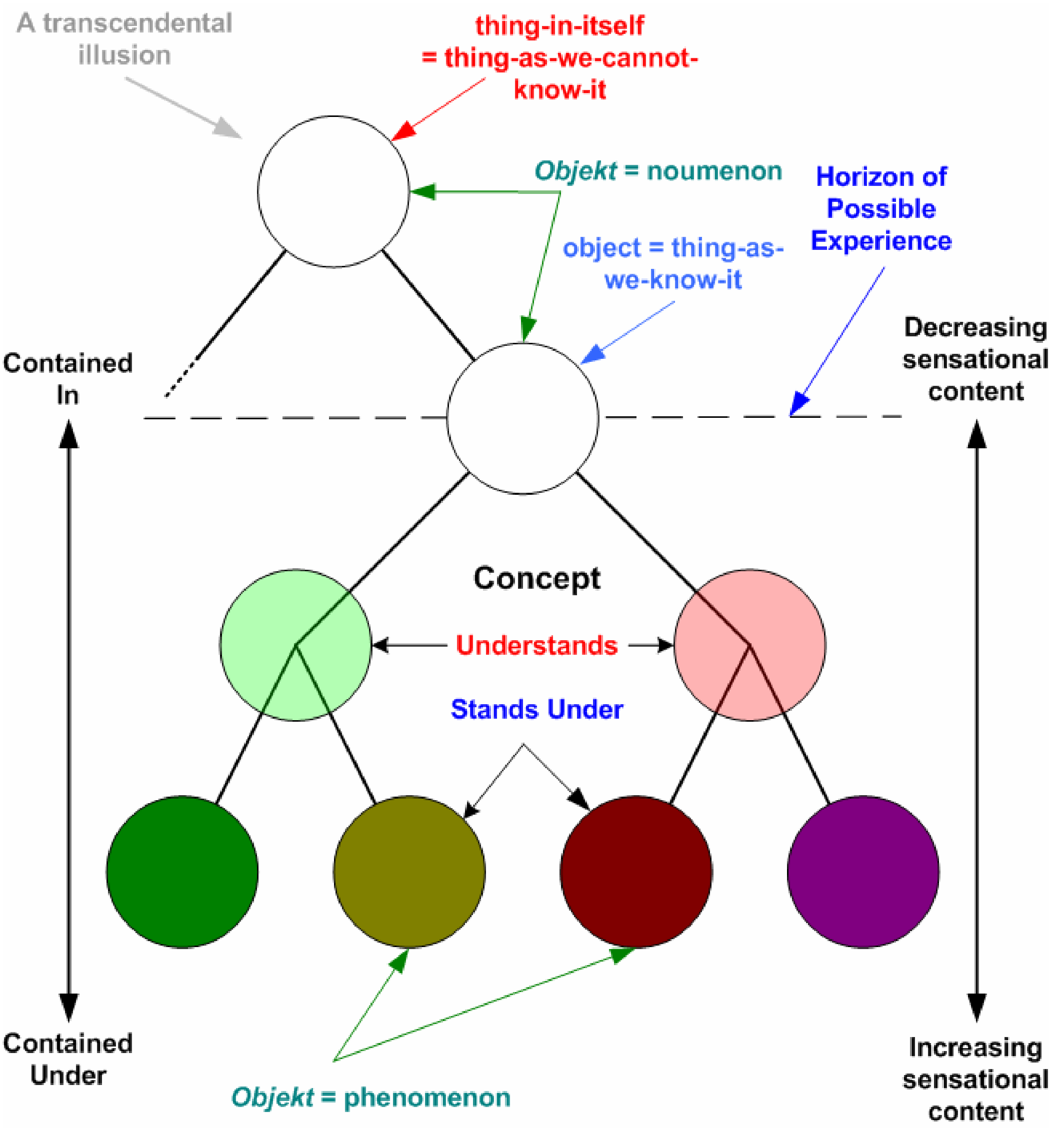

Figure 24.4.1: Illustration of the horizon of experience in the synthesis of ideas. As the manifold of concepts progresses from lower to higher concepts there is a progressive decrease in the degree of intensive magnitude contained in each concept-circle. The lowest unfilled circle represents the first point in the synthesis of higher concepts where the degree of sensation vanishes. (The figure is greatly simplified inasmuch as the number of levels of synthesis and the number of combinations of determinant judgments prior to this negation will generally be many more than is depicted in this illustration).

concept must have a lesser extensive and a lesser intensive magnitude than the concepts contained under it. The latter is denoted in the illustration by a reduction in the depth of color shading of the circles representing the concepts.

At some point in the on-going synthesis of higher concepts the process will arrive at a coordinate concept in which the degree of sensational content vanishes. This coordinate concept therefore no longer contains the real in sensation and is a concept of pure form. It is a concept for which no intuition of an object of experience is possible and is thus a concept for which the object is supersensible. We call such a concept an *idea*. Nonetheless, the concept retains an *immediate* connection to lower concepts that still contain non-vanishing degrees of sensational matter directly traceable back down the series to concepts of phenomena of actual experience. It is this *immediate connection* in actual experience that provides this *lowest noumenal concept* with objective validity. The Object for this *noumenal* concept is a *noumenon* and we call its object a *thing-as-we-can-know-it*. The concept stands as a *boundary mark* at what Kant called ***the horizon of possible experience***. The horizon of possible experience is the farthest extension of deep distinctness in understanding beyond which no theoretical objective validity can be claimed.

Although these boundary ideas of *noumena* are concepts for which the corresponding intuitions are those of empty time, still these concepts are not representations of *nothing* because they still retain rules of pure *form*. Because neither reflective judgment nor pure Reason are immediately concerned with concepts of objects, a continuation of coordinate synthesis is still possible for these concepts. Such a synthesis is illustrated in figure 24.4.1 by the higher unfilled circle. This concept (idea) represents a coordinate mark of two lower ideas, both the latter possibly being ideas that lie on the horizon of possible experience. However, because the two concepts standing under the coordinate mark have no degree of sensation their coordinate concept has *no immediate connection in any possible experience* and is therefore *the concept of a pure noumenon*. It is a transcendent idea and is in no way necessary for the possibility of experience. Its object is a thing-in-itself, a term we can now understand as *a thing-as-we-cannot-know-it*. The categories of understanding are rules *of experience* but here they produce only an illusion of this.

The concept of this *noumenon* will always satisfy Margenau's rule of proper constructs, being neither peninsular nor an "island universe". However, this Object lacks *real* objective validity and therefore *it cannot satisfy the intent of Margenau's word 'verifact'*. **This is the Critical criterion for judging scientific constructs: A construct that stands as a coordinate concept only of two or more *noumenal* constructs is not a verifact**. The highest *objectively valid constructs* in figure 24.2.1 are those that stand in immediate relationship with constructs of actual experiences (those 'under' the horizon of experience in figure 24.4.1).

For example, in some physics textbooks one occasionally comes across a statement to the effect, "mass is the seat of gravity." Now it is clear to any scientist that 'mass' and 'gravity' share some common sphere of concepts; this is merely to say we know they have a relationship. But to mark the idea of 'mass' as containing a property of 'being a *seat*-of-gravity' says more than we can know from actual observations or experiments. Being a 'seat-of-gravity' is no more (and no less) than merely a *statement of a formal relationship* between two ideas (those of 'mass' and 'gravity'). On the other hand if one is careful to say the concept 'seat-of-gravity' is to mean no more than that measurements of the moving power of 'gravity' correspond in a precise way with determinations of 'massive objects' in space, then the idea of 'seat-of-gravity' can be "moved down" to the horizon of possible experience by providing the idea with immediate relationships to objectively valid lower concepts of actual experience. The point I am making here is just this: precise *conditions of limitations* in the meanings of *noumenal* ideas are required to guard against slipping past the horizon of possible experience and thereby losing objective validity for the idea. The practice and habit of always supplying such conditions of limitations **is a Critical maxim of discipline** for scientific reasoning.

We have now seen an illustration of what Kant means by the term '*noumenon*' and we have seen that we must distinguish between a *noumenon* as thing-as-we-can-know-it and a *noumenon* as thing-as-we-cannot-know-it. The latter is a transcendental illusion; the former has theoretical objective validity. In regard to the conditions to be set in the manifold of concepts, it is worthwhile to say once more that these conditions are the conditions set down earlier in the discussion on the synthesis of imagination and on the type of inference of judgment (specifically, that they must be inferences of ideation) that went into the construction of the manifold leading to the *noumenal* idea. These determinant judgments of coordination have the Modality of actuality.

Failure to recognize there are these two types of *noumena* and that only one of these types retains objective validity is, I think, another factor in a common misunderstanding of Kant's theory, namely, "Kant says we cannot know the real world." The first factor – namely placing ontology in the center of metaphysics rather than epistemology – goes to the heart of what one means by the idea of 'the *real* world.' This second factor – failure to distinguish the types of *noumena* – goes to the idea given a tongue by Lakatos earlier, namely that the Kantian system is a 'prison'. Once one has decided this, the 'prison' idea implicates two logical conclusions: First, that the Critical Philosophy must lead one to skepticism; Second, that the Critical Philosophy must implicate a problematic idealism. I think we have already adequately dealt with and disposed of these conclusions, but let us follow up on this 'prison' idea a bit more. If the fact that a human being's knowledge of Nature[1] has limits[2] constitutes a 'prison,' it is indeed a strange prison with 'movable walls.' Here is what I mean by this.

It is a naive presumption to think the restriction of the notion of reality in the Quality of determinant judgments in any way rules out or invalidates *the use of scientific instruments* to *bring to us* the matter of sensation. Many people either do not know or they forget that Kant's early career was the career of a scientist. Indeed, he occasionally makes reference to using a

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[1] in Kant's sense of this word rather than Margenau's
[2] the horizon of possible experience

telescope in astronomical observations. Kant was not a stranger to the role of instruments in science. But what does an instrument do? It is a mechanism for extending the power of receptivity to phenomena that could otherwise not affect one's unaided outer sense.

But how is this? Obviously what an instrument such as an oscilloscope or a spectrometer measures is what an unscientific person might denigrate as 'occult quantities'. We do not, for example, possess a 'sense of voltage' as part of the somatic capacity of a human body. What such a view misunderstands is this: The measurements reported by a scientific instrument are designed to follow and conform with objectively valid constructs built 'upward' from Margenau's 'plane of the immediately given'. More specifically, the design of a modern instrument begins with what are known as scientific *standards* (e.g. the 'standard meter', the 'standard second', and so on). These standards are physical objects immediately employable for the power of receptivity. As the operation of an instrument 'moves away' from Margenau's Nature into the realm of constructs there is a definite and objectively valid series of connections corresponding to each operation employed in the instrument. Ascertaining the correctness of each step is made by a process known as 'calibration of the instrument'.

That the measuring process involves 'theory' is clearly true. But this is precisely what Lakatos meant earlier when he said, "theories are extensions of our senses." Before the invention of the microscope many people would have regarded the idea that tiny organisms invisible to the eye were the cause of disease as ridiculous and unfounded. In those days everyone 'knew' that disease was a just punishment from God. Educated people now are better informed[3]. Bacteria and viruses have been 'made real' (epistemologically speaking) by the invention of instruments.

In the United States there is a bureau within the federal government that once bore the name 'National Bureau of Standards' (today it goes by the acronym NIST[4]). The mission of the Bureau was to provide a means by which the makers of scientific instruments could ensure that their products did in fact accurately perform the function they were claimed to perform. Strict adherence to the 'traceability' of the measurement function back to the defining standards is something instrument manufacturers know they must accomplish if they are to remain in business for long. A scientific instrument that cannot demonstrate step-by-step the manner by which its operations connect back to the fundamental standards of measurement is not in fact a *scientific* instrument at all.

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[3] Unfortunately, this has not meant that superstition and ignorance has been obliterated from our culture. There are still people who simply transfer the old superstition and say bacteria and viruses are 'instruments of God' in inflicting punishment on the victims of disease. Some still hold with 'possession' by 'demons'. Some even still hold that there are witches and magic. It is an amazingly sorry state of human affairs.

[4] Personally, I better-liked the old name. The name made the intended *focus* of the bureau clear. I admit to sometimes wondering if the 'new' bureau still understands the *primary* importance of their original charter.

Scientific instruments make possible the construction of more layers of concepts wherein the *realizable* in sensation is maintained. By doing so *they extend the horizon of possible experience*. Here it is worth saying the phrase ‘possible experience’ does not mean *problematic* experience. It means deeper distinctness *in actual experience* (experience understood through concepts judged with the Modality of actuality & non-being) *is possible*. Extending the horizon of possible experience via instruments *employs judgments of the category of necessity & contingency*.

§ 4.2 Transcendental Speculation and Transcendental Illusion

Now let us look at how objective validity can be lost in the construction of the manifold of concepts. Perhaps the most obvious opportunity for this arises when thinking employs the power of *productive* (rather than reproductive) imagination. Because they are intuitions *a priori* (prior to a *specific* actual experience), products of productive imagination when conceptualized are concepts of speculation. Here we must distinguish between two judgments of Modality involved in their construction: the merely problematical judgment (category of possibility & impossibility), and the apodictic judgment (category of necessity & contingency). The former is a *creative speculation*. The latter is a *predictive speculation*. I will be so bold as to say science could not advance a single step without these capacities in judgmentation, but if science is not to *misstep* we must have Critical *discipline* in speculation.

To better appreciate the epistemology of these types of speculation we must appreciate the role of the Critical Standpoints in judgmentation. For the discussion that follows it will be helpful to refer to Figure 24.4.2, which summarizes the functional organization of the faculty of *nous*. Figure 24.4.3 illustrates the first level synthetic representation (1LSR) in the context of the Standpoints. All acts of synthesis are inherently three-fold and involve something determinable, a determination, and the union of the determinable and the determination. The synthesis of a coordinate mark of two concepts, viewed from the Standpoints, involves the representations of a belief (the determinable) and a cognition (the determination); belief regarded as cognition serves *purpose* through logical and aesthetical perfection. To better understand this, one recalls that the synthesis in sensibility, which produces the intuition to which the coordinate concept corresponds, is judged by reflective judgment (which is a non-objective judgment of the matter of affective perceptions under the principle of the formal expedience of Nature). The act of synthesis is an act of adaptation aiming for the negation of *Lust per se* through the opposition of the feelings of *Lust* and *Unlust*. Put somewhat loosely, the Organized Being *recognizes* what it is *seeking* but the latter is an object of appetite (which belongs to pure practical Reason) effected on the one side through motoregulatory expression and on the other by ratio-expression. This can be

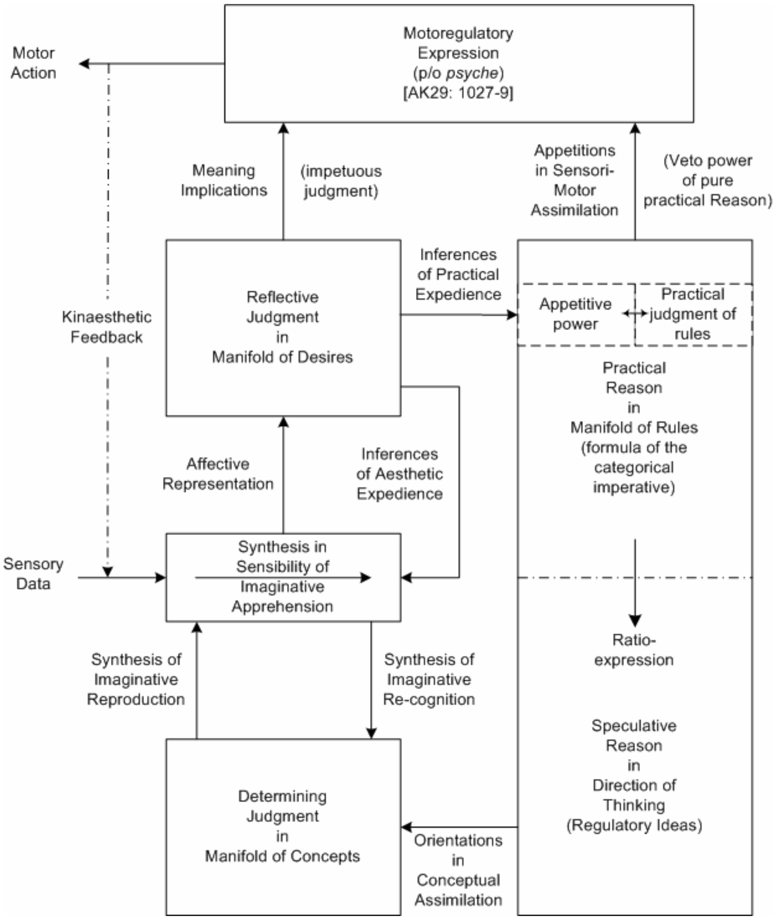

Figure 24.4.2: Functional Organization of the Faculty of *Nous*.

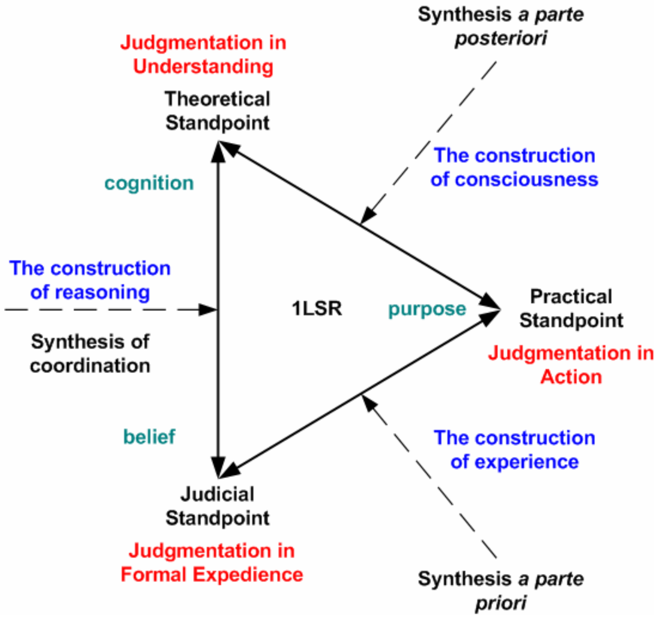

Figure 24.4.3: First level synthetic representation in the context of the Standpoints. Acts of synthesis are inherently three-fold involving something determinable (lower left corner), a determination (upper left corner) and the union of the determinable and the determination (right corner). From any two of these synthesis proceeds via the third.

called the practical construction of reasoning.

Empirical consciousness is the representation that a representation is in me. But such a representation is clearly not a mere representation of sensibility but, rather, belongs to judgment. All root concepts and (later) all higher concepts are first formed through belief. The capacity for the synthesis of deeper distinctness in the manifold of concepts we might say is guided by belief and driven by purpose. Judgmentation in understanding (cognition) is a prosyllogism (synthesis *a parte priori*), its act is the construction of experience, and its outcome is understanding. From this point of view belief replaces the 'wax tablet' or 'blank paper' that empiricism's copy-of-reality hypothesis had to suppose. Kant remarked,

> In experience alone can our concepts be given fully *in concreto*, hence their objective reality come to be fully presented. Concepts to whose nature it is contrary to be presented in experience are mere Ideas. Hence the objective reality of all concepts, i.e. their signification, is to be sought in the reference to possible experience. Others, which are, namely, mere Ideas, can certainly be assumed as hypotheses but cannot count as demonstrable.
>
> Now when it has to do with the possibility of pure knowledge *a priori* we can transform the question into this: whether experience contains merely knowledge that is given only *a posteriori*, or whether something is also encountered in it which is not empirical and yet contains the ground of the possibility of experience.
>
> There first belongs to all experiences the representation of the senses. Second, consciousness; this, if it is immediately combined with the former, is called empirical consciousness, and the representation of the senses combined with empirical consciousness is called perception. If experience were nothing more than an agglomeration of perceptions then nothing would be found in it which is not of empirical origin.
>
> But the consciousness of perceptions relates all representations only to our self as modifications of our state; they are in this case separables, and are especially not cognitions of any things and are related to no Object. They are thus not yet experience, which must, to be sure, contain empirical representations, but at the same time must contain cognition of the objects of the senses. . .
>
> Thus experience is possible only through judgments, in which to be sure perceptions comprise the empirical materials, but the reference of the same to an object and the cognition of the same through perceptions cannot depend on empirical consciousness alone [AK18: 385-386].

The synthesis of non-objective reflective judgment and practical judgmentation in action *stimulates* the construction of cognitions of objects. Now, an intuition that undergoes the synthesis of re-cognition in imagination to bring into determining judgment a higher concept does not yet meet with the condition of equilibrium *until* this concept has undergone combination in a determinant judgment *and* the product of this has been marked again in sensibility (intuition of the concept *of the determinant judgment*. It is this act of which we speak when we speak of 'harmonization of the inner loop' in the free play of imagination and determining judgment). The act of combination of concepts in determining judgment proceeds *from* the re-cognized higher concept to the concepts brought to stand under it and is thus a synthesis *a parte posteriori* (an episyllogism). We can justly call this *the construction of empirical consciousness* in thinking.

No copy-of-reality mechanism stamps an impress upon our minds. One immediate consequence of this fact is that human understanding always has its roots in subjectivity and our understanding of objects thus always begins as a persuasion. The "beauty of a theory" inheres in persuasion. We discussed this before, in Chapter 18, but it is a point that bears repeating.

> Holding-to-be-true is an occurrence in our understanding that may rest on objective grounds, but that also requires subjective causes in the mind of he who judges. If it is valid for everyone merely as long as he has reason, then its ground is objectively sufficient, and in that case holding-to-be-true is called *conviction*. If it has its ground only in the particular constitution of the subject then it is called *persuasion*.
>
> Persuasion is mere semblance because the ground of the judgment, which lies solely in the subject, is held to be objective. . . Truth, however, rests upon congruence with the Object, with regard to which, consequently, the judgments of every understanding must agree . . .
>
> Accordingly, persuasion cannot be distinguished from conviction subjectively . . . but the experiment one makes on the understanding of others, to see if the grounds that are valid for us have the same effect on the reason of others, is a means . . . for revealing the merely private validity of the judgment, i.e. something in it that is mere persuasion [KANT1a: 684-685 (B: 848-849)].

Elsewhere Kant comments,

> Holding-to-be-true and truth are distinct subjectively and objectively. If holding-to-be-true is subjectively incomplete it is called persuasion. But a subjectively complete holding-to-be-true is conviction, the state of mind where I give approval to a cognition, the holding-to-be-true [KANT8a: 302-303 (24: 849)].

I am inclined to suspect that many of us have met at least one "person of conviction" who "stands up for his beliefs" in the teeth of what *we* see as compelling evidence to the contrary. It is for such people we often reserve adjectives such as 'stubborn', 'close-minded', 'opinionated' and, sometimes, 'ignorant'. Most professional scientists will "stand up for" a long-held theory when it is challenged either by experimental results or a new theory that runs counter to the old. This can be both good and bad. It is good if further research uncovers an error in the experiment or the new idea; it is bad if he clings to the old idea long after a preponderance of evidence turns against it. Lakatos commented, in regard to the falsification of theories, that

> scientists frequently seem to be irrationally slow: for instance, eighty-five years elapsed between the acceptance of the perihelion of Mercury as an anomaly and its acceptance as a falsification of Newton's theory, in spite of the fact that the *ceteris paribus* clause was reasonably well corroborated. On the other hand, scientists frequently seem to be irrationally rash: for instance, Galileo and his disciples accepted Copernican heliocentric celestial mechanics in spite of the abundant evidence against the rotation of the earth; or Bohr and his disciples accepted a theory of light emission in spite of the fact that it ran counter to Maxwell's well-corroborated theory [LAKA2: 30].

When Einstein turned his back on the statistical interpretation of the quantum theory, most physicists saw this as tragic and misguided. Of all the findings of Kant's theory, I suspect the one scientists will find most unpalatable is the finding that even our most 'objective truths' are raised upon a subjective foundation[5]. This is nowhere more the case than for the case of transcendent ideas of *noumena*. Kant remarked that in many cases of 'pseudo-proofs' conviction is attained by means of a fundamental 'weakness' in the Nature of human reasoning, namely where

> a great abundance of evidence for the origin of natural things in accordance with the principle of purposes is adduced and advantage is taken of the merely subjective ground of human reason, namely its native propensity to conceive of one principle instead of many as long as it can do so without contradiction and, where only one or several of the requisites for the determination of a concept are found in this principle, to complete the concept of a thing by means of an arbitrary supplement [KANT5c: 325-326 (5: 461)].

It is with this "arbitrary supplement" by means of other concepts that we are presently concerned.

A person who is anchored by strong persuasion to the habit of viewing the world strictly by way of an ontology-centered focus seems to be habitually able to do without a clear distinction of the difference between existence as *Dasein* and existence as *Existenz*. 'Existence' in the connotation of 'being' is, however, not a real predicate. Kant writes,

> I now ask: if *Dasein* is no predicate of things, how can I then make use of the expression '*Dasein*' at all; for this discerns something with regard to the thing which can be assigned to it from the thing itself. E.g. a certain thing comes to actuality. Because the very same thing can be posited in various ways, this positing itself seems to be a mark of the difference which, as a predicate, one could attribute to a thing or separate from it. It is, however, certain that this difference merely comes to how this thing is posited with all that belongs to it, and not to what is posited in it. . .
>
> The concept of an existing thing can never be altered in a judgment in which the thing would be the subject and *Existenz* the predicate.
>
> On the contrary, something existent must be the subject and all that belongs to it must be its predicates [AK17: 242-243].

All our concerns with the 'reality' and the 'actuality' of any thing come down to clearly understanding what it is we posit as 'belonging to' the thing, how we came to make the specific attributions, and the ground from whence judgmentation acted.

Productive Imagination and Inferences of Analogy

Let us first consider concepts arising from productive imagination and an inference of analogy. The most easily dealt with are those creative concepts of *fictions*. It would come as a profound shock to me were I to meet a mild mannered reporter at a great metropolitan newspaper who could leap tall buildings in a single bound, change the course of mighty rivers, bend steel in his bare hands, and who spent all his spare time fighting for Truth, Justice, and the American Way. What goes into the making of the idea of Superman?

The origin of this fictional character clearly lies in *purposes*: to entertain via storytelling; to

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[5] I will comment in passing here that it is this underlying subjectivity which contributes in no small way to why scientific truth is hard to come by and is gained only with great effort.

make money (probably); and perhaps in some degree to moralize in the tradition of Aesop's *Fables*. Obviously the Superman character originates entirely from spontaneity and the causality of freedom, and is the product of *productive* imagination and the fictive faculty. As to the 'nature' of the Superman character, he is to be like a man but is to be endowed with superhuman capabilities. It is clear that in establishing his characteristics we are seeing the handiwork of inference by analogy. Here the Object was conceptualized first (no doubt drawn from other concepts such as 'hero') and his specific characteristics of appearance were added afterward, perhaps using some number of artist sketches by the original cartoonist. Although it is fruitless to speculate upon what was going through the mind of the original cartoonist as the character was taking shape, it is at least obvious that we are seeing the fruit of a complex anasynthesis in which the specific representations of the character were products of numerous episyllogisms (syntheses *a parte posteriori*) by which marks were freely given to the Superman concept rather than having been extracted *from* the representation of the Object.

While we do not know what I will call the 'mental history' of the judgmentation and thinking carried out by the cartoonist who created the Superman character, we can nonetheless use figure 24.4.3 to visualize a simplified illustration of a kindred series of syntheses. We start with the synthesis *a parte posteriori* (sweeping down to the lower left corner of the triangle) to gather up partial concepts for appearances of the Superman Object. We follow with a synthesis *a parte priori* (sweeping up to the upper left corner) to place these partial concepts in recognized concepts of appearances. We then carry out a synthesis of coordination to combine partial appearances in the concept of the *specific* object (Superman) according to our purposes. Were I the cartoonist I might now have a rough sketch of the character or perhaps a short list of attributes (e.g., 'he is male', 'he is clean-shaven', 'he is square-jawed', etc.). The syntheses are repeated, building up the partial appearances to 'fill in more detail'.[6]

It is obvious that Modality in the determinant judgments of these concepts could have been none other than the *momentum* of possibility & impossibility. Certainly no detail of the appearance – color of his hair, shape of his face, specific musculature of the torso, the colors of his suit – contain sensation originating *immediately* through receptivity.[7] All had to have been borrowed from other examples and made part of the appearance through the free play of determining judgment and productive imagination. The marks of appearance can therefore be

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[6] I remark in passing that this closed cycle of syntheses fits Piaget's definition of a scheme.

[7] This is not to say that the *actions* in producing the image of the character did not at all involve receptivity. If the original artist drew sketches of the character then obviously the appearance of the sketches is known through receptivity. But here we must bear in mind that the actions in *making* these sketches are determined purposively and *a priori* according to partly subjective and partly objective intentions, and the standard of judgment subsists in the purposiveness of the actions. The sketch is a partial exhibition *in concreto*.

judged neither by actuality & non-being nor, even less, by necessity & contingency. (Here, too, we see an example of the regulation of the *Verstandes Actus* in the synthesis in sensibility by aesthetical reflective judgment). It is here, in the Modality of the determinant judgments, that we find the most important difference between the idea of Superman and the idea of the type of *noumenon* at the horizon of experience in science. In the previous section the specific coordinate concepts were the product of synthesis *a parte priori* and this synthesis is carried out through the combined powers of receptivity and spontaneity with the Modality of actuality & non-being in the logical-theoretical reflective perspective. The synthesis of the image of the Superman character involves spontaneity alone under the inducement of an aesthetical Idea (Modality of possibility & impossibility in logical-theoretical perspective).

Although the logical difference being discussed here is merely a difference in Modality (judgment of a judgment), this is the crucial difference between the *noumenon* in the previous section and the idea of Superman. In the previous section the *noumenon* is object-in-Nature and its limitation in Reality is that of a 'real thing' or 'thing-in-Nature'. In the case of Superman *the Superman character is not the* noumenon. The *noumenon* can be called 'fictional characters'. Thus we say Superman is real *as a fictional character* but unreal as a living-thing-in-Nature. We look for him in comic book collections and movies, not in newspaper offices or telephone booths. He is a deliberate fiction.

Of course all of us (with the possible exception of some children) know this. In the discussion above we have taken on the point of view of Superman's creator. But what of the person who reads or hears a fictional story? Let us consider Pliny's absurdities. It seems that Pliny himself must have thought the fantasies he recorded in his *Natural History* were true. Solinus' *Collectanea Rerum Memorabilium* was still presenting Pliny's umbrella-footed and dog-headed people in the third century A.D., and *Historia Naturalis* remained a textbook (at least so far as astronomy and some other topics were concerned) well into the middle ages. It stretches credulity to think that in those superstitious ages no one took Pliny's fantastic beings seriously. So far as acceptance of such myths is concerned we are looking here at the phenomenon of *persuasion by authority*. The readers of Pliny's book did not have to create these fictions for themselves; Pliny's book handed them to the reader ready-made and the reader's own capacity for inference by analogy would be enough for him to 'get the picture.' After this it is merely a matter of making this picture 'fit' within a general context in that person's manifold of concepts.

> Freedom in thinking means the subjugation of reason to no other laws *except those which it gives itself*; and its opposite is the maxim of a **lawless use** of reason (in order, as genius supposes, to see

> further than under the restriction of laws[8]). The consequence of this is the natural habit: that if reason will not subject itself to the laws it gives itself, it must bend under the yoke of laws given by another; for without any law nothing, not even the grandest nonsense, can drive its play for long. Thus the unavoidable consequence of *professed* lawlessness in thinking (of a liberation from the restrictions of reason) is this: that freedom to think will ultimately be forfeited[9] [KANT12a: 16-17 (8: 145)].

It is worthwhile to compare Kant's comment about this 'natural habit' to the phenomenon of Piaget's 'unilateral respect' exhibited by the child in the early stages of moral realism, and to remind oneself that we all went through this stage of development and did set some maxims of thinking for ourselves when we did.

Productive Imagination and Inferences of Induction

Analogy when applied too obviously in making predications of the *Existenz* of objects in Nature is usually distrusted by scientists and the history of science demonstrates there is good reason for this. Heavy use of analogy was made during the decline of Scholasticism and this use, joined with 'Aristotelian' logic, led to a peculiar sort of resigned pessimism in regard to the pursuit of knowledge. We catch some of the flavor of this in the writings of Nicholas of Cusa.

> We see that by a divine gift there is within all things a certain natural desire to exist in the best manner in which the condition of each thing's nature permits. Toward this end all things work and possess the appropriate instruments. They also have an inborn judgment agreeing with the purpose of their knowledge so that their desire may not be frustrated but may be able to attain rest in that object which the inclination of each thing's own nature desires. If at some time this is not the case, it is necessarily the result of an accident, as when sickness deceives taste or opinion misleads reason.
>
> Therefore we say that the sound and free intellect knows as true that which, from an innate searching, it insatiably longs to attain and apprehends in a loving embrace. For we are convinced that no sound mind can reject what is most true. But all who investigate judge the uncertain proportionally by comparing it to what is presupposed as certain. Therefore every inquiry is comparative and uses the method of proportion. As long as the objects of inquiry can be compared by a close proportion leading back to what is presupposed as certain, our judgment understands easily, but when we need many intermediaries then we are faced with difficulty and hard work. This is acknowledged in mathematics, where earlier propositions are more easily led back to the first and most evident principles, but subsequent propositions give more difficulty since they are led back to first principles only by means of the earlier propositions.
>
> Every inquiry, therefore, consists in a comparative proportion that is either easy or difficult. Because the infinite escapes all proportion the infinite as infinite is unknown. But since proportion expresses agreement in some one point and also expresses otherness, it cannot be understood apart from number. Number, therefore, includes all that is capable of proportion. Hence number, which

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[8] One present-day expression popularly used to capture the sentiment of this statement is "thinking outside the box."

[9] Once one has constructed a sturdy concept structure, including conceptualized maxims of thinking, from the "thinking outside the box" point of origin, one has a commitment to (and can become trapped within) a 'new box.' Here the type-α compensation behavior (ignoring disturbing factors) can play a prominent role. For example, Superman is always clean-shaven and neatly barbered, but no razor can cut his hair. His suit is impervious to bullets and invulnerable to fire, but his adopted mother tailored it for him from his baby blanket.

> effects proportion, does not consist in quantity only but also in all those things which in any way can agree or differ substantially or accidentally. Perhaps this is why Pythagoras insisted that all things are constituted and understood through the power of numbers.
>
> However the precise combinations in corporeal things and the congruent application of known to unknown so far exceeds human reason that Socrates believed he knew nothing except that he did not know. The very wise Solomon declared that all things are difficult and cannot be explained in words, and another thinker of divine spirit says that wisdom and the seat of understanding lie hidden "from the eyes of all the living." Likewise, the very profound Aristotle, in the First Philosophy, asserts that with things most evident by nature we encounter the same difficulty as a night owl trying to look at the sun. If all this is true, since the desire in us for knowledge is not in vain, surely then it is our desire to know that we do not know. If we can attain this completely, we will attain learned ignorance. For nothing more perfect comes to a person, even the most zealous in learning, than to be found most learned in the ignorance that is uniquely one's own. One will be the more learned the more one knows that one is ignorant.[10]

There is wisdom in what Cusa writes, but it also contains enough seed to plant skepticism and justify surcease from science. To sentiment such as this Bacon reacted vigorously:

> The school of Plato introduced skepticism, first, as it were in joke and irony, from their dislike to Protagoras, Hippias, and others, who were ashamed of appearing not to doubt upon any subject. But the new academy dogmatized in their skepticism, and held it as their tenet. Although this method be more honest than arbitrary decision . . . yet when the human mind has once despaired of discovering truth, everything begins to languish. Hence men turn aside into pleasant controversies and discussions, and into a sort of wandering over subjects rather than sustain any rigorous investigation [BACO2: 115-116].

Bacon saw the use of analogy, embedded in and dressed up as classical logic, to be a source of error and illusion. He wrote,

> The greatest and, perhaps, most radical distinction between different men's dispositions for philosophy and the sciences is this, that some are more vigorous and active in observing the differences of things, others in observing their resemblances; for a steady and acute disposition can fix its thoughts, and dwell upon and adhere to a point, through all the refinements of differences, but those that are sublime and discursive recognize and compare even the most delicate and general resemblances; each of them readily falls into excess, by catching either at nice distinctions or shadows of resemblance [BACO2: 111-112].

The Scholastics were in their own way as vigorous in the use of formal logic as were the Stoics centuries before them. But in Bacon's eyes the manner in which they employed logic did more harm than good.

> The subtlety of nature is far beyond that of sense or of the understanding; so that the specious meditations, speculations, and theories of mankind are but a kind of insanity, only there is no one to stand by and observe it.
>
> As the present sciences are useless for the discovery of effects, so the present system of logic is useless for the discovery of the sciences.
>
> The present system of logic rather assists in confirming and rendering inveterate the errors founded on vulgar notions than in searching after truth, and is therefore more hurtful than useful.
>
> The syllogism is not applied to the principles of the sciences, and is of no avail in intermediate

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[10] Nicholas of Cusa, *On Learned Ignorance*, 1440, H. Lawrence Bond (tr.).

> axioms, as being very unequal to the subtlety of nature. It forces assent, therefore, and not things.
>
> The syllogism consists of propositions, propositions of words; words are signs of notions. If, therefore, the notions (which form the basis of the whole) be confused and carelessly abstracted from things, there is no solidity in the superstructure. Our only hope, then, is in genuine induction [BACO2: 107-108].

Mathematics especially and modern science generally tend to trust the fruits of induction, although mathematicians by the nature of their profession have a better appreciation that from time to time induction can go seriously awry. It is not wrong to say that analysis is that branch of mathematics tasked with finding out what went wrong when induction leads to absurdity. One of the best known examples is the "proof" that 0 = 1. The specious induction goes like this:

$0 = 0$ (a simple tautology);
$0 = 0 + 0 + 0 + \ldots$ (property of the additive identity);
$0 = (1 + -1) + (1 + -1) + (1 + -1) + \ldots$ (property of the additive inverse);
$0 = 1 + (-1 + 1) + (-1 + 1) + (-1 + 1) + \ldots$ (associative property of addition);
$0 = 1 + 0 + 0 + 0 + \ldots$ (property of the additive inverse);
$0 = 1$ (absurd inference of induction).

What goes wrong in this argument is a nicely subtle presupposition, namely that the infinite series of additions *approaches a limit*. Mathematical analysis concludes that this presupposition is false because when the series consists of a whole number of (1, -1) pairs we get zero on the right-hand side but when the series does not we get 1. In other words, any finite series oscillates between 1 and 0 and therefore possesses *no* limit as the series goes to infinity. Our induction *ad infinitum* was, the Analysts will tell us, "improperly done" in this case.

This is one reason why the study of limits is so important in mathematics. In the case of the example we have just looked at, the 'illegal' operation came when we invoked the associative property of addition. Simply put, the infinite series of sums we wrote down at the third step does not obey the associative property of addition (owing to the absence of a specific limit in the series). This may seem like a small point to the non-mathematician but there is something here scientists and engineers (mathematicians excluded) often feel uncomfortable about. It is this: the associative property is one of the fundamental defining properties of the additive group. The implication of our example is: here is a special case where 'addition' is 'different'; the addition operation shown above does not obey a group structure. Simply put, "the rules have changed."

It does not require this example for us to know that group structure is a casualty when mathematical infinity becomes involved in our mathematics. This is evident enough from something even freshman students know, namely the rule $\infty + 1 = \infty$. This, too, is contrary to the property of a group; in this case the violated property is the uniqueness of the additive identity. The Analysts comfort us by telling us we should not regard "∞" as a number (or, at least, not a number in the usual sense). It is little wonder Davis and Hersch called infinity "the miraculous jar of mathematics."

However, we need not explicitly invoke the ∞ symbol to come up with mathematical results that learners usually find a bit at odds with how they are accustomed to seeing the world. The relationship between fractions and the 'long-hand' algorithms we are taught for doing division and multiplication provide another example. In fraction arithmetic we are taught

$$\frac{1}{3}+\frac{1}{3}+\frac{1}{3}=3\times\frac{1}{3}=1.$$

This is one example of the very common-sense rule of fractions that $a/a = 1$ for any non-zero integer a. On the other hand, when we are introduced to decimals and the algorithm of long division we are taught

$$1\div 3=0.33333\cdots.$$

But the algorithm we learn for doing long multiplication then implies

$$3\times(1\div 3)=0.99999\cdots$$

since by induction we can see no 'place' in the infinite string of '3's where a carry would occur. Thus, comparing fraction arithmetic with decimal arithmetic, we seem to be forced to conclude $1=0.99999\cdots$.

What do the Analysts tell us here? In this case we are told, "Yes, that is correct." The answer to this riddle lies with how we must properly *interpret the notation* $0.99999\cdots$. When we compare fraction arithmetic with 'long division arithmetic' most of us make the reasonable supposition that because our teachers teach us both methods both methods must be equivalent. The Analysts tell us, "Yes. But you must properly understand what 'equivalent' means here." In this example we re-write $0.99999\cdots$ as

$$0.99999\cdots=9\times 0.11111\cdots=9\times(0.1+0.01+0.001+\cdots)=\lim_{n\to\infty}9\times\sum_{k=1}^{n-1}(0.1)^k .$$

Now for any *finite* value of n we will find it to be true that

$$a\times\sum_{k=1}^{n-1}q^k=a\times\frac{q-q^n}{1-q}$$

using the usual rules of arithmetic we are all taught as children. The sum term in this equation is called a power series and occurs frequently in many types of physics and engineering problems.

Applying this formula with $a = 9$ and $q = 0.1$ we get

$$0.99999\cdots = 9 \times \frac{0.1 - \lim\limits_{n\to\infty}(0.1)^n}{1 - 0.1}.$$

The next question is what the term involving the limit as n goes to infinity equals. Here 'common sense' tells us $\lim\limits_{n\to\infty}(0.1)^n \to 0$ and in this case mathematics agrees with 'common sense'. Thus,

$$0.99999\cdots = 9 \times \frac{0.1}{0.9} = \frac{0.9}{0.9} = \frac{0.9}{0.9} \times \frac{10}{10} = \frac{9}{9} = 1.$$

Put into plain English, what the Analysts tell us is that the *definition* of $0.99999\cdots$ is completely equivalent to 1. It is an example of *mathematical continuity* as worked out by Weierstrass.

At this point many of us who are not mathematicians might be inclined to think, "Ah! The trick is that the terms in the sum go to zero as n becomes unboundedly large!" This is quite a natural reaction at this point but here the Analysts might respond, "Whoa now, not so fast." Let us consider the sum *ad infinitum*

$$1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \cdots = \sum_{n=1}^{\infty} \frac{1}{n}.$$

Here again 'common sense' tells us $\lim\limits_{n\to\infty}(1/n) \to 0$, and the Analysts will tell us, "This is so." *But* the infinite *sum* in this case does not converge to a finite value; instead it 'blows up' to ∞. What matters is not merely that the terms in the sum individually go to zero; what matters is, in the words of mathematician Rózsa Péter[11], "how *fast* they go to zero."

These examples all involve not so much 'numbers' as they do *processes* (or, as the mathematicians might prefer us to say, *operations*). Most scientists tend to take these routine operations for granted even when induction *ad infinitum* is involved. Mathematicians, by the nature of their profession, are typically much more aware of the sorts of things that can go wrong with induction and much less inclined to take operations that proceed *ad infinitum* for granted.

As objects mathematical operations are *Unsache*-things. Mathematicians deal successfully with 'problems' like those illustrated here because mathematical objects are *precisely definable*. This is an advantage the 'mathematical world' enjoys that is not shared with or to be found in the 'physical' sciences. A physical object cannot be defined; rather, it is *explained*. One's understanding of a physical object is, at any moment in time, *contingent*. We do not know if or

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[11] Rózsa Péter, *Playing With Infinity*, Z.P. Dienes (tr.), N.Y.: Dover Publications, 1976.

when a new 'experience' will knock our explanation of a physical object off its base. This is nowhere more true than when we are dealing with ontological explanations of objects arising from inferences of induction that pass beyond actual sensible experience. Here it is not even a question of induction *ad infinitum*; merely going beyond the scope of present experience in the explanation of an object puts the contingency of our knowledge into play. In Chapter 17 we saw Kant's statement that mathematical concepts ('arbitrarily made concepts') can be and are defined. The situation is different when it comes to empirical objects.

> A definition is a sufficiently distinct and precise concept (*conceptus rei adaequatus in minimis terminis, complete determinatus*[12]).
>
> The definition alone is to be regarded as a logically perfect concept, for in it are united the two essential perfections of a concept: distinctness, and completeness and precision in distinctness (Quantity of distinctness).
>
> Since the synthesis of empirical concepts is not arbitrary but rather is empirical and as such can never be complete (because one can always discover in experience more marks of the concept), empirical concepts likewise cannot be defined.
>
> Not all concepts *can* be defined, and not all *need* to be.
> There are approximations to the definition of certain concepts; these are partly *expositions* (*expositiones*), partly *descriptions* (*descriptiones*).
> The *expounding* of a concept subsists in the (successive) suspended representation of its marks so far as these are found through analysis.
> *Description* is the exposition of a concept insofar as it is not precise [KANT8a: 631-633 (9: 140-143)].

Science is required to make predictive speculations. A scientist can avoid the risk of being *wrong* by refusing to do so, but then, as a scientist, he will never be *right* about anything either. Every scientific prediction goes beyond what we already know, thus is a synthesis, and this act involves an inference of judgment – often an inference of induction. If the inference of induction were part of the functional capacity of determining judgment it could never fail to describe Nature (because thinking must conform to its own laws). But inferences of judgment do not belong to the capacity for determining judgment and instead belong to subjective reflective judgment. They are part of the teleological function of judgmentation and serve to make a system of Nature. If mathematics, which enjoys the luxury of dealing only with defined objects, finds it necessary to devote part of its doctrine to fixing problems that arise from induction, how much less must the degree of certainty in an inference of induction be for the physical sciences? 'Common sense' or 'intuition' in this activity is no sure guide, as the examples above illustrate.

> Induction makes general what one knows of the things of a species and extends it (synthetically)

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[12] A concept adequate to the thing, in minimal terms, completely determined.

> to things of the same species that one does not know [AK16: 756].
>
> Induction infers *a particulari ad universali*[13] according to the principle of generalization. Through induction one develops general, not universal, propositions [AK16: 757].

The power of going from particulars to a general representation belongs to reflective judgment. Therefore induction always has none but subjective validity. If every bird I have seen has feathers I might well conclude, "All birds have feathers." Likewise, if everything I have seen that flies is a bird I might conclude by induction, "Everything that flies is a bird." In this case I am in for a bit of a shock when I see an ostrich (which cannot fly) or a bat (which flies but has no feathers).

Scientific Generalizations

Scientific generalizations are, of course, based on a good deal more experience than these simple examples. Experiments in high-energy physics led to the postulate in 1963 that hadron particles (e.g. protons and neutrons) were composed of more elementary entities called quarks.[14] At first the theory arose as a sort of classification system for categorizing the vast zoo of subatomic particles experimenters were observing in the products of high-energy collisions produced in particle accelerators. Initially the theory called for three 'species' of quarks, named 'up', 'down', and 'strange'. These names reflected the three types of quantum numbers needed for the theory. Later on it was found that three types of quarks (and their 'antiquarks') were not enough. The following quote gives us something of the flavor of the thinking that went into the development of present day quark theory:

> A serious problem with the idea that baryons[15] are composed of quarks was that the presence of two or three quarks of the same kind in a particular particle (for instance, two 'up' quarks in a proton) violates the exclusion principle. Quarks ought to be subject to this principle since they are fermions with spins of ½. To get around this problem, it was suggested that quarks and antiquarks have an additional property of some kind that can be manifested in a total of six different ways, rather as electric charge is a property that can be manifested in the two different ways that have come to be called positive and negative. In the case of quarks, this property became known as 'color,' and its three possibilities were called red, green, and blue. The antiquark colors are antired, antigreen, and antiblue.[16]

The 'color' of a quark is an analogy to the 'charge' of an electron or proton. It is a 'property' every type of quark is supposed to possess. But even adding the 'color' property to the existing trio of quarks (and the three antiquark 'companions') did not provide enough degrees of freedom to fit all the experimental data. Eventually three more species of quarks were added; they are

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[13] from the particular to the general

[14] Murray Gell-Mann and, independently, George Zweig proposed the quark hypothesis in 1963. Gell-Mann was awarded the Nobel Prize in physics in 1969 for his work.

[15] Hadrons are subdivided into two sub-classes called baryons and mesons.

[16] Arthur Beiser, *Concepts of Modern Physics* (5th ed.), NY: McGraw-Hill, 1995, pp. 492-3.

called the ‘charm’, ‘top’, and ‘bottom’ quarks. So now we have a total of six types of quarks.

Murray Gell-Mann, who deserves the title of ‘the father of quark theory,’ was famously wary of the philosophical issues posed by quarks. He more or less regarded quarks as a means to construct a ‘field theory’ for subatomic particles and it is this ‘field theory’ (quantum chromodynamics or QCD) which, in the terminology of this treatise, stands as the *noumenon* uniting the various subatomic phenomena being seen by the experimentalists at the horizon of (presently) possible experience.[17] Gell-Mann’s theory presents us with what is one of the best scientific examples of the construction of scientific *noumena* we have today, and so we will spend a bit of time describing it. In the next section we will put this theory into the broader context of Kant-Margenau construction.

Quantum chromodynamics is the theory of what is called ‘the strong force’ – one of the four acknowledged ‘fundamental interactions’ at the bedrock of modern physics. Of the four fundamental interactions (gravity, the weak force, the strong force, and electromagnetism) the strong force is the oddest duck in the pond. The ‘strength’ of the ‘strong force’ *increases* as the ‘distance between particles’ (quarks) increases. This is in stark contrast to gravity and electromagnetism, where interaction ‘force’ drops off by the square of the distance. The effective range of the strong force is on the order of 10^{-15} meters, which corresponds nicely with the so-called classical radius of the proton. At this range quarks are ‘loosely bound’ – what physicists call the “asymptotic freedom” property of quarks. In order to describe the strong interaction taking place between nucleons Gell-Mann used an analogy with quantum electrodynamics. The ‘color’ property of quarks is likened to electric charge (“color charge”) and a mediating boson, the gluon, is postulated for mediating the interaction. The gluon is the analogous counterpart to the role the photon plays in QED theory. The theory calls for eight types of gluons, each carrying a ‘color charge’ and an ‘anticolor charge’.

Neither the quark nor the gluon have ever been directly observed in any experiment. However, Gell-Mann was able to use his theory to predict a new kind of particle, called the ‘omega minus’ particle, and, more importantly, he was able to tell experimenters exactly how to look for it. The omega minus particle was confirmed by experiment in 1964 and Gell-Mann’s Nobel Prize followed five years later. Thus, as a *noumenon* the quark-gluon theory has a

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[17] If it strikes you as in any way jolting that I have here called a *theory* a noumenon, you are experiencing the cognitive dissonance that comes from keeping up an ontology-centered habit of thinking. A concept of an Object is a rule, and a rule is an assertion made under a general condition. From an epistemology-centered point of view *every* noumenon *at the horizon of possible experience is a rule of a function that unites the concepts standing under it*. I call this Murphy’s dictum, named after a former student of mine, Keelan Murphy, who first described the idea of a *noumenon* in this way. Murphy’s dictum prescribes the *practical* basis for *noumenal* Objects.

reasonably solid standing in science. The ontological standing of quarks and gluons as separate 'entities in their own right' is more problematical (since neither have been 'individually' observed). A prediction that has come out of QCD is the prediction that, because they carry 'color charges', gluons should be able to interact to form 'glueballs'. No glueball has yet been found by experimenters. On the other hand, studies of high-energy electron scattering by protons appear to show three pointlike concentrations of electric charge within the proton, which is in agreement with the quark theory.

Even without presenting the mathematics here, it is probably clear to the reader that QCD is far from being a "simple idea" such as we might have expected to see based on the illustration in figure 24.4.1. If what, for the sake of brevity, we may call the 'strong field' stands as *noumenon* in a diagram like figure 24.4.1, where do all these quarks, gluons, color charges, etc. contained *in* the idea of the strong field fit into the picture? Are they *noumena* floating beyond the horizon of possible experience? If so, that would make them things-as-we-cannot-know-them (transcendental illusions), a characterization that would not sit well with physicists. If not, do they stand under the idea of the strong field? The answer here must be 'no.' None of these ideas are concepts containing actual sensational matter. A quick glance at figure 24.4.1 tells us that quarks, etc. cannot stand *under* the idea of the strong field for this reason. All that seems to be left to us is that these constructs must somehow be viewed as 'being inside the circle' that represents the idea of the strong field in a figure like 24.4.1. But what could this possibly mean? It means they are contained *in* the idea of the *noumenon*. I will expand this explanation in the next section. But before going on to that discussion it is worthwhile and interesting to describe a very recent finding made by a team of experimentalists at Brookhaven National Laboratory.

Ever since the 'standard model' arising out of QCD was linked (by speculation) to Big Bang cosmology, most theorists have presumed that within the first 10 microseconds (ten millionths of a second) after the 'creation of the universe' things had altogether too much kinetic energy for quarks to bind themselves together to form protons and neutrons. Theorists pictured this 'primal universe' as a 'gas' of quarks.[18] If I have correctly understood what physicists have told me, this 'gas model' is not an immediate consequence of the QCD theory but, rather, is an assumption made to simplify the mathematics enough to yield usable calculations. What the Brookhaven scientists have been able to do is collide ions of gold nuclei together with sufficient energy to briefly 'liberate' quarks from their confinement within the protons and neutrons of these nuclei. This did not permit them to 'capture' individual quarks or gluons but it did enable them to produce "hot, dense bursts of matter and energy" that could be detected by advanced instruments

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[18] Presumably it is clear to you that this 'picture' is ontology-centered. It treats quarks as primal entities.

built into the collider at Brookhaven.[19]

Nothing seen so far in the outcomes of this experiment has directly contradicted the basics of QCD theory. However, to everyone's surprise the results flatly contradicted the presupposition of a quark-gluon "gas." Instead, the results are much more consistent with a model of a quark-gluon "liquid." More interesting still, this 'liquid' seems to have no or almost no viscosity, making it (in the words of Riordan and Zajc), "probably the most perfect liquid ever observed."

Why were the physicists surprised by this outcome? Did they not compute the expected consequences prior to running the experiment? The answer here is 'no but with good reason.' It turns out that 'exact calculations' based on QCD fundamentals are presently impractical to carry out even using the largest, fastest, supercomputers dedicated to this sort of computation. It is because of this that the theorists employed a bit of inference of analogy and a bit of inference of induction to obtain a 'simpler picture' of the physics, so as to be able to form expectations for what sort of phenomena the experiment should look for. *This* process of inference, which treated quarks and gluons as transcendent entities, led them to predictions that laboratory experience has slapped in the face. Theorists are now looking for a way to 'fuse' conventional QCD theory with a theory of 'strings' in order to put together a tractable mathematical explanation.

§ 4.3 Slepian Quantities

At the end of §4.2 we left hanging the question of how quarks, gluons, etc. 'fit into' the diagram of figure 24.4.1. We will partly resolve this issue now and finish it in §7. The starting point for this is a brief review of something we talked about in Chapter 21, namely the situation that now exists regarding the relationship between the Calculus and physical science.

Newton had used the idea of absolute quantities to provide a *metaphysical* ground for his "method of first and last ratios of quantities". Using $f(t) = t^3$ as our example function, his method yields as the first derivative of this function the expression

$$\dot{f}(t) = \lim_{h \to 0} \frac{6ht^2 + 2h^3}{2h} = \lim_{h \to 0} 3t^2 + h^2 \to 3t^2$$

The objectionable step, so far as mathematics in his day was concerned, was the 0/0 operation that happens when the 'infinitesimal' quantity h 'arrives at' 0 in the limiting process. There are two ways by which the "problem of infinitesimals" can be approached. The modern way is the argument from continuity as this was formalized by Weierstrass in the 19th century. Weierstrass'

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[19] For an accessible description of this experiment see Michael Riordan and William A. Zajc, "The first few microseconds," *Scientific American*, vol. 294, no. 5, May, 2006, pp. 34-41.

formalization is regarded as settling the issue by most modern mathematicians. The alternative way of looking at the problem is presented by the 'non-standard analysis' of Robinson[1]. We are, however, now in a position to see that the continuity argument is an inference of induction. In effect it is the same as saying nothing unexpected happens as h becomes infinitesimally close to zero and, by proxy, no 'discontinuity' will happen at $h = 0$.

But how do we know this? It is one thing for mathematics to define its objects in such a way that continuity holds. It is something else altogether to say that such a mathematical rule applies to natural phenomena with necessity. Newton knew this, and this is where his absolute quantities came into play. He argued that 0/0 may be undefined *mathematically* but *physically* the result *can* be defined and it is this: the limit converges to the absolute quantity. This, too, is an inference of induction, but if we use Newton's geometrical diagrams it is an inference that seems 'obvious' and 'self-evident' intuitively. We remember that in Newton's day geometry (there was only one geometry then) was the foundation of mathematics, and the professional mathematicians of the day (who, it is to be remembered, were also 'natural philosophers') accepted Newton's argument. The towering success of Newton's new physics seemed to confirm his metaphysical argument and it was an easy step to argue *that its consequences proved the correctness of the premise*.

George Berkeley did not agree.

> Nothing is plainer than that no just conclusion can be directly drawn from two inconsistent suppositions. You may indeed suppose anything possible; but afterwards you may not suppose anything that destroys what you first supposed: or, if you do, you must begin *de nova*. If therefore you suppose that the augments vanish, i.e. that there are no augments, you are to begin again and see what follows from such supposition. But nothing will follow to your purpose. You cannot by that means ever arrive at your conclusion, or succeed in what is called by the celebrated author[2], the investigation of the first or last proportions of nascent and evanescent quantities, by instigating the analysis in finite ones. I repeat it again: you are at liberty to make any possible supposition: and you may destroy one supposition by another: but then you may not retain the consequences, or any part of the consequences, of your first supposition so destroyed. I admit that signs may be made to denote either anything or nothing: and consequently that in the original notation $x + o$, o might have signified either an increment or nothing. But then, which of these soever you make it signify, you must argue consistently with such its signification, and not proceed upon a double meaning: which to do is a manifest sophism. Whether you argue in symbols or in words the rules of right reason are still the same. Nor can it be supposed you will plead a privilege in mathematics to be exempt from them [BERK2: 27].

There is no doubt which of the two men, Newton or Berkeley, carried the weight of opinion in the community of scientists. But from one point of view mathematics later did make a concession to Berkeley's objections. Weierstrass' epsilon-delta method does specify that epsilon and delta are both to be positive (non-zero) quantities. Nonetheless, one finds it hard to deny Newton aimed to

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[1] Abraham Robinson, *Non-standard Analysis*, revised edition, Princeton, NJ: Princeton University Press, 1996.

[2] i.e., Newton.

provide for physics the same apodictic certainty in mechanics that everyone in his day was sure held for mathematics. In this he was as much rationalist as empiricist, as much Platonist as Aristotelian, in his science.

But all this changed with Einstein and the relativity theory. Gone now were the unknowable absolute quantities of Newton; they were no longer needed by a relativistic physics that prescribed rules of form that valid equations of physics were to follow. Even before this, gone too was the apodictic certainty of mathematics that had been drawn from Euclidean geometry, defeated by the discovery of non-Euclidean geometries and the mathematical 'monsters' of the 19th century born when analysis outran intuition in geometry.

Of course, Einstein's physics itself made use of Newton's Calculus. But if relativity had cut loose the cord of absolute quantities by which the Calculus had claimed to make pronouncements on nature, would this not also fall under Berkeley's censure? Of course it would. Positivism chooses to ignore such a 'philosophical' issue, but he who is not a positivist must face it. How now stand mathematics and physical science in relationship to one another?

Poincaré was one who saw the handwriting on the wall. Even before the first installment of Einstein's theory he wrote:

> Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty. These are two points that cannot be questioned. But then, if experiment is everything, what place is left for mathematical physics? What can experimental physics do with such an auxiliary – an auxiliary, moreover, which seems useless and may even be dangerous?
>
> However, mathematical physics exists. It has rendered undeniable service, and that is a fact that has to be explained. It is not sufficient merely to observe; we must use our observations, and for that purpose we must generalize. This is what has always been done, only as the recollection of past errors has made man more and more circumspect, he has observed more and more and generalized less and less. Every age has scoffed at its predecessor, accusing it of having generalized too boldly and too naively. . . and no doubt some day our children will laugh at us. Is there no way of getting at once to the gist of the matter, and thereby escape the raillery which we foresee? Cannot we be content with experiment alone? No, that is impossible; that would be a complete misunderstanding of the true nature of science. The man of science must work with method. Science is built up of facts, as a house is built of stones; but an accumulation of facts is no more a science than a heap of stones is a house. Most important of all, the man of science must exhibit foresight. . .
>
> It is often said that experiments should be made without preconceived ideas. That is impossible. Not only would it make every experiment fruitless, but even if we wished to do so, it could not be done. Every man has his own conception of the world, and this he cannot so easily lay aside. We must, for example, use language, and our language is necessarily steeped in preconceived ideas. Only they are unconscious preconceived ideas, which are a thousand times the most dangerous of all. Shall we say, that if we cause others to intervene of which we are fully conscious, that we shall only aggravate the evil? I do not think so. I am inclined to think that they will serve as ample counterpoises – I was almost going to say antidotes. They will generally disagree, they will enter into conflict one with another, and *ipso facto*, they will force us to look at things under different aspects. That is enough to free us. He is no longer a slave who can choose his master.
>
> Thus, by generalization, every fact observed enables us to predict a large number of others; only, we ought not to forget that the first alone is certain, and that all the others are merely probable.

However solidly founded a prediction may appear to us, we are never *absolutely* sure that experiment will not prove it to be baseless if we set to work to verify it. But the probability of its accuracy is often so great that practically we may be content with it. It is far better to predict without certainty than never to have predicted at all. We should never, therefore, disdain to verify when the opportunity presents itself. . . Every experiment must enable us to make a maximum number of predictions having the highest possible degree of probability. . . I may be permitted to compare science to a library which must go on increasing indefinitely; the librarian has limited funds for his purchases, and he must, therefore, strain every nerve not to waste them. Experimental physics has to make the purchases, and experimental physics alone can enrich the library. As for mathematical physics, her duty is to draw up the catalog. If the catalog is well done the library is none the richer for it; but the reader will be enabled to utilize its riches; and also by showing the librarian the gaps in his collection, it will help him to make a judicious use of his funds, which is all the more important inasmuch as those funds are entirely inadequate. This is the role of mathematical physics. It must direct generalization, so as to increase what I called just now the output of science. . .

Every generalization is a hypothesis. Hypothesis therefore plays a necessary role, which no one has ever contested. Only, it should always be as soon as possible submitted to verification. It goes without saying that, if it cannot stand this test, it must be abandoned without any hesitation. This is, indeed, what is generally done; but sometimes with a certain impatience. Ah well! this impatience is not justified. The physicist who has just given up one of his hypotheses should, on the contrary, rejoice, for he found an unexpected opportunity of discovery. His hypothesis, I imagine, had not been lightly adopted. It took into account all the known factors which seem capable of intervention in the phenomenon. If it is not verified, it is because there is something unexpected and extraordinary about it, because we are on the point of finding something unknown and new. Has the hypothesis thus rejected been sterile? Far from it. It may even be said that it has rendered more service than a true hypothesis. Not only has it been the occasion of a decisive experiment, but if this experiment had been made by chance, without the hypothesis, no conclusion could have been drawn; nothing extraordinary would have been seen; and only one fact the more would have been cataloged, without deducing from it the remotest consequence.

Now, under what conditions is the use of hypothesis without danger? The proposal to submit all to experiment is not sufficient. Some hypotheses are dangerous, – first and foremost those which are tacit and unconscious. And since we make them without knowing them, we cannot get rid of them. Here again, there is a service that mathematical physics may render us. By the precision which is its characteristic, we are compelled to formulate all the hypotheses that we would unhesitatingly make without its aid. Let us also notice that it is important not to multiply hypotheses indefinitely. If we construct a theory based upon multiple hypotheses, and if experiment condemns it, which of the premises must be changed? It is impossible to tell. Conversely, if experiment succeeds, must we suppose that it has verified all these hypotheses at once? Can several unknowns be determined from a single equation?

It might be asked, why in physical science generalization so readily takes the mathematical form. The reason is now easy to see. It is not only because we have to express numerical laws; it is because the observable phenomenon is due to the superposition of a large number of elementary phenomena which are *all similar to each other*; and in this way differential equations are quite naturally introduced. It is not enough that each elementary phenomenon should obey simple laws; all those that we have to combine must obey the same law; then only is the intervention of mathematics of any use. Mathematics teaches us, in fact, to combine like with like. Its object is to divine the result of a combination without having to reconstruct that combination element by element. If we have to repeat the same operation several times, mathematics permits us to avoid this repetition by a kind of induction. . . But for that purpose all these operations must be similar; in the contrary case we must evidently make up our minds to working them out in full one after the other, and mathematics will be useless. It is therefore, thanks to the approximate homogeneity of the matter studied by physics, that mathematical physics came into existence. In the natural sciences the following conditions are no longer to be found: – homogeneity, relative independence of remote parts; simplicity of the elementary fact; and that is why the student of natural science is compelled to have recourse to other modes of generalization [POIN1: 140-159].

I quote Poincaré at length here because he puts his hand on several key factors that go to the heart of method in science. The role for mathematics he described here is immanently *practical*. In his division between 'experimental physics' and 'mathematical physics' we can see and may affirm ***a logical division of the method of science into two parts***. There is what one may call 'the physical world' and what one may call 'the mathematical world.' Poincaré points us to seeing this division and describes the different roles played by each in the *unity* of the science. When science is brought to bear upon objects – as it must be by the nature of its aim – his logical distinction should be the occasion for us to remember Kant's distinction between sensible (physical) objects and intelligible objects. 'Experimental physics' (science of experience) and 'mathematical physics' (doctrine of method *for understanding*) are complements and both are needed if through science we aim *to comprehend Nature*. It is with the details of this *practical partnership* that this chapter of this treatise is concerned.

Within the professional organization named The Institute for Electrical and Electronic Engineers there is a society named the Information Theory Society. We recall that the science of information theory has the peculiarity that its topic, information, is a supersensible Object. Once a year the ITS bestows its most prestigious award, the Shannon Award, in recognition of persons whose work is acknowledged to have made broad and significant contributions to the science. It is this Society's highest honor, its modest equivalent of the Nobel Prize in the physical sciences. In 1974 one of the recipients was David Slepian, and he chose for the topic of his Shannon Lecture a subject that pertains directly to our present topic at hand.[3]

In signal processing theory there was a long-standing paradox known as the Bandwidth Paradox. Briefly, the paradox is this. There are good physical reasons to think that real signals do not possess an infinite range of frequencies; but if this is so there are unassailable mathematical reasons to conclude that such signals must be unlimited in duration – that is, they can neither begin nor end in objective time. Contrariwise, any signal of finite duration in time must be unlimited in its frequency content. Both conclusions are regarded as absurd, and this is the Bandwidth Paradox.

> My starting point is to recall to you that each of the quantitative physical sciences . . . is comprised of an amalgam of two *distinctly different* components. That these two facets of each science are indeed distinct from one another, that they are made of totally different stuff, is rarely mentioned and certainly not emphasized in the traditional college training of the engineer or scientist. Separate concepts from the two components are continuously confused. In fact, we even lack a convenient language for keeping them straight in our thinking. I shall call the two parts Facet A and Facet B.

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[3] Slepian's lecture was reprinted as: David Slepian, "On bandwidth," *Proceedings of the IEEE*, vol. 64, no. 3, Mar. 1976, pp. 292-300.

> Facet A consists of observations on and manipulations of the "real world." Do not ask me what this real world is: my thoughts become hopelessly muddled here. . . For the electrical engineer, this real world contains oscilloscopes and wires and voltmeters and coils and transistors and thousands of other tangible devices. . .
>
> Facet B is something else again. It is a mathematical model and the means for operating with this model. It consists of papers and pencils and symbols and rules for manipulating the symbols. It also consists of the minds of the men and women who invent and interpret the rules and manipulate the symbols, for without the seeming consistency of their thinking processes there would be no single model to consider. . .
>
> Now as you all know, we like to think that there is an intimate relationship between Facet A and Facet B of a given science. . . I have carefully said that we "like to think" there is an intimate relationship between the facets because in fact, under closer scrutiny one sees the correspondence tenuous, most incomplete, and imprecise. There is a myriad of detail in the laboratory ignored in the model. Worse yet, many key parts of the model – many of its concepts and operations – have no counterpart in Facet A.[4]

We can recognize the meaning of Slepian's 'Facet A' as sensible Nature; it is what we normally call the "physical world". His facet B corresponds to what Kant called "the intelligible world"; we may call it the "mathematical world." Facet A has what we might call an Aristotelian character; Facet B has a Platonic character. Figure 24.4.4 illustrates Slepian's facets. He went on to say:

> Our mathematical models are full of concepts, operations, and symbols that have no counterpart in Facet A. Take the very fundamental notion of a real number, for instance. In Facet B certain symbols take numerical values that are supposed to correspond to the readings of instruments in Facet A. Almost always in Facet B these numerical values are elements of the real number continuum, the rationals *and irrationals*. This latter sort of number seems to have no counterpart in Facet A. In Facet B, irrational numbers are defined by limiting operations or Dedekind cuts - mental

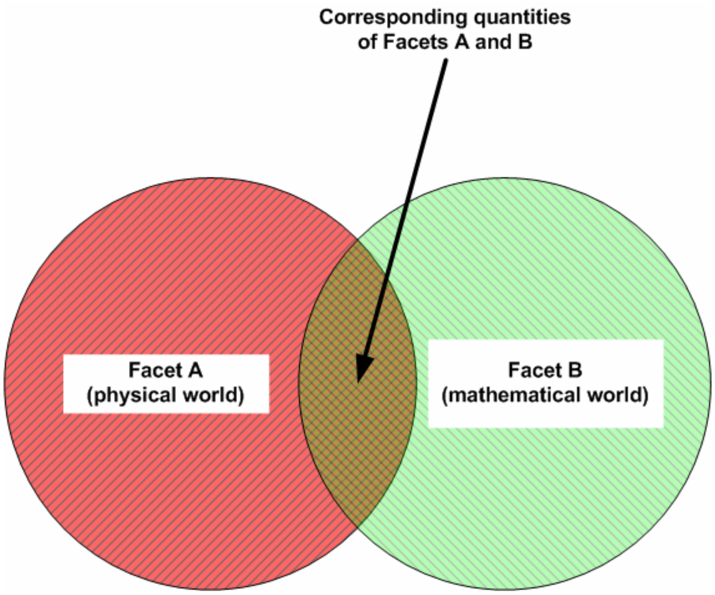

Figure 24.4.4: Slepian's Two-facets Concept

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[4] *ibid.*

> exercises that with some effort and practice we can be trained to "understand" and agree upon. After years of experience with them, we theoreticians find them very "real," but they do not seem to belong to the real world of Facet A. *The direct result of every instrument reading in the laboratory is a finite string of decimal digits – usually fewer than 6 – and a small integer indicating the exponent of some power of 10 to be used as a factor*. Irrationals just cannot result directly from real measurements, as I understand them.
>
> Now there are several ways in which we can handle this fundamental lack of correspondence between symbol values in Facet B and measurements in Facet A. We could build a mathematical model in which only a finite number of numbers can occur, say those with 10 significant digits and one of a few hundred exponents. Differential equations would be replaced by difference equations, and complicated boundary conditions and rules would have to be added to treat the roundoff problem at every stage. The model would be exceedingly complex. Much simpler is the scheme usually adopted and known to you all. We admit the real-line continuum into Facet B and we impose yet another abstraction – continuity. In the end, if the model says the voltage is π, we are pleased if the meter in Facet A reads 3.1417. We work with the abstract continuum in Facet B, and we round off to make the correspondence with Facet A.
>
> Mathematical continuity deserves a few words. It is another concept with no counterpart in the real world. It makes no sense at all to ask whether *in Facet A* the position of the voltmeter needle is a continuous function of time. Observing the position of the needle at millisecond or microsecond or even picosecond intervals comes no closer to answering the question than does measurement daily or annually. Yet continuity is a vital concept for Facet B. By invoking it, by demanding continuous solutions of the equations of our models, we make the parts of the model that correspond to measurements in Facet A insensitive to small changes in the parts of the model that do not correspond to anything in Facet A. Specifically, continuity means that the first five significant digits in our computed answers, those to which we do ultimately attribute real significance, will depend only weakly on the sixth to tenth significant digits of the numbers we assign to the parameters of the model. They will be essentially independent of the 100th or 1000th significant digit – constructs of importance to the working of Facet B but with no meaningful counterpart in Facet A.[5]

This idea of **correspondence** between sensible experience (phenomena in 'Facet A') and intelligible constructs (*noumena* of 'Facet B') is the keystone of Slepian's thesis. Within 'Facet B' some constructs *can be made to anticipate* possible sensible experience; these constructs are symbolized in figure 24.4.4 by the part of 'Facet B' that overlaps with 'Facet A'. Other constructs in 'Facet B' have no such possible correspondence, and these are symbolized by the part of 'Facet B' that does not overlap 'Facet A'. As for the part of 'Facet A' that overlaps no part of 'Facet B', these constituents of 'Facet A' are those for which no theory has been presented. Slepian names his two types of constructs in 'Facet B' **principal quantities** and **secondary quantities**, respectively.

> The situation just exemplified by this discussion of numbers and continuity occurs in many different guises in the sciences. There are certain constructs in our models (such as the first few significant digits of some numerical variable) to which we attach physical significance. That is to say, we wish them to agree quantitatively with certain measurable quantities in a real-world experiment. Let us call these the *principal quantities* of Facet B. Other parts of our models have no direct meaningful counterparts in Facet A but are mathematical abstractions introduced into Facet B to make a tractable model. We call these *secondary constructs* or *secondary quantities*. One can, of course, consider and study any model that one chooses to. It is my contention, however, that a necessary and important condition for a model to be *useful* in science is that the *principal quantities*

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[5] *ibid*.

> *of the model be insensitive to small changes in the secondary quantities*. Most of us would treat with great suspicion a model that predicts stable flight for an airplane if some parameter is irrational but predicts disaster if that parameter is a nearby rational number. Few of us would board a plane designed from such a model.[6]

We can refer to the principle Slepian states above as **Slepian's indistinguishability criterion**. At this point in his lecture Slepian returned to signal theory and provided examples of the indistinguishability criterion.

> I have already commented on the lack of precise correspondence between signals in Facet B and Facet A. Since small enough changes in the signals of the model are not to affect quantities with meaning in Facet A, it seems natural to attempt to make the correspondence many-to-one. We wish to say two Facet B signals correspond to the same Facet A signal if they are enough alike in form. If they do correspond to the same Facet A signal, we shall also say they are "really indistinguishable." But what should we take for this criterion of indistinguishability? The energy of the difference, $E[s_1 - s_2]$, of course.[7] Thus we adopt the Facet B definition:
>
> Two signals, $s_1(t)$ and $s_2(t)$, are *really indistinguishable at level* ε if
>
> $$E[s_1(t) - s_2(t)] \equiv \int_{-\infty}^{\infty} [s_1(t) - s_2(t)]^2 \, dt \le \varepsilon.$$
>
> Thus if, in the real world, we cannot measure the energy of the difference of the corresponding signals, the signals must be considered "the same." Notice that, at level ε, $s_1(t)$ may be really indistinguishable from $s_2(t)$, and $s_2(t)$ may be really indistinguishable from $s_3(t)$, while $s_1(t)$ and $s_3(t)$ are not really indistinguishable from one another.[8]

We can see in Slepian's idea of a "level ε" the correspondence with our earlier discussion of the limit of sensation (i.e. a degree of intensive magnitude too small to perceive). It matters not at all that we are speaking here of *instrumented* sensibility because scientific instruments 'extend the range' of our senses. What does matter is that we know what we're talking about when we speak of 'instrumented sensibility.' One should note how the words 'meaning' and 'meaningful' recur throughout Slepian's lecture. After giving his new definition of 'bandwidth' in terms of the indistinguishability criterion, Slepian goes on to say,

> Note that with these definitions, doubling the strength of a signal may well increase its bandwidth. Similar remarks hold for the time duration of a signal. A consequence of these definitions is that *all* signals of finite energy are both bandlimited to some finite bandwidth W and timelimited to some finite duration T.[9]

A further consequence of the indistinguishability criterion is that, with advancing capabilities in scientific instrumentation, what was once a secondary quantity of Facet B can become a principal

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[6] *ibid.*

[7] The idea of the "energy" of a signal is borrowed (by analogy) from physics. It is defined as the time integral of the square of the signal. We should probably call this quantity "mathematical energy" since the concept is always applied to Facet B signals in signal processing theory.

[8] *ibid.*

[9] *ibid.*

quantity when instrumentation becomes ‘sensitive enough’ to lower “level ε” in Facet A. We can symbolically imagine this by picturing the Facet A ‘sphere’ in figure 24.4.4 expanding to cover more of the Facet B ‘sphere’. This is equivalent to ‘extending the horizon of possible experience’ in our earlier discussion.

A “quantity in Facet B” need not be a signal nor a mathematical function. More generally it is any kind of mathematical construct. In his biography of Richard Feynman, James Gleick wrote:

> There was a reality problem, distinctly more intense than the problem posed by more familiar entities such as electrons. . . Gell-Mann was wary of the philosophical as well as the sociological problem created by any assertion one way or the other about quarks being real. For him quarks were at first a way of making a simple toy field theory: he would investigate the theory’s properties, abstract the general principles, and then throw away the theory. “It is fun to speculate about the way quarks would behave if they were physical particles of finite mass (instead of purely mathematical entities as they would be in the limit of infinite mass),” he wrote.[10]

Although physicists today are in the habit of thinking of quarks as ‘physical particles’ the plain fact is that we cannot yet make any measurement that permits us to “see” quarks individually. What we can measure are consequences of what, for want of a better term, I will call “the QCD field.” Using Slepian’s language, the QCD field is a principal quantity; quarks and gluons are still in the shadows of secondary quantities in the Facet B of quantum chromodynamics. This is not to say that they will never be principal quantities; ‘never’ is a long time. It is to say that they are not *yet* principal quantities. We presently have no object of experience in sensible Nature to which they correspond. We *do* have Facet A phenomena anticipated by the QCD field.

Nor do I mean that when the day comes (if it comes) where instrumentation advances to the point where a Facet A correspondent to the idea of a quark is obtained, that this by itself will make everything *in* the idea of a quark a principal quantity. The idea of an electron is a principal quantity today with measurable anticipants in Facet A. But not everything we can think about an electron is a principal quantity. Perhaps the best example is the puzzle of ‘the electronic mass’ and ‘electron radius’. In his famous “Feynman Lectures” Feynman derived for his students the result that ‘electronic mass’ is inversely proportional to ‘electron radius’ when the electron is modeled as a little sphere of charge [FEYN4: Ch. 28, 1-3]. This means that if the electron is taken to be a ‘point charge’ it must have infinite mass (which measurements show it does not). But if the radius is taken to be non-zero then we run into another problem.

> Now if we have a sphere of charge, the electrical forces are all repulsive and an electron would tend to fly apart. Because the system has unbalanced forces, we can get all kinds of errors in the laws relating energy and momentum. To get a *consistent* picture, we must imagine that something holds the electron together. The charges must be *held* to the sphere by some kind of rubber bands –

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[10] James Gleick, *Genius*: *The Life and Science of Richard Feynman*, NY: Pantheon Books, 1992.

> something that keeps the charges from flying off. It was first pointed out by Poincaré that the rubber bands – or whatever it is that holds the electron together – must be included in the energy and momentum calculations. For this reason the extra nonelectrical forces are also known by the more elegant name "Poincaré stresses." If the extra forces are included in the calculations, the masses obtained in two ways are changed (in a way that depends on the detailed assumptions). . .
>
> Clearly, as soon as we have to put forces on the inside of the electron, the beauty of the whole idea begins to disappear. Things get very complicated. You would want to ask: How strong are the stresses? How does the electron shake? Does it oscillate? What are all its internal properties? And so on. It might be possible that an electron does have some complicated internal properties. If we made a theory of the electron along these lines, it would predict odd properties, like modes of oscillation, which apparently haven't been observed. We say "apparently" because we observe a lot of things in nature that still do not make sense. We may someday find out that one of the things we don't understand today (for example, the muon) can, in fact, be explained as an oscillation of the Poincaré stresses. It doesn't seem likely, but no one can say for sure. There are so many things about fundamental particles that we still don't understand [FEYN4: Ch. 28, pp. 4-5].

The point here is that some mathematical ideas, like 'electron radius', are linked by theory to other ideas (like 'electronic mass'). When we introduce a secondary quantity (electron radius) in a theory and this introduction has implications for a principal quantity (mass of an electron), then Slepian's indistinguishability criterion states that the latter must be insensitive to small changes in the former. This is not the case in regard to the classical 'electron radius' and most physicists with whom I am acquainted are content to regard the classical 'electron radius' as a meaningless concept. It is not part of 'the physical picture' in any accepted theory of physics. But, of course, this does not mean that theoreticians never *think* about these little puzzles.

> We must mention one more piece of information, which is the most annoying. There is another particle in the world called a *muon* – or μ-meson – which, so far as we can tell, differs in no way whatsoever from an electron except for its mass. It acts in every way like an electron: it interacts with neutrinos and with the electromagnetic field, and it has no nuclear forces. It does nothing different from what an electron does – at least, nothing which cannot be understood as merely a consequence of its higher mass (206.77 times the electron mass). Therefore, whenever someone finally gets the explanation of the mass of an electron, he will then have the puzzle of where a muon gets its mass. Why? Because whatever an electron does, the muon does the same – so its mass ought to come out the same. There are those who believe faithfully in the idea that the muon and the electron are the same particle and that, in the final theory of the mass, the formula for the mass will be a quadratic function with two roots – one for each particle. There are also those who propose it will be a transcendental equation with an infinite number of roots, and who are engaged in guessing what the mass of the other particles in the series must be, and why these particles haven't been discovered yet [FEYN4: Ch. 28, pg. 12].

I should mention here that the theory of quarks does not address the issue Feynman describes above. Electrons and muons are not hadrons; they are leptons and do not exhibit the strong force.

Quantum electrodynamics (QED) theory makes use of two secondary quantities that Feynman called n (the 'rest mass' of an 'ideal' electron) and j (the 'charge' of an 'ideal' electron). These quantities are used for doing calculations in QED and they are not the same as "the mass of a real electron", m, and "the charge of a real electron", e. The latter two quantities are determined from measurements and are principal quantities in physics. How n and j are used in the theory is quite informative. We shall let Feynman describe this:

> Let's see how we actually calculate *m*. We write a series of terms that is something like the series we saw for the magnetic moment of an electron: the first term has no couplings . . . and represents the ideal electron going directly from point to point in space-time. The second term has two couplings and represents a photon being emitted and absorbed. Then come terms with four, six, and eight couplings, and so on . . .
>
> When calculating terms with couplings, we must consider (as always) all the possible points where couplings can occur, right down to cases where the two coupling points are right on top of each other – with zero distance between them. The problem is, when we try to calculate all the way down to zero distance, the equation blows up in our face and gives meaningless answers – things like infinity. This caused a lot of trouble when the theory of quantum electrodynamics first came out. People were getting infinity for every problem they tried to calculate! (One should be able to go down to zero distance in order to be mathematically consistent, but that's where there is no *n* or *j* that makes any sense; that's where the trouble is).
>
> Well, instead of including all possible coupling points down to a distance of zero, if one *stops* the calculation when the distance between coupling points is very small – say 10^{-30} centimeters, billions and billions of times smaller than anything observable in experiment – then there are definite values for *n* and *j* that we can use so that the calculated mass comes out to match the *m* observed in experiments, and the calculated charge matches the observed charge, *e*. Now, here's the catch: if somebody else comes along and stops their calculation at a different distance – say, 10^{-40} centimeters – *their* values for *n* and *j* needed to get the same *m* and *e* come out *different*!
>
> Twenty years later, in 1949, Hans Bethe and Victor Weisskopf noticed something: if two people who stopped at different distances to determine *n* and *j* from the same *m* and *e* then calculated the answer to some *other* problem – each using the appropriate but different values for *n* and *j* – . . . their answers to this other problem came out nearly the same! In fact, the closer to zero distance that the calculations for *n* and *j* were stopped, the better the final answers for the other problem would agree! Schwinger, Tomonaga, and I independently invented ways to make definite calculations to confirm that is true (we got prizes for that). People could finally calculate with the theory of quantum electrodynamics.
>
> So it appears that the *only* things that depend on the small distances between coupling points are the values for *n* and *j* – *theoretical numbers that are not directly observable anyway*; everything else, which *can* be observed, seems not to be affected.
>
> The shell game that we play to find *n* and *j* is technically called "renormalization" [FEYN1: 127-128].

Here is a theory in which the principal quantities are made to be not-sensitive to secondary quantities (*n*, *j*, and the 'stopping distance' for renormalization). QED is the outstanding premier example of Slepian's indistinguishability criterion in practice.

This brings us around, finally, to the answer to the question posed earlier: namely, "where do quarks and gluons 'fit' in figure 24.4.1?" Secondary quantities *have no ontological significance*. If we call them objects at all, they are merely intelligible objects of Facet B. The Nature of their 'reality' is *mathematical* reality: they are defined mathematical objects we use to put together *the form of mathematical anticipations of experience*. Principal quantities are likewise defined mathematical objects but the difference is this: Principal quantities can be *assigned* meanings in terms of *their context for objects of experience*. Their objective validity is **practical**. In contrast, a secondary quantity has a *derivative* meaning only in its context for principal quantities. Its validity can be *logical* but it can not have real objective validity. Pure mathematical objects are not objects of sensuous experience, have no ontological moment, and have practical objective validity only insofar as principal quantities are assigned meanings from experience. If we think of 'quantities of Facet B' in our diagram of figure 24.4.1 at all, they must stay 'inside' the circles that represent concepts of objects-in-Nature; they serve only for **metaphysical** *nexus*.[11,12]

§ 5. The Discipline of Pure Reason

Human reasoning is innately dialectical. In our long journey of discovery through this treatise we have seen why this is and how it comes about. The power of pure Reason is wholly practical; it cares not for feelings nor for objects. The process of reflective judgment, from which arise all our general concepts, is wholly non-objective and judges only affectivity. Before any concept is objectively sufficient it must first be made subjectively sufficient according to the principle of the formal expedience of Nature. Only in the process of determining judgment do we find primitive rules for the representation of objects, and the real validity of these rules does not extend beyond the horizon of possible experience. Their basis is *necessity for the possibility of experience*.

Concepts of ideals serve pure practical Reason because in an ideal we have the semblance of completeness in the series of conditions and the semblance of grounding in Reality. It is no wonder that human judgmentation tends to rush to complete its work by means of the earliest opportunity for constructing an ideal. This characteristic of human reasoning has long been noted by many prominent thinkers throughout history.

> Another error is an impatience of doubt, and haste to assertion without due and mature suspension of judgment. For the two ways of contemplation are not unlike the two ways of action commonly spoken of by the ancients: the one plain and smooth in the beginning, and in the end impossible; the other rough and troublesome in the entrance, but after a while fair and even: so it is in contemplation; if a man will begin with certainties, he shall end in doubts; but if he will be content to begin with doubts, he shall end in certainties [BACO1: Bk. I, §8, pg. 16].

The "haste to assertion" often makes a leap (*saltus*), sometimes a very subtle one, and not infrequently leaves a gap (*hiatus*), gaps being susceptible to type-α compensations (ignórance).

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[11] I will present another of illustration, different from figure 24.4.1, in §7 representing Facet B quantities by analogy to the imaginary part of a complex number. The Facet B quantities will be (in a sense to be defined) orthogonal to the 'real dimension' figure 24.4.1 represents. In preview of what is to come, this representation we will call "Slepian dimensioning". It is based on the transcendental Idea of *context*.

[12] This *nexus* is, of course, a *mathematical* form. But here I recall to you that all of mathematics can be generated from the three fundamental 'mother structures' of the Bourbaki mathematicians: topological structure, order structure, and algebraic structure. The first two (topological and order structures) have their epistemological roots in the pure intuitions of space and time. The third (algebraic structure) has its epistemological roots in the process of judgmentation. It is in this way that concepts of mathematical form are connected with Nature, and this manner of connection is also the reason why objectively valid knowledge of real objects is limited by the horizon of possible experience.

But where a gap *is* noticed and *does* constitute a disturbance (a "hole in the theory"), then concerted efforts to "fill in the gap" are susceptible to the dialectical character of reasoning, and it is then that speculation can easily let itself slip past the horizon of possible experience to sail on to the making of transcendent concepts (transcendental illusions) which, although lacking entirely in real objective validity, nonetheless satisfy the condition of subjective sufficiency. These are mere persuasions, but, for those accustomed to working in Facet B and who have much *mathematical* experience, *enthusiasm* for the pursuit can build these persuasions up to the level of conviction.

Enthusiasm is often good and useful, but undisciplined enthusiasm can and does easily lead us astray in our speculations. Disciplined enthusiasm requires what Bacon called "due and mature suspension of judgment." Moreover, there is more than one kind of enthusiasm. The type described above by Bacon might be called an enthusiasm for discovery; another is an enthusiasm for the preservation of traditional ideas and approaches. Bacon referred to this as "the intemperance of systems":

> The understanding must also be cautioned against the intemperance of systems, so far as regards it giving or withholding of assent; for such intemperance appears to fix and perpetuate idols, so as to leave no means of removing them [BACO2: Bk. I, §67, pg. 115].

Thirdly, we can also speak of a type of enthusiasm that combines both the others. This is an enthusiasm of speculation aimed at the preservation of a traditional system. The third differs from the second in this way. The second enthuses for a system as it presently stands; the third engages in speculation to prop up a system in trouble; to use Locke's earlier metaphor, it props up the system with an elephant and then props up the elephant with a tortoise.

Kant described this tendency of reasoning in the following way.

> The course of things is roughly this. First *genius* is very pleased with its bold flights, since it has cast off the thread by which reason used to steer it. Soon it enchants others with its authoritative decree and great expectations and now seems to have set itself on a throne which was so badly graced by slow, ponderous reason, whose language, however, it always wields. It thereupon takes on a maxim of invalidity for superior lawgiving reason we common men call **enthusiasm**[13], while those favored by beneficent nature call it *illumination*. Since reason alone can command validly for everyone, a confusion of language must soon arise among them; each one now follows his own inspiration, and so inner inspirations must ultimately be seen to arise from the testimony of preserved achievements, traditions which were chosen originally but with time become *intrusive* proto-discoveries; in a word, what arises is the utter subjugation of reason to achievements, i.e. **overconfidence**, because this at least has a *legitimate form* and thereby pacifies [KANT12a: 17 (8: 145)].

It is to protect ourselves from precisely this dialectical predisposition in judgmentation that the

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[13] *Schwärmerei*. The word carries a connotation of fanaticism.

doctrine of method in the Critical Philosophy imposes rules (policies) for us to use for securing discipline in our speculations. With regard to proofs, Kant points out that there is a special caution required whenever we make speculations concerning *noumena*.

> The proofs of transcendental and synthetic propositions are unique among all proofs of synthetic knowledge *a priori* as such: that reason may not apply itself directly to the objects by means of its concepts, but must first explain the objective validity of the concepts and the possibility of their synthesis *a priori*. This is not merely a necessary rule of caution but concerns the essence and the possibility of the proofs themselves. Whenever I must go beyond the concept to an object *a priori*, this is impossible without a special guide to be found outside this concept. In mathematics it is *a priori* intuition that guides my synthesis, and there all inferences can be immediately drawn from pure intuition. In transcendental knowledge, as long as this has to do merely with concepts of understanding, this guideline is possible experience [KANT1a: 665 (B: 810-811)].

We have seen this rule, the acroam of objective validity, applied again and again throughout this treatise. The reader will have noticed the emphasis placed throughout on establishing the *objective validity* of our principles from *the possibility of experience*.

When the subject matter has to do with synthesis *a parte priori* (prosyllogisms) in regard to objects of outer sense, the categories of understanding can carry us only so far as the concept contains the matter of sensation ("the real of sensation"), and when we come at last to an objective *noumenon* our sole link to this Object rests on the notion of causality & dependency. This leaves us with nothing but a concept of pure form that declares the *Dasein* of the Object but can provide us with no knowledge of the manner of its *Existenz*. It is at this point where we may refine the *idea* of the *noumenon* hypothetically through the use of mathematics, but even here we must recognize that our speculations have to do only with the concept (idea) and not with the thing-regarded-as-it-is-in-itself. It is here, too, that Slepian's criterion of indistinguishability applies with its attendant distinction between principal and secondary quantities in our mathematical description. *This entire description must be addressed to nothing other than explication of the meanings of the principal quantities*, and these explanations are ultimately and only *practical*. As for the secondary quantities, they are always problematical, hypothetical, and can have none other than a merely logical validity. When we reach the *noumenal* object of outer experience we explain nothing about the object as *thing* but, rather, explain merely its practical idea, i.e. what is *in* the idea in order *for* the correspondence of Facet B to experience in Facet A. The *noumenon* then stands as a *practical condition for Nature*.

When the object has to do with inner experience (phenomenon of mind) we have one advantage not granted to us for objects of outer experience. This advantage is the absolute certainty we each attach to our own *Dasein*. But even here – here especially – all our explanations concern intelligible objects within a merely logical division of the Organized Being. Therefore all such explanations can have none but a practical objective validity and must therefore take their guidance from the *actions* of the Organized Being. Here especially the sole criterion for all propositions of the organization of *nous* and *psyche* is the transcendental criterion: that the Object be **necessary for the possibility of experience** as we know it for the Organized Being.

Kant laid down three more explicit rules regarding proofs.

> The first rule, therefore, is this: to attempt no transcendental proofs without having first considered whence one can justifiably derive the first principles on which one intends to build and with what right one can expect success in inferences from them. If they are first principles of understanding (e.g. causality), then it is in vain to try to arrive by their means at Ideas of pure reason; for those are valid only for objects of possible experience. If they are to be first principles from pure reason, then again all effort is in vain. For reason has principles, to be sure, but as objective first principles they are all dialectical, and can only be valid as regulative first principles of the systematically coherent use of experience [KANT1a: 667 (B: 814)].

We can employ the categories of understanding with real objective validity only for so long as we remain on our side of the horizon of possible experience. The idea of the *noumenon* is the shore marker, beyond which lies Kant's ocean of fog and speculative illusion. As for the transcendental Ideas, their only validity is as regulative principles for the thoroughgoing systematic unity of in the application of concepts of experience. Ideas (*Ideen*) are practical.

It is appropriate in this regard to comment upon something I suspect a few discerning readers might have already noticed and questioned. In this treatise it looks as if I have been making the transcendental Ideas have a sort of dual character. On the one hand, you have seen them cast as the regulative principles governing the phenomenon of mind, and this is consistent with what Kant says above. But, on the other hand, you have also seen me use them as the fundamental principles of Kant's pure metaphysics proper. How can this be? The answer is simple but somewhat subtle. Let us first ask: In pure metaphysics centered on epistemology, what sort of principles can claim to be metaphysical *first* principles? Obviously what must be required of them is that they arise from nowhere else than the *phenomenon of mind*. Epistemological first principles must be the principles by which we come to have human knowledge. Such principles can therefore be none other than the principles of pure Reason, and these principles are, as we have seen, regulative principles.

But if they are the regulative principles of pure Reason, how do we come to know about them as Ideas? Here is the great genius of Kant's method. As we know from our own experiences, mind can reflect upon itself. This is what Kant has done. *He has turned the power of Reason back upon itself and made Reason use its own regulative principles to bring out an* ***understanding*** *of these same first principles*. By digging down to the bedrock of what Ideas mean and do, he has *through analysis of the experience of understanding* put the actions of the phenomenon of mind to work to produce the concepts of its Nature, and these concepts are the Ideas of pure metaphysics proper. **This is what Kant means when he says philosophy is knowledge *through* concepts, mathematics knowledge through *the construction* of concepts**.

Kant's second rule pertains to transcendental propositions.

> The second peculiarity of transcendental proofs is this: that for each transcendental proposition only *a single* proof can be found. If I am to infer not from concepts but rather from the intuition which corresponds to a concept, whether it be a pure intuition, as in mathematics, or an empirical intuition, as in natural science, the intuition that gives the grounds offers me a manifold of subject-matter for synthetic propositions that I can connect in more than one way, thus allowing me to reach the same proposition by different paths since I may start out from more than one point.
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> Every transcendental proposition, however, proceeds solely from one concept and states the synthetic condition of the possibility of the object in accordance with this concept. The ground of the proof can therefore only be unique, since outside this concept there is nothing further by means of which the object could be determined, and the proof can therefore contain nothing more than the determination of an object in general in accordance with this concept, which is also unique [KANT1a: 667-668 (B: 815-816)].

One consequence of this, as Kant later noted, was that, in matters of transcendental proofs, if a theorist ("dogmatist") can offer multiple proofs of the same transcendental proposition, "one can believe he has none at all."

Kant's third rule holds that transcendental proofs can only be ostensive and never apagogic. An ostensive proof is a proof through a broad principle. An apagogic proof, in contrast, is proof by showing that any counter-proposition is absurd. In *Critique of Pure Reason* Kant demolished both empiricism and rationalism by setting side-by-side their apagogic proofs of such transcendent propositions as "the world had a beginning in time" vs. "the world had no beginning in time." This and the other propositions are called the transcendental antinomies. Each side (the thesis and the antithesis) argued by denying the premise and showing that the consequence was self-contradictory. In other words, the thesis 'proved' the antithesis was absurd and therefore claimed the thesis had to be correct, and the antithesis did precisely the same thing to the thesis. Kant shows us in this section of the *Critique* what the error each side made was, and what the Critical resolution of the antinomy was. What is pertinent to our present discussion is that apagogic proofs do not actually prove anything when the subject at hand concerns transcendental propositions (although they can be employed in mathematics). Worse, an apagogic proof is deceptive if it is possible to mistake what is merely subjective in our representations for what is objective. In transcendental matters, therefore, apagogic proofs tend to bolster conviction in transcendental illusions.

Another part of the discipline of pure Reason concerns the contrast between what one can do in mathematics vs. what is possible for empirical concepts. Here, too, the considerations are three-fold [KANT1a: 637-643 (B: 755-766)].

The first consideration is that of definitions vs. explanations. Concepts of mathematical Objects are made concepts and, provided only that the definition is not self-contradictory, the concept is never in error.[14,15] To define is, properly, to originally exhibit the exhaustive concept of a thing, and this mathematics does. A typical format in a mathematical argument runs: definitions – lemmas – theorem – proof. Oftentimes the proof consists merely of applying the lemmas to the definitions, and in this sense mathematical theorems are tautologies. A concept of an empirical Object, by contrast, is explained by exposition and for them we can never claim that the concept exhaustively exhibits the object. In this we can see a fundamental characteristic difference between a principal quantity in Slepian's Facet B and the object in Facet A, for which the principal quantity is a mathematical explanation. All mathematical objects are intelligible objects, and were it not for thinking the *rule* requiring principal quantities to be congruent with the concept of the empirical object, mathematical constructs would constitute one of Margenau's "island universes."

The second consideration is that of the distinction of axioms vs. acroams. An axiom is a synthetic *a priori* principle insofar as it is immediately certain. Axioms belong exclusively to mathematics, and because they are products of intuition made through spontaneity in the free play of productive imagination and determining judgment, they enjoy *mathematical* certainty under the acroam of Axioms of Intuition. But mathematical certainty is not the same as real objective certainty. Confined *strictly* to mathematical objects, the axioms of Zermelo-Fraenkel-Skolem set theory are certain. This is why the formalists do not lie to us when they insist that their axioms are "merely rules of the game." The *uncertainty* in regard to axioms that prevailed during the "crisis in the foundations of mathematics" at the end of the 19th and beginning of the 20th centuries was not an uncertainty attending mathematical objects; it was uncertainty attending the congruence of mathematical objects with objects of Facet A. This is why what I previously called "hypothetical mathematics" (in Chapter 23) is still *pure mathematics*. If I may be permitted to build on Margenau's terminology, we can call *any* system of mathematics built upon some particular set of axioms a "galaxy" in figure 24.2.1. The task of Critical mathematics (Chapter 23) is to provide us with a means of preventing a mathematical galaxy from becoming an island universe. Critical mathematics can employ *only* those axioms we can deduce from the Critical acroams, and its principal task is to look to the rules of correspondence between principal quantities and concepts

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[14] An example of a self-contradictory definition is "x is the multiplicative inverse of zero." Zero has no multiplicative inverse. The operation $x = 0\times\infty$ is *undefined* in mathematics because no unique answer is given by this operation for x.

[15] Error is disagreement between the concept and its object. In mathematics the concept makes the object what it is and, consequently, the concept never disagrees with its object.

of empirical objects. By contrast, hypothetical mathematics looks after the secondary quantities and their connection to the principal quantities. It is thus indispensable for the *practice* of science. Critical mathematics will tell us which mathematical objects can be principal quantities and it should provide criteria by which we can distinguish principal from secondary quantities. Hypothetical mathematics will then enjoy freedom to pursue mathematical knowledge in whatever way seems best to the mathematician, provided only that its context in regard to Critical mathematics remains clear.

The third consideration concerns demonstrations.

> Only an apodictic proof, so far as it is intuitive, can be called a demonstration. Experience may well teach us what it is but not that it could not be otherwise. Hence empirical grounds of proof can provide no apodictic proof. However, from notions *a priori* (in discursive knowledge) intuitive certainty, i.e. evidence, can never arise however apodictically certain the judgment may otherwise be. Thus only mathematics contains demonstrations since it does not derive its knowledge from concepts but rather from their construction, i.e. from the intuition that can be given *a priori* corresponding to the concepts. . . Philosophical knowledge, on the contrary, must do without this advantage since it must always consider the general *in abstracto* (through notions), while mathematics can assess the general *in concreto* (in the individual intuition) and yet through pure representation *a priori* where every false step becomes visible. I would prefer to call the former *acroamatic* (discursive) proofs – because they are only conducted through plain words – rather than demonstrations, which, as the expression already shows, proceed by intuition of the object [KANT1a: 641 (B: 762-763)].

One consequence of this is that philosophy can never, as Kant put it, decorate itself with the titles and ribbons of mathematics "to whose ranks philosophy does not belong" nor strut about pretending to be mathematics. Kant was always critical of Newton's view that there was such a thing as 'mathematical philosophy.' Mathematics can demonstrate; philosophy cannot. This is not to say there are no philosophical deductions. It is to say that the keystone of all metaphysics comes always back to the criterion of *necessity for the possibility of experience*. But experience is contingent; tomorrow may bring something that upsets one's understanding of experience. Thus, unlike mathematics, no apodictic, iron-clad proofs (demonstrations) are possible in philosophy in regard to the special objects *in concreto*. (It is worth recalling that 'apodictic' is a logical *momentum* concerning the sphere of concepts, 'necessity' is a notion of understanding in regard to the scope of concepts and, as a notion of Modality, is a judgment of a judgment of experience).

§ 6. The New Platonism

Nothing better illustrates the need in present day science for the discipline of pure Reason than the rising tide of what I call the new Platonism in physics that has been increasingly popular over the course of the last two-plus decades. In one sense it may be inaccurate to call this change in attitude by the name Platonism, but if so this is only because most of its practitioners probably do not realize they are following in Plato's footsteps. I rather expect most of them would issue injured protests and denials at being so labeled. I also do not mean the phrase "new Platonism" to refer in any way to the classical neo-Platonists, who were not so much philosophers as they were mystics and whose doctrine was not so much a philosophy as much as it was a religion. Nor do I use the term to mean the Renaissance Platonism, nor the 17th through 20th century Cambridge Platonism, nor the 18th century Hellenic Platonism. The attitude to which I refer does, however, meet the criterion of Platonism as described in *The Oxford Dictionary of Philosophy*, to wit:

> **Platonism:** The view . . . that abstract objects, such as those of mathematics, or concepts such as the concept of number or justice, are real, independent, timeless, and objective entities.

I doubt very much if those whom I label "new Platonists" are likely to recognize themselves in *the Oxford's* description, especially since I suspect most of them do not regard themselves as philosophers, certainly not as metaphysicians, and they do not cite Plato as a source of their inspirations or the directions of their works. These people are scientists and their ranks include many who are held in very high esteem. During the critique that follows, I ask the reader to understand that I am not critical of their work insofar as it is science; I am critical of the pseudo-metaphysical prejudices that shape it and the enthusiasm that taints it.

Because my aim is to illustrate the need for Critical discipline in science, I am going to confine my Critique mostly to one arena of present day speculation, namely Big Bang cosmology. This is not because there are no other examples. The new "string theory" could probably serve equally well for my purposes. But the paradigm of cosmology called Big Bang cosmology has been generating for itself a lot of press coverage over the past three decades, it is being represented as an established fact and taught as such in the high schools, and it is being featured in numerous educational television programs, aimed at the lay public, where its status as fact is taken so much for granted that this premise is the starting point for all else.

I will state right now that I do not think the Big Bang ever happened; I also do not think it did not happen. Furthermore, it is a matter of supreme indifference to me whether it did or did not. I do not expect to know the answer to this question in my lifetime, and I am okay with that, too. What I do care about is science professing knowledge where it has none, presenting mere hypothesis as if it were a fact, and increasingly engaging the public with absurd fantasies (e.g. "time travel") that better suit science fiction than science. I think it is mostly good for scientists to be enthusiastic about doing science, but science itself must not be enthusiastically inflated.

I also do not say science should not try to present itself to the public at large. Indeed, the level of science illiteracy in the United States is nothing short of appalling and to what if not to science education can we turn to remedy this problem? But one cannot do anything productive about this by treating the lay public as if every non-scientist is too stupid to understand anything about science[16] or by fearing that honest full disclosure of what is speculation vs. what is fact might hurt the national funding for science. These presumptions lead to harmful consequences. The calendar may say we are living in the 21st century, but our society shows signs of sliding backwards into a 19th century society, and I see no comforting sign that we would stop there. The only thing I find more appalling than the level of our science illiteracy is the black darkness of our nation's ignorance of history, which is in its own way another form of science illiteracy.

Now you know your author's agenda for this section. Let us learn about the Big Bang.

§ 6.1 The Roots of the New Platonism

As physics entered the twentieth century positivism strongly dominated the views and attitudes of its practitioners. Furthermore, physics was still very mechanistic. Although the works of Faraday and Maxwell had brought about a very successful theory of electromagnetism (which, thanks to Maxwell, was already very mathematical), even this branch of the science had its underpinnings in the mechanistic paradigm. The mathematics of mechanics and of thermodynamics was already quite sophisticated, but nonetheless very down-to-earth and even, compared to today's standards, reasonably simple and not all that hard for a mathematically literate amateur to understand.

But this state of affairs was about to change in a revolutionary paradigm shift. The years from 1900 to 1929 were the years of the discovery and initial development of the quantum theory. A wonderful account of this time, written by one who was present first-hand near the end of that period and knew its major contributors, has been provided for us by George Gamow[17]. It began with the discovery of the quantum of action by Max Planck in 1900. Planck introduced the

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[16] One thing I will acknowledge of Big Bang cosmology: its airings to the public are not intentionally deceptive. I think the proponents of the theory hold-it-to-be-true in high degree. The worst of the worst are those presentations where science programs "go Hollywood." In one recent 'educational' television program the topic was the evolution of our species, *Homo sapiens*. The series presented actors dressed up as prehistoric hunter-gatherers running about in a Keystone Kops cluster for no apparent reason in a storm-blasted desert. Two members of the group were struck by lightning, one being killed, the other rendered unconscious, not breathing, and apparently dead. Some time later another member of the group killed an animal that just happened to come strolling by through this same barren wasteland and, by coincidence, the unconscious person (who by then had not been breathing for quite a long time) just happened to regain consciousness at the moment the animal died. This vignette was presented as "how man came to have the ideas of spirit and religion." Did the writers and producers of this show really think people are so stupid or naive that we would not know science has utterly no way of knowing if this ever happened, or that people who haven't been breathing for a long time suffer severe brain damage and do not just get up and go about their business like nothing ever happened? Apparently the people who put this show together want us to think that "Yeah, maybe it could have happened this way," is a legitimate way of practicing science. Nothing hurts the credibility of scientists more than hogwash like this.

[17] George Gamow, *Thirty Years that Shook Physics*, Garden City, NY: Anchor Books, 1966.

quantum hypothesis as a way to derive a mathematical formula that correctly describes a phenomenon in thermodynamics known as "black body radiation." The black body radiation problem was an important physics problem at the time because the existing theory, the Rayleigh-Jeans equation, made a prediction that was starkly at odds with experiment. This was known at the time as "the ultraviolet catastrophe". Put simply, the theory was wrong and everyone knew it.

Planck's equation was in beautiful agreement with experiment. He had introduced the quantum as a mathematical trick based on the hypothesis that energy could only be absorbed and transmitted in discrete amounts (i.e., blackbody energy was quantized). Planck himself was very loath to attribute real significance to his quantum; he spent many years trying to find an alternate way to come up with his equation without needing to introduce the quantum hypothesis. In Slepian's terminology, the quantum was a secondary quantity, not itself corresponding to anything measurable, and it was furthermore a peninsular construct (in the Margenau sense of figure 24.2.1). This, however, changed in 1905 when Einstein used the quantum idea to explain another quirky experimental peculiarity, known as "the photoelectric effect." Plank's quantum construct now had two independent pathways by which it could travel to reach Margenau's plane of Nature.[18]

The next chapter in the saga was written by a rambunctious young physicist named Niels Bohr in 1913. Sir J.J. Thomson had discovered the electron in 1897 but his attempts to model the structure of the atom in light of the existence of the electron were wholly unsuccessful. In the meantime another physicist, Ernest Rutherford, had proposed a "solar-system-like" model of the atom in which the electrons are seen as orbiting around a positively-charged nucleus. He based this model on the results of his famous "scattering" experiments, and the Rutherford model is the cartoon most of us learn about in middle school science class. The problem with the Rutherford model was that it was in flat defiance of an important consequence of Maxwell's theory. An electron orbiting a nucleus undergoes a strong acceleration that keeps it in orbit and, according to the theory of electromagnetism, an accelerated charged particle must radiate energy into space. In an incredibly brief amount of time the electron should spiral down into the nucleus and the Rutherford atom should cease to exist.

Bohr argued that if radiation energy is quantized then mechanical energy – specifically the energy of the electron in its orbit – should be quantized too. The mathematical consequence of this requirement was that electrons could occupy only certain specific orbits (those where their energy was an integer multiple of Planck's fundamental quantum). Bohr also added the

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[18] Einstein is so thoroughly famous for his relativity theory that many people are surprised when they learn Einstein's Nobel Prize came for his work on the photoelectric effect and not for relativity.

specification that radiation by the electron could only occur in 'chunks' of Planck's quantum. In effect, he argued that Maxwell's theory did not apply to the electron in its orbit. By proxy, since the Maxwell equations do not predict the quantum phenomenon, Bohr's position amounted to saying that the theory of electromagnetism was wrong. Yet Bohr's bold hypothesis led to a successful explanation of experimentally observed properties of radiation by atoms that had been known since the late 1800s. Planck's quantum now had three independent pathways to Margenau's plane of Nature.

Nonetheless, the flat contradiction that existed between Bohr's theory and Maxwell's theory was, to put it mildly, very disturbing. The situation could not be tolerated forever, and in 1925 another young physicist, Louis de Broglie, added the next revolutionary idea. The Planck-Einstein theory endowed light (which was regarded under Maxwell's theory as merely a form of electromagnetic wave) with particle-like properties. (The 'particle of light' is called the photon). De Broglie proposed that particles would likewise exhibit wave-like properties. As we discussed earlier in this treatise, this was the proposal that led to "de Broglie waves" as a new secondary quantity in theoretical physics, the effects of which were later confirmed experimentally[19]. De Broglie's wave hypothesis was given more rigorous mathematical form in 1926 by two other men, Erwin Schrödinger and Werner Heisenberg. Max Born added a probability interpretation of de Broglie waves, and thus was born the probability amplitude theory of quantum mechanics.

Under this new theory the conflict between Bohr's model and Maxwell's theory was resolved, but in a most interesting and unexpected way. Bohr's model had pictured the electron as a small, electrically-charged particle orbiting the nucleus. Under the new de Broglie-Schrödinger-Heisenberg theory this simple and intuitively-appealing picture has to be given up. Instead, the electron in an atom has to be pictured only in terms of the mathematical expression given by the wave equation (in Schrödinger's formulation) or, even more abstractly, in terms of a special matrix called the 'Hamiltonian matrix' (in Heisenberg's formulation). The mathematical result is that one can not speak with objective validity of the "location" of an electron in an atom. Instead of describing the situation in terms of 'orbits' the phenomenon of the electron is described in terms of 'orbitals' and 'operators' that link these orbitals to measurable effects. The end result is a very non-particle-like description reminiscent in many ways of Aristotle's abstract idea of 'place'. When this 'orbital-operator' description is used to obtain the corresponding predictions from Maxwell's equations the radiation paradox disappears. Physicists express this by saying "the

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[19] The 'de Broglie waves' themselves are not experimentally observable. The principal quantity in these experiments was the mathematical electron. 'Wave mechanics' makes predictions for what we should observe under certain conditions and these predictions were what the experiment verified.

orbitals are stationary," meaning that the previous 'acceleration' the electron had to undergo in Bohr's model no longer applies to 'orbitals'.

The new quantum theory explained a great many phenomena that the pre-quantum theory of physics could either not at all explain or gave incorrect predictions, and it made many additional new predictions, confirmed by experiments, that the older physics could not even begin to imagine. Nonetheless, there was at least one thing the new theory could not explain. This was the phenomenon of ferromagnetism. We are all familiar with permanent magnets. People use them to attach notes to refrigerator doors and permanent magnets are used to build many different kinds of electric motors. The simple compass uses a permanent magnet as the needle and "the needle always points north" because of its interaction with the earth's magnetic field. The phenomenon of ferromagnetism has been known since ancient times. (The Greeks called a particular type of ferromagnetic material "lodestone"). The problem was: according to classical physics permanent magnets should not exist except at extremely cold temperatures – colder temperatures than are found occurring naturally anywhere on earth.

The new quantum theory did not fix this problem. It was possible to make some *ad hoc* patches to the theory. Wolfgang Pauli was a leading figure in helping to "patch in" mathematical correction factors for describing ferromagnetism. He introduced what became known as the "Pauli spin matrices" as the phenomenological correction factor in the early days of the Bohr model. The spin matrices were based on another experimental oddity. Very detailed experiments measuring the spectrum of light emitted by 'excited' atoms in the presence of a strong magnetic field showed a small but significant discrepancy with quantum theory. This turned out to be due to another property of the electron. It was found that the electron possesses its own "magnetic moment" – i.e. an electron acts as if it were a tiny bar magnet. In 1925 two Dutch physicists, Samuel Goudsmit and George Uhlenbeck, proposed the model that perhaps the electron "spins" on its own axis, much like the earth spins on its axis as it orbits the sun. Since the electron was viewed as a "charged particle", this spinning would constitute an electric current and, according to Maxwell's equations, such a current must produce a magnetic field. This is where Pauli's matrices obtained the name "spin" matrices.

There were at least two problems with this idea. First, the Schrödinger equation simply had no place in it to accommodate the "spinning electron." Second, when physicists calculated how fast the electron had to 'spin' in order to produce the observed magnitude of its magnetic moment it turned out that the 'surface' of the electron had to travel at a velocity greater than the speed of light – which violated an important result of Einstein's relativity theory. On top of this, there really is no way to reconcile this picture with the 'orbital' picture of the atomic electron without re-introducing the fundamental contradiction between Bohr's model and Maxwell's equations.

This problem was solved, in an unexpected way, by Paul Dirac in 1928. Einstein's relativity theory had required the equations of physics to be invariant to certain changes in the coordinate system used to write them. The Schrödinger equation does not satisfy this invariance requirement. It is a 'non-relativistic' equation. What Dirac set out to do was re-cast de Broglie's idea in the form of an equation that satisfied Einstein's requirement. Dirac's "relativistic wave equation" met this requirement and, as a bonus, his equation provided for the magnetic moment of the electron. This was the birth of quantum electrodynamics. Thus, it turned out that ferromagnetism was not only a quantum-mechanical effect; it was a *relativistic* quantum-mechanical effect. Dirac's theory had other unexpected consequences, the most famous of which was his prediction of the existence of 'anti-particles.' We touched on this aspect briefly earlier; the problems Dirac's theory produced were cleared up by the later theory of quantum electrodynamics. But by 1930 the first act in the drama of the quantum revolution in physics had drawn to a close.[20]

No reasonable and informed person could argue that the revolution in physics from 1900 to 1930 does not constitute a gigantic triumph for science. But the price to be paid for this progress was the utter destruction of classical physics' ontology for looking at the world. Before 1900 corpuscles and waves were seen as entities existing in Slepian's Facet A of Nature. After 1930 they were not even principal quantities in Facet B. Rules for "expected values" that correspond roughly to the old ideas of corpuscles and waves do fill the role of principal quantities in the new physics, and new terms like 'wavicle' have been coined as a means of trying to salvage some ontological picture in Facet A. But the simple fact is that now, more than ever, our understanding of the physics is mathematical and no *experienced* physicist is all that shaken any more when the mathematics is at odds with his Facet A ontological presuppositions. The mathematical theory *works*, and for the pragmatic-minded physicist this is what counts.

> What I am going to tell you about is what we teach our physics students in the third or fourth year of graduate school – and you think I'm going to explain it to you so you can understand it? No, you're not going to be able to understand it. Why, then, am I going to bother you with all this? Why are you going to sit here all this time, when you won't be able to understand what I am going to say? It is my task to convince you *not* to turn away because you don't understand it. You see, my physics students don't understand it either. That is because *I* don't understand it. Nobody does. . .
>
> I'm going to describe to you how Nature is – and if you don't like it, that's going to get in the way of your understanding it. It's a problem physicists have learned to deal with: They've learned to realize that whether they like a theory or they don't like a theory is *not* the essential question.

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[20] Physics did not, of course, come to a standstill in 1930. There were still plenty of physics problems left to be worked, not the least of which were the problems of nuclear physics leading to quantum chromodynamics in the 1960s. But it is fair to say the years from 1900 to 1930 constituted the first act in the drama of the twentieth century's revolution in physics.

> Rather, it is whether or not a theory gives predictions that agree with experiment. It is not a question of whether a theory is philosophically delightful, or easy to understand, or perfectly reasonable from the point of view of common sense. The theory of quantum electrodynamics describes Nature as absurd from the point of view of common sense. And it fully agrees with experiment. So I hope you can accept Nature as She is – absurd.
>
> I'm going to have fun telling you about this absurdity, because I find it delightful. Please don't turn yourself off because you can't believe Nature is so strange. Just hear me out, and I hope you'll all be as delighted as I am when we're through [FEYN1: 9-10].

In this prologue to a lecture series on QED, Feynman gave as clear and honest a description of the attitude a professional physicist must adopt in his work as I have ever heard. When a well-established theory makes a startling announcement in consequence of a careful and rigorous analysis of the implications of its mathematical description, a physicist has very little choice but to accept that pronouncement as it stands. Of course this does not relieve one of the need to go into the laboratory to check on the veracity of this prediction. A *theory* differs from a "mere speculation" by virtue of the fact that a theory makes testable anticipations, and the more precise these anticipations can be, the better the theory is. In Feynman's view, a vague theory is no theory at all. An *ad hoc* theory is no theory at all, although it might serve as a *route* by which a theory can be reached. Speculation comes in different degrees, ranging from the least in a well-established theory to the greatest in making guesses of possible explanations. An hypothesis is a scientific guess based on facts.

Nonetheless, equations do not come with an owner's manual spelling out, "Apply me to this case and that, but not to this other case." When scientific research is at the forefront of knowledge a scientist perforce finds him- or her-self engaged in concept-making as a necessary activity in preparation for being able to apply mathematics. And this is the key point of this section. Concept-making relies upon how a person "views the world"; it is inherently metaphysical thinking and it inherently relies upon the notions and ideas a person holds as his or her *ontological* suppositions. If these ontological suppositions and the concept-making based on them lead to a fecund and successful outcome, the scientist has made a "breakthrough." If it does not, well "a scientist's work is never done."

The question then comes down to: What sort of *discipline* are we to apply to our *ontological* preparations? To sort through this question *scientifically* requires a careful understanding of the 'entities' one is using for concept-making. It requires one to be aware of whether one's 'entity' is a Facet A phenomenon, a Facet B principal quantity, or a Facet B secondary quantity. What I call the new Platonism is an un-Critical mistaking of secondary quantities in Facet B for principal quantities or even for objects of Slepian's Facet A. Plato believed "the world of Ideas" (the Platonic Ideas in his case) was more real than the "world of opinion" (sensible phenomena) we live in. The new Platonism is this attitude in un-Critical and *undisciplined* maxims.

§ 6.2 The Roots of the Big Bang

Theories and speculations that gain the attention and respect of the scientific community do not just materialize from out of thin air. A lot of groundwork goes into preparing for them. This is just as true for big bang theories as for any other topic of study that attracts the efforts of scientists. There are a number of observations and a set of key *facts* providing the foundation for cosmology research (both 'Big Bang' and 'big bang'). We will review these in this section.

When Einstein published the general theory of relativity in 1915, everyone assumed the universe was "static." What this was basically supposed to mean was that the universe was neither getting bigger over time nor getting smaller. After all, if the universe is "everything" how could "everything" get either bigger or smaller? Such was the common sense view that had been held by everyone who bothered to think about such things for well over two millennia. It was therefore rather bothersome that the relativity theory of 1915 appeared to be at odds with this common sense view of things. More specifically, the relativity theory appeared to require that the universe *not* be "static'; rather, it seemed to tell us that the size of the universe had to be changing as time progressed. This was pointed out by the Dutch astronomer Willem de Sitter in 1916 and 1917. Einstein also realized this problem, which he discussed in a famous 1917 paper, "Cosmological considerations on the general theory of relativity," in which he introduced a new factor, the "cosmological constant," into the equations of general relativity. By a judicious choice of cosmological constant, a static (but unstable) universe can be obtained.

In the meantime, observational evidence was being gathered by astronomers that challenged this popular idea of a "static" universe. To understand this evidence and its interpretation it is important to know that starlight contains a mix of different characteristic frequencies. Classically, light is describable as an electromagnetic wave[1] and the different frequencies of light are called its colors[2]. Red light is a wave of lower frequency than blue light. The overall mix of these different frequencies is called the "spectrum" of the light. The particular spectrum exhibited by the light from any particular star is determined by the composition of the chemical constituents of the star in a way fairly well modeled and understood by basic quantum mechanics. Stars with the same chemical composition will emit the same spectrum of light. Although the relative intensities of the different colors may vary due to different amounts of the various chemical constituents, the heat of the star, and other factors, the frequencies do not.

What the astronomers were noticing was that the light from stars in nearby galaxies seemed

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[1] The quantum theory does not alter the validity of looking at light in this way.

[2] Different photoreceptor cells in the retina and different neural pathways in the brain respond differently to light at different frequencies. This neural network structure is the biophysical mechanism in *soma* for the phenomenon of color perception.

to be “red shifted.” This means that the light spectrum consists of lower frequencies than we see emitted from our sun, but if one were to overlay the sun’s spectrum (or that of another ‘reference’ star) on top of the measured spectrum from the galaxies, all the frequencies seem to be shifted by the same factor. Now this phenomenon has a simple explanation according to the special theory of relativity. If the galaxy being observed is moving away from us with some particular velocity, the relativity theory predicts that the light we observe from this galaxy will be shifted toward the red end of the spectrum in precisely the way the observational data was turning out. The effect is called Doppler shift.

A reasonable question to ask at this point is: Doesn’t this interpretation have to assume that all stars are composed of the same types of chemical constituents? Yes, it does. However, this is a pretty safe assumption to make. Otherwise we would have to postulate that these “red shifted” stars were composed of atomic constituents of some unknown type. The periodic table of chemical elements contains no “blanks” in its entries, and so there is no justifiable reason to think the red shift is due to some chemical mechanism of this sort. Doppler shift, on the other hand, is based on very well established physics and is a far more trustworthy explanation for the red shift.

Another reasonable question can be asked by a person who knows a little bit more about the consequences of the relativity theory. According to the general theory, light traveling away from a star will show a shift to the red due simply to the reduction in the gravitational potential as one gets farther away from the star. This is called the “gravitational red shift.” Why couldn’t the red shift phenomenon be due merely to gravitational red shift and not to the star moving away from us? The problem with this idea is “numerical.” The amount of red shift in going from, say, our sun to the earth can be calculated, and the numbers from this calculation show the amount of shift to be extremely tiny – so tiny in fact that it cannot be resolved by our measuring instruments. Well, then, what about stars that are much more massive than our sun? If we consider “white dwarf” stars such as Sirius B, we find that the gravitational red shift *can* be detected, and observations of Sirius B confirm that this gravitational red shift does occur. Again, however, the numbers obtained for this mechanism are much smaller than the amount of red shift observed for distant galaxies. Gravitational red shift may add to the total amount of red shift, but it is not adequate to explain *all* (or even more than a tiny part of) the observed red shift.

A person who knows a little bit about the quantum theory can pose yet a third question. According to the quantum theory of light the energy of a photon is proportional to its frequency and is given by Planck’s equation $E = hf$, where E is the energy, f is the frequency, and h is a universal constant called Planck’s constant. If for some reason a photon loses energy on its way to our measuring instrument, wouldn’t this mean the light would appear red shifted? Yes, it does.

But the problem with this idea, sometimes called the "tired light" hypothesis, is that all known scattering mechanisms by which light could "lose" energy in this fashion also predict other observable consequences. These consequences are *not* observed to accompany the red shift phenomenon astronomers measure. Thus the "tired light" idea, while mathematically possible, appears to be contradicted by actual measurements.

Given all this, the astronomers' conclusion – that red shift means the galaxy is moving away from us – appears to be an unshakably correct view of our understanding of Nature. The red shift observations provide the first step in the genesis of cosmology theory. The next step was, arguably, taken in 1924 by astronomer Carl Wirtz. Wirtz gathered up all the observational data at the time – some forty-odd measurements – and found that the fainter galaxies all showed more red shift than the brighter ones. If one assumes that fainter galaxies are farther away, this implies they are moving away from us faster than the brighter (and therefore nearer) galaxies. Wirtz proposed this as a tentative hypothesis, "tentative" because at the time there was no generally accepted way to find out if the fainter galaxies really were farther away. Indeed, it had not yet been generally accepted that galaxies as such actually existed (as opposed to the possibility that maybe what was being observed were merely nebulae distributed throughout the Milky Way; galaxies were originally called 'nebulae' and were thought to be clouds of dust and gas).

The next and decisive step for cosmology theory, the one that would eventually lead to both 'big bang' and 'Big Bang' hypotheses, was taken by Edwin Hubble and his assistant, Milton Humason, in 1929. In 1923 Hubble had observed an important class of stars, called Cepheid variable stars, in the Andromeda nebula. Cepheids are stars whose brightness varies periodically. In 1912 Henrietta Leavitt had discovered that the period of the variation in brightness of Cepheid variable stars was related to the total amount of light emitted by the star. Therefore if one measures both the period of the Cepheid's variation and the apparent intensity of its light, one can estimate how far away from us it is. Hubble used this to show that Andromeda is very, very far away from us. It was this finding that established the model of galaxies as "island universes" separated by enormous gulfs of near-empty space.

When Hubble examined the red shift data for galaxies where he could make out Cepheid variable stars, he, too, drew the same conclusion as Wirtz. Unlike Wirtz, however, he now had some measurements to justify the assumption that fainter galaxies really were farther away, and he had enough data to be able to propose a relationship – known now as Hubble's law – that linked how far away a galaxy is to how fast it appears to be moving away from us. Hubble's work is taken as the first clear evidence that the universe is expanding. His findings convinced almost every major scientist, including Einstein, to abandon the "static" model of the universe.

Before we pass on to the development of big bang hypotheses (including the Big Bang one – the one that claims to explain the 'creation of the universe') a few comments are in order. The first concerns an all-too-easy-to-make misunderstanding of Hubble's theory. It is easy to get the impression that the theory says *every* galaxy is moving away from us. But this is not true. Our nearest neighboring galaxy, Andromeda, appears in fact to be moving *towards* the Milky Way. Astronomers tell us that someday in the far future the two galaxies will collide. When theorists tell us that other galaxies are moving away from us, what they mean for us to understand is this is an overall *trend*, within which there can be and are numerous exceptions. This is a qualification (oftentimes not stated in presentations of the theory to the lay public) one is supposed to know and bear in mind in any discussion of cosmology. We will return to this point later.

Second, it is obvious (or should be) that the derivation of Hubble's law relies upon the model of the Cepheid variable stars. In other words, Hubble's law was a synthesis of observational data and an existing theory which allows astronomical distances to be "calibrated" to an independent variable (namely the Cepheid's period of variation). Some extra-cautious scientists call findings that rely upon the correctness of a model "theory-laden data." Now, this is not a general condemnation of "theory-laden theories" because, as we have noted earlier, *all* observations that rely on measuring instruments to "extend our senses" are to some degree "theory-laden." Science would have very, very few findings if "theory-laden theories" were banned. This does, however, raise the issue: How much are we to trust results derived from the Cepheid variables model?

Here there enters into the discussion the easy-to-make observation that the CV model is based on the classical physics of the pre-wave-mechanics/pre-general-relativity era. We should, therefore, be very interested in what exactly the thinking is that goes into making the CV model. The usual assumption is that the variability of a Cepheid's brightness is caused by a pulsating oscillation in the radius of the star itself. If the star periodically expands and contracts this will lead to periodic variations in its mean temperature and therefore variations in the stellar chemistry by which light is produced. Observations of the spectral constituents of light from Cepheids appear to more or less agree with this 'pulsating star' hypothesis. There is enough variation in the measurement results to let us know the model is not perfect, but on the whole the data appears to be consistent enough for us to make the hypothesis that pulsation is probably the major factor in the causality of the phenomenon. The model of the star as a pulsating spherical mass of gas predicts that the period of variation should vary approximately as the inverse square root of density, and observations show that Cepheids closely follow this law. In addition the absolute value of the period can be approximated to a small margin of uncertainty and the observed period is found to agree with the calculation. On the other hand, we have (so far as I know) no theoretical explanation for why the star should pulsate in the first place. Also, the mathematical theory of a pulsating star predicts that maximum brightness should occur at the moment of greatest compression, whereas observations indicate maximum brightness occurs more typically a quarter-period later. Thus, we know the model is not perfect but does not seem to produce very great variations from the observations.

Third, one is bound to ask: How do we know we have a well-calibrated relationship between the period of the CV's variations and its distance? This important piece of the picture is obtained from observations of near-by Cepheids, for which alternate methods of estimating distance (e.g. triangulation methods) are available.

Finally, when it comes to observations of very distant galaxies even Cepheid variable stars are too faint to make out. What do we do in this case? Here things begin to get a bit more problematical. To measure very great astronomical distances we require objects that are much brighter than Cepheid variable stars. There is a candidate for this; it is called a Type Ia supernova. Empirical evidence suggests its distance is determinable from how its brightness changes with time. Confidence in this, however, is based on its model, a point we will return to later.

§ 6.3 Mathematical Relativity in Cosmology

The observational data just discussed does not, all by itself, mean that the universe had a beginning, much less that it began in a Big Bang. All it implies is that for us, standing here on the Earth today, the distant galaxies appear to be following a trend of moving away from us. Obviously we are not in possession of observation data taken from locations in other galaxies and so we do not know for sure that an observation made from some other place will show the same red shift dynamics as we see from the earth. It is conceivable there could be something "special" about our place in the universe. The red shift data just might be an accident of our own location.

Generally scientists distrust this idea for the very good reason that this is more or less the same argument used by Ptolemaic astronomy for arguing that the earth is the center of the universe and everything else revolves around us. Indeed, "common sense" was far more on the side of the Ptolemaic astronomers with regard to Copernicus' hypothesis that the earth revolves around the sun. There might be something "special" about our own location in the universe, but there also might be *nothing* "special" about it. When one considers the unimaginable vastness of the observable universe, the idea that humankind just happened to evolve in a "special place" in the universe does indeed seem highly improbable. Nothing in actual experience supports the idea there is anything unusual or "special" about the location of our solar system and there is no *scientific* reason to support a "special place" hypothesis.

A fundamental precept of Einstein's relativity theory is this: The laws of physics must be invariant to any change in coordinate systems. In effect this precept rules out *a priori* the employment of any "special observer" in the formulation of the laws of physics. Therefore if one wishes to have a mathematical model for explaining the structure of the visible universe, the form of that model must follow the rules laid down by Einstein's general relativity theory. This is why astronomical cosmology work takes the relativity theory as its most basic starting point. Naturally, this assumes Einstein's theory is actually correct, i.e. that the relativity principle is an objectively valid principle for connecting mathematical physics with Facet A. But this, too, appears to be a 'safe' assumption inasmuch as no experiment or observed data has been found to contradict the anticipations of the relativity theory for any well-formulated problem admitting to a non-vague prediction. The genesis of the relativity theory was itself based on epistemological considerations. This comes through very clearly from reading Einstein's original papers on the subject. It is worthwhile to repeat here what was said earlier in this treatise: the *practical* objective validity of relativity is vested in its role as a principle that dictates *rules* for the mathematical form that physics equations must follow. It prescribes rules to the *mathematics*, and physics is then bound to abide by the mathematical consequences of equations that conform to these rules *and* are not contradicted in actual experience.

In order to have any kind of relativistic theory of cosmology we require three things in order to establish the cosmological form of space-time. First, we must have a description of space-time geometry. In relativity theory this is expressed in terms of a "metric"; it is a mathematical construct that describes four-dimensional 'intervals' of space-time. (The four 'dimensions' consist of the usual three 'spatial' dimensions – up & down, left & right, forward & backward – and the 'dimension' of objective time). Second, we must have the Einstein field equation which describes the "actions of gravity". Finally, we have to have a description for the equations of state that characterize the bulk properties of matter and energy.

Each and every one of these necessary constituents of a cosmological theory requires of us a number of simplifying assumptions and many approximations. This is where speculation must lend a hand in formulating a relativistic cosmology. I think it is abundantly clear to any calm person that any attempt to find a complete and exact description of 'the universe' without making use of simplifying assumptions and approximations is utterly without any possibility of success. There are limitless examples of physics problems far simpler than 'the whole universe' where a "complete and exact" description and solution are unattainable in practice. Weinberg has provided a very nice illustration of this issue in [WEIN: 6-12] where he discusses the shrewd guesses and approximations Newton had to make in order to come up with his law of universal gravitation. He remarks,

> At this point, Newton stopped simplifying and solved the equations analytically. He had actually made numerous other simplifications, such as his consideration of each of the solar bodies as point masses. In each of these cases, he and his contemporaries were generally more aware of – and more concerned about – the simplifying assumptions than are many present-day physics professors who lecture about Newton's calculations. Students, consequently, find it hard to understand why Newton's calculation of planetary orbits is ranked as one of the highest achievements of the human mind. . . Newton was a genius, but not because of the superior computational power of his brain. Newton's genius was, on the contrary, his ability to simplify, idealize, and streamline the world so that it became, in some measure, tractable to the brains of ordinary men [WEIN: 11-12].

Newton had very good reason to be "more aware of and concerned about" his assumptions and simplifications; what he was attempting had never been done before. He had no 'case histories' available to give him confidence that he could 'get away with' making so many assumptions and simplifications. It was not until he *did* 'get away with it' (by producing a theory that stood in outstanding agreement with observation) that he finally knew he *had* 'gotten away with it.' To really appreciate Newton's genius we can make an estimate of just how much he simplified his problem by estimating the reduction in the number of equations he achieved with his simplifications. To completely describe a solar system containing on the order of 100,000 massive bodies (the sun, planets, asteroids, etc.) requires on the order of about $10^{30,000}$ equations. These equations are coupled and the system contains ridiculously more equations than we can solve. If we ignore all the "small masses" and only keep the 10 most massive objects, the number of equations is reduced to on the order of 1000. This we can solve with the aid of powerful computers, but Newton did not have this tool available. Then if we ignore the interactions except those between *pairs* of bodies we drop down to about 45 equations. Finally, if we ignore all the pair-wise interactions except those involving the sun, we get down to just 9 *separable* equations. Now *that's* simplifying! Newton's genius is shown by his ability to figure out a successful way to carry this out.

All the models of 'the cosmology of the universe' (and there have been more than one) must likewise simplify things no less tremendously than Newton did. It is a practical necessity and there is no way to avoid it. Let us look at some of the more important simplifications.

Models, Differential Equations, and Boundary Conditions

We'll start with a requirement peculiar to mathematical models described in terms of partial differential equations, namely the imposition of boundary conditions. As a relatively simple example we will take the equation

$$\frac{\partial^2 \psi}{\partial x^2} + a^2 \psi = 0$$

where ψ is a function that solves this equation, x is a coordinate in a one-dimensional 'geometry' for the problem, and a is some positive constant. (For one particular physical specification of a this equation is known as Schrödinger's equation for a particle in a one-dimensional box). The general solution for this differential equation is

$$\psi = A\sin(a\,x) + B\cos(a\,x)$$

where A and B are undetermined constants. In other words, we do not have one unique solution. We have an unlimited number of ψ functions that satisfy the differential equation.

In order to make the solution apply to some physical problem we have to find a way to put some kind of specification into the problem so that we can determine A and B. In mathematics this specification is called a *boundary condition*. As an example, suppose we have some reason to specify that ψ has a value of zero at $x = 0$ and at $x = L$. The $x = 0$ boundary condition tells us right away that $B = 0$. But the boundary condition at $x = L$ only tells us $A \sin(aL) = 0$. If we set $A = 0$ we get a trivial solution: $\psi = 0$. However, we can get non-trivial solutions if a takes on special values, namely those for which aL is an integer multiple of π.

Of course, this still leaves us with an unlimited family of solutions and we still do not have any definite value for A. We must therefore add *still more* specifications to the problem before we get a specific solution. For example, we might impose a *normalization condition*, e.g.

$$\int_0^L |\psi(x)|^2 dx = \int_0^L A^2 \sin^2(a\,x) dx = 1 .$$

Imposition of this condition gives us a definite value $A = \sqrt{2/L}$. However, we still have an unlimited number of solutions because there are an unlimited number of values for a that satisfy the differential equation, namely all values $a_n = n\,\pi/L$ where n is any positive integer. At this point a physicist will typically admit *all* these solutions but will find a way to parameterize the a variable in terms of some physical quantity. One popular way to do this is to make a some function of the energy of the system. In this case, we would say that our differential equation *plus* its boundary conditions require that the energy of the system be *quantized* to particular discrete values. The energy corresponding to $n = 1$ is then usually called the "ground state energy" of the system because it is the smallest possible energy for which ψ does not vanish.

Boundary conditions are often imposed by physical characteristics of the system being modeled. It is usually the case that physically accurate boundary conditions result in situations where a closed-form solution to the differential equation cannot be obtained. A prime example of this is provided by electromagnetics. Most engineering problems in electromagnetics require very accurate solutions for Maxwell's equations, and these solutions typically must be obtained numerically using a computer. There are specialists in this field whose work is dedicated almost entirely to finding efficient computer methods for solving the equations. Only a surprisingly few problems in electromagnetics, those involving "simple" or highly idealized geometries with symmetry properties that can be exploited, yield closed-form solutions. Standard textbooks on electromagnetic theory usually present the majority of these few important special cases.

A numerical solution to a problem is often viewed as not very satisfactory by scientists for the simple reason that such a solution tells us what the answer is in *this* case but tells us nothing at all about what the answer will look like under slightly different conditions with different physical parameters or slightly different boundary conditions. It is oftentimes important to know how sensitive the solution is to the different parameters of the problem; this is sometimes called "having a feel for the solution" or "understanding the physics of the problem." One method frequently used by researchers whose work requires computer solutions is to generate several different numerical solutions involving different sets of parameters and then to 'fit' these solutions with some function of the system parameters using statistical methods. Statisticians call this "finding the response surface of a model." Response surface methods provide one with a qualitative understanding of how the solution will change when the parameters of the system change and to which parameters the solution is most sensitive in different regions of the response surface. The two disadvantages of this method are: 1) it can be expensive and very time consuming to generate an adequate response surface; and 2) the response surface characterization is reliable only within the range of parameters used in obtaining it, and tends to rather quickly become very inaccurate for parameter sets outside the range used in obtaining the 'fit'.

Today's younger mathematicians and physicists are undeniably more comfortable with and "literate" in the use of computers than was the usual case thirty years ago. Even so, numerical solution methods of the sort just described are often seen as 'inelegant' and 'brute force.' Also, even when one resorts to computer methods for solving a problem it is not unusual for *some* simplification of the problem to be needed in order to obtain a tractable solution (i.e. one that does not require days, weeks, or months of computer time to generate one number). When one is dealing with differential equations belonging to the class known as 'linear' differential equations, it is oftentimes possible to adopt a different strategy. In this strategy the solution to problems involving complicated boundary conditions is approximated using known closed-form solutions to simpler cases as so-called "basis functions." The solution is approximated as some linear combination of these functions and the problem then reduces to one of finding the appropriate factors by which to 'weight' the combination of functions. One of the best known examples of this approach is used for studying the solid state physics of materials with covalent and ionic chemical bonds. It is called the "linear combination of atomic orbitals" or LCAO method. The 'basis functions' used here are the known solutions of the Schrödinger equation for the hydrogen atom.[3] The approximate solutions generated by this method are accurate to within about 20-30% of the 'exact solution' (as determined by comparing the theoretical results with the outcomes of laboratory measurements).

When the underlying differential equation is nonlinear the situation becomes considerably more challenging. It is an unfortunate fact that solutions for nonlinear differential equations are known for only a very few equations. In the vast majority of nonlinear cases no analytical solution at all is available, and here at least some recourse to numerical methods must usually be employed. In some cases it is possible to obtain approximate solutions, valid over some limited range of parameters, by approximating the equation over some parametric range with a simpler version of it. When a scientist speaks of using "perturbation methods" this is one example of this sort of approach, and it often can yield closed-form approximations to the solution. Here the main challenge is to determine over what range of 'perturbation' the approximation is accurate enough for the purposes of the analysis.

One property of nonlinear equations that must always be kept in mind is that these equations might have multiple, very different solutions. A point P at which the equation has multiple solutions in the vicinity of P is sometimes called a "branch point" and is sometimes called a "bifurcation point." The evolution of a solution as a function of time can be very, very different for small changes in parameters or variables about the neighborhood of P. An examination of whether or not this happens in any particular problem is very important in nonlinear dynamics.

The training scientists receive in mathematics is, understandably, largely devoted to linear equations for which uniqueness of the solution function can usually be taken for granted. This has a tendency to lead to an habitual assumption that if one knows a solution for one set of parameters, similar solutions will obtain for similar sets of parameters. In the case of nonlinear systems this assumption sometimes gets one into trouble. Complicating the issue is the fact that even if the differential equation has a unique asymptotic solution as the time variable goes to infinity, the corresponding difference equation required for computer analysis may not. Systems characterized by linear differential equations with constant coefficients have solutions (called the 'forced response' by mathematicians) that are independent of the initial conditions of the system. This does not always happen for systems characterized by nonlinear differential equations. Meteorology and turbulent flow fluid mechanics are two examples where this issue is prevalent.

One consequence of all this is something that has been known for a long time but not very

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[3] For an excellent treatment of this method see Walter Harrison, *Electronic Structure and the Properties of Solids*, San Francisco, CA: W.H. Freeman & Company, 1980.

well appreciated until the 1960s: Solutions to equations of this sort are sometimes extremely sensitive to the tiniest differences in parameters or initial conditions. Feynman once remarked,

> If water falls over a dam, it splashes. If we stand nearby, every now and then a drop will land on our nose. This appears to be completely random. . . The tiniest irregularities are magnified in falling, so we get complete randomness. . .
>
> Speaking more precisely, given an arbitrary accuracy, no matter how precise, one can find a time long enough that we cannot make predictions valid for that long a time. Now the point is that this length of time is not very large. . . It turns out that in only a very, very tiny time we lose all our information. . . We can no longer predict what is going to happen![4]

No one is particularly surprised when the weather report the evening before promises a bright and sunny day and instead it rains all day long. I have never quite understood why so many science and engineering textbooks fail to mention this issue, leaving the student with the false impression that the title "exact science" implies "exact predictability" from our mathematical models. Perhaps the fear is impressionable young people will over-generalize the shortcomings of mathematical theories and therefore not take theory seriously at all? I have students every semester for whom this 'prediction' would prove quite accurate. I have other students who have more faith in mathematics than a saint has in Christ, and for them the news that mathematical models have their 'real-world' shortcomings would be a crushing blow. But for at least the graduate student level and certainly for the post graduate level of training, overconfidence in mathematical models is not a good thing.

The Robertson-Walker Metric

Now let us look specifically at some details of cosmological models. Our primary interest here lies with the simplifications and assumptions that go into the making of these models. Most of the grandiloquent pronouncements of Big Bang cosmology are anchored to inferences of induction applied to a metric fundamental form used to describe the expansion of the universe according to the relativity theory. The dominant metric fundamental form in use today is one derived independently by Howard P. Robertson and Arthur G. Walker in the 1930s. It is known as the Robertson-Walker (RW) metric.

What is a 'metric fundamental form' in the sense in which that term is used in relativity and cosmology? The term itself comes from the mathematics of differential geometry (the branch of mathematics that includes such topics as Riemannian geometry). Suppose we wished to measure the length of some arc, for example the length of a fraction of the edge of a circle as shown in Figure 24.6.1. Referring to this figure, the differential length *ds* corresponding to the differential

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[4] J. Gleick, *op. cit.* pp. 430-431.

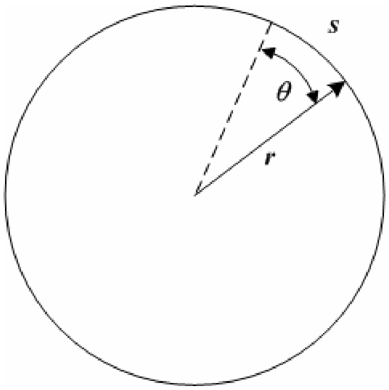

Figure 24.6.1: Example of an arc length on a circle of radius *r*.

angle $d\theta$ is given by $ds = r\,d\theta$, and so the arc length corresponding to an angle θ is simply

$$s = \int_0^\theta r\,d\theta = r\theta\,.$$

But how did we know $ds = r\,d\theta$ in the first place? This knowledge is what the metric fundamental form specifies. It is a function of the coordinates being used in the particular geometry such that integral expressions like the one shown above can be formulated for finding lengths on a surface, angles, surface areas, etc. Perhaps the most key property of the metric fundamental form, which I shall hereafter denote by the symbol ds^2 in order to save repeating 'metric fundamental form' over and over again, is that ds^2 must be a rule that is invariant under transformations of the coordinate system. (This requirement comes from the fact that we don't want our numerical answer to depend on an accidental choice of one coordinate system vs. another).

When our geometry is used to represent an objective space we can, without a too-egregious analogy, think of a ds^2 located at some particular coordinate in our geometrical space as specifying a sort of idealized 'infinitesimal' ruler for marking out a kind of distance (lengths, angles) for that location in the space.[5] At all other coordinate points immediately neighboring this point we can pretend to have similar ds^2-specified "rulers" such that by laying them end to end we can measure out arc lengths, etc. in our space. These arcs are taken to represent trajectories taken by particles or photons ("light particles") moving through our space. These arcs are given the rather poetic name "world lines" in the terminology of relativity theory and ds^2 is called a "fundamental world line element."

The relativity theory uses a four-dimensional geometry to describe "space-time" comprised of one time coordinate (t) and three 'space' coordinates (x, y, and z). Our ds^2 is defined so that quantities such as $s^2 = c^2t^2 - x^2 - y^2 - z^2$ (where c is the speed of light) are invariant under changes

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[5] Strictly speaking, this analogy requires small but not infinitesimal 'rulers'. To use the infinitesimal ds^2 we must invoke limit arguments from calculus.

in our coordinate system. If $s^2 > 0$ the world line is called “time-like”; $s^2 < 0$ is called “space-like”; and $s^2 = 0$ is called “light-like.” This third designation is meant to remind us that null values of s^2 correspond to space-time paths taken by light. These are called “geodesic lines.” They also correspond to trajectories taken by any material body not acted upon by any forces other than the “force” of gravity. (Whether or not gravity is a “force” is another question altogether, but “force of gravity” is common terminology and I will use it here without intending to imply any ontological pronouncement upon it). The general theory of relativity is, at its foundations, the theory for coming up with expressions for ds^2 such that the principle of relativity is satisfied.

The RW metric is an exact solution for Einstein’s equations under a particular set of assumptions. One of these, number 4, is the tame assumption that all observers have available to them instruments for making observations (clocks, telescopes, spectrometers, etc., etc.). The other three assumptions are the important ones. We will let Robertson and his student, Thomas Noonan, tell us about them. The first is the assumption of natural motion:

> The redshifts of galaxies at a given apparent magnitude deviate from the mean by a small amount. For redshifts as large as 10^{-1}, the scatter in redshift is only about 10^{-3}. . . It is difficult to escape the implication that there exists at each point in the universe a state of *natural motion*. With reference to a local coordinate system in natural motion, the motion of nearby galaxies are small and average out to zero. The possibility that the redshifts may not be due to motion alone does not affect the argument, because differential velocities will still produce differential redshifts. Objects in natural motion sweep out a non-intersecting congruence of *fundamental* world-lines.
>
> ASSUMPTION 1. The congruence of fundamental world-lines fills the universe.[6]

I would prefer a less cryptic way of expressing this assumption, but the idea is simpler than this description. We will find that the RW metric does not apply to whatever may be going on “locally.” Its main conclusion – that the universe is expanding – does not apply to events in your neighborhood. The universe may be getting bigger, but Brooklyn is not. The RW metric does not describe the orbits of the planets around our sun nor the fact that Andromeda and the Milky Way are moving towards each other.

Basically, assumption 1 permits “local gravitational effects” and says the sum total picture of the structure of the universe is made up of two constituents: local gravitational effects and an astronomical (“cosmological”) effect. The RW metric pertains only to the latter. This seems to be a reasonable assumption to make, but let us be aware of what it means for the mathematics. Invoking assumption 1 is the same as postulating that the non-linear differential equations of Einstein can be analyzed by perturbation methods and the overall result obtained by superposition

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[6] H.P. Robertson and Thomas W. Noonan, *Relativity and Cosmology*, Philadelphia, PA: W.B. Saunders Co., 1968, pg. 335.

of ‘local’ and ‘cosmological’ solutions. The RW metric ignores the former and treats the latter. For the universe one sees when one looks out the window, this assumption is very reasonable and I doubt if anyone will find much fault with it. On the other hand, it is not so clear how valid this assumption is for the nascent universe of the early Big Bang era. The picture of this universe as seen by Big Bang theory is an inference of induction – namely, one ‘runs the equations backwards in time’ to ‘see’ what the early universe is like. But, I ask, does this same induction not imply that eventually ‘local’ and ‘cosmological’ gravitational descriptions must ‘merge’? The RW derivation uses a mathematical trick (called the “thin sandwich conjecture”) to turn the four-dimensional system of Einstein’s equations into an easier-to-solve “3 + 1” form, after which the perturbation approximation is introduced by specification of certain conditions on the space-like part of the geometry. This can be and is done for the universe of ‘today’. But if we run the geometrodynamics ‘backwards in time’ until ‘local’ and ‘cosmological’ effects merge, is the projection unique or are there branch points? As I see it, the only way to check this is to do a *full* analysis of the nonlinear dynamics and find out. Just the fact that a number of *different* idealized ‘cosmologies’ have been worked out hints that the Einstein equations might contain branch points when we project backwards in time. But solving this problem will likely be very, very difficult.

The second assumption is the isotropy assumption:

> The galaxies give no indication that there are any preferred directions. The spatial density of galaxies shows no obvious gradient in any direction, and the orientations of spiral galaxies show no tendency toward alignment. Thus, for example, a space traveler on a long trip would lose all sense of direction and would be unable to return to his starting point, unless he kept a record of the specific galaxies and clusters of galaxies which he passed. Thus there is *large-scale* isotropy in the *visible part* of the universe. It will be assumed that there is *exact* isotropy *throughout* the universe.
>
> ASSUMPTION 2. The universe is isotropic.[7]

“Isotropic” merely means “the same in all directions.” Those of us who are not astronomers might be inclined at this point to say, “Whoa! Wait a minute! That isn’t true. Just look at the sky!” However, we would be wrong to make this objection. Our experience with the night sky is far more meager than an astronomer’s experience with it. There are statistical tests that can detect even small trends in random-looking data, and when these tests have been applied to observation data, the results appear to confirm there is no discernible ‘trend’ in the distributions of observable objects, assuming that we say “an object is an object” and make abstraction of any peculiar differences that distinguish, say, one galaxy from another or a galaxy from a quasar. This is what Robertson means by “large-scale isotropy.” Obviously from earth we can look in one direction and see the Milky Way, then look in an orthogonal direction and see a much “emptier” sky. We

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[7] *ibid*., pp. 335-336.

can travel down south of the equator and see a different collection of constellations. But these are examples of "local anisotropy" – a 'small scale' phenomenon, if one is willing to apply the term 'small scale' to astronomical phenomena.

We must be careful not to confuse the idea of "isotropy" with another idea we will come to in a minute, namely "homogeneity." Both words more or less mean "it's all the same" but the same in what sense? Put another way, precisely what observational data are we to say demonstrates "isotropy" rather than "homogeneity"? *The Penguin Dictionary of Science* defines "isotropic" as "equal or identical in all directions and thus invariant under rotation." Isotropy then means "having the property of being isotropic." This is in contrast to "anisotropic", which this dictionary defines as "a system that has different properties in different directions." This dictionary is less helpful when it comes to the word "homogeneous"; it says "homogeneous" means, "Literally 'the same all through'. For example a phase[8] is homogeneous if its composition is the same at all places." It is perhaps clear to the reader that these definitions do not draw a nice crisp distinction between these two terms other than to associate the geometrical idea of 'invariant under rotation' with the word "isotropic" and the more thing-like idea of "composition" with the word "homogeneous." Robertson uses the word "isotropy" in precisely this geometric sense, i.e. he uses assumption 2 in order to cast ds^2 in a geometric form the mathematicians say has rotational symmetry. Here a unit vector in the 'time direction' is precisely orthogonal to the three unit vectors that describe spatial 'hypersurfaces' and then the geometry of these hypersurfaces is made invariant under rotation as seen from any place in the universe. In other words, assumption 2 says "the geometry of space does not depend on which direction in space we look."

Again, I have no problem with this insofar as the universe I see looking out the window is concerned. But it is worth bearing in mind that the Einstein equations say the correct relativistic geometry does depend on the distribution of matter-energy, and so assumption 2 is predicated on a presumption of a property of this distribution *that must be completely independent of 'time'*. So, again, one must ask: What is to be the observational criterion of universal isotropy? It is not clear to me that the astronomical community speaks with one voice here. James Trefil, one of the pioneers of quark theory, uses an operational criterion, namely the quantity of microwave radiation measured by radiation detectors. Being a high energy physicist and a Big Bang expert, Trefil sees both an experimental verification of universal isotropy *and* a nasty implication for our physical understanding of the cosmos:

> When microwave detectors are flown high in the atmosphere in balloons or aircraft, the radiation is found to be isotropic to an accuracy of better than .01 percent. This means if we measure the

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[8] For example, solid, liquid, gas, and plasma; these are "phases of matter."

> photons coming from one direction of the sky and then turn 180° and perform the same operation, we will find that the two batches of photons are identical to within that accuracy.
>
> At first glance, this seems to be what we would expect on the basis of our intuition. Why should one portion of the sky be different from any other? But if you recall the origin of [the cosmic microwave background] radiation, you will remember that what we see now depends on the temperature of the region of space from which that radiation was emitted 500,000 years after the Big Bang. The problem is that radiation now reaching our detectors from opposite ends of the universe was emitted from sections of freezing plasma that were more than 500,000 light years apart. If this is true, how could it have happened that the two regions were at exactly the same temperature?[9]

This is known as "the horizon problem" in Big Bang theory.

Trefil's operational definition for 'isotropy' is better than none at all, but one should recognize that this operational definition is not precisely the same as Robertson's usage. In coming up with the solution giving ds^2, 'isotropy' is a geometrical quantity – a quantity of Facet B in Slepian's terminology – whereas Trefil's usage belongs to Facet A. It is not given *a priori* that these two quantities must be one and the same, nor is it clear (at least to me) what defines the *principal* quantity of Facet B to which Trefil's quantity corresponds.

I would like to understand this, and so I humbly ask the public relations wing of Big Bang theory, "Please explain to us very clearly what it is, precisely, we're talking about here; in what way does this construct connect to Margenau's 'Nature'? how is it congruent with the matter-energy conditions of general relativity early on in the Big Bang? and how is the Facet B principal quantity defined?" This surely cannot be asking for too much if the Big Bang is a *fact* rather than an hypothesis.

The third assumption is the homogeneity assumption:

> The spatial density of galaxies shows no obvious large-scale inhomogeneities. Even the clusters of clusters of galaxies, if they exist, seem to be distributed randomly throughout the visible universe. Thus the *visible* universe is, *to a first approximation*, homogeneous. It will be assumed that the *whole* universe is *exactly* spatially homogeneous. This assumption is to be used in the somewhat stronger form:
>
> ASSUMPTION 3. The view of the universe in its entirety, obtained by any one observer in natural motion, is identical with the view of the universe in its entirety obtained by any other contemporary observer in natural motion.
>
> Assumption 3 is known as the *cosmological principle*. Clearly this assumption cannot be used until a measurement of time has been defined.[10]

At the risk of being accused of nitpicking, this is not one assumption. It is two. The stated assumption 3 is the hypothesis, and insofar as the reason for its introduction is to ward off a theory that requires the earth to be "a special place" in violation of the relativity principle, there is

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[9] James S. Trefil, *The Moment of Creation: Big Bang Physics from Before the First Millisecond to the Present Universe*, Mineola, NY: Dover Publications, 2004, pg. 44. (The Dover edition is a republication of the original 1983 book published by Collier Books).

[10] Robertson and Noonan, *op. cit.*, pg. 336.

no big quarrel with it. What is perhaps less immediately clear is that this assumption is the basis for the *actual* condition used in deriving ds^2, namely the homogeneity assumption. It is a form of boundary condition used in deriving the geometry of the space-like terms in the three dimensional 'hypersurfaces' defined at each instant of 'cosmic' time and for deriving a rule by which hypersurfaces evolve over time.[11] Imagine the surface of an inflated balloon is a sphere. The surface of the balloon is analogous to the space-like hypersurface at some time t. Now suppose we blow a little more air into the balloon. The balloon gets bigger but its surface is still a sphere. This new sphere is analogous to the hypersurface at time $t + \Delta t$. *Geometrically* speaking, every little region on the surface of a sphere is like every other. This is what homogeneity implicates.

The universe one sees looking out the window is obviously not *materially* homogeneous. Most of the objects in it are clumped into galaxies with enormous stretches of nearly-empty objective space in between. We must, however, be a bit careful here because what the RW derivation pays strict attention to is spatial homogeneity on a Riemann surface of constant 'cosmic time.' This is more than a little difficult to visualize, but the point is this: there may be 'near-emptiness' between objects at the same radius r but there are no apparent 'alleys' along a given 'look direction' that are 'empty' traveling in the r-direction. The significance here is that we must pay attention to what Robertson means by the phrase "to a first approximation."

> Astronomical observations reveal that the universe is homogeneous and isotropic on scales of ~ 10^8 light years and larger. Taking a "fine scale" point of view, one sees the agglomeration of matter into stars, galaxies, and clusters of galaxies in regions of size ~ 1 light year, ~ 10^6 light years, and ~ 3×10^7 light years, respectively. But taking instead a "large-scale" viewpoint, one sees little difference between an elementary volume of the universe of the order of 10^8 light years on a side centered on the Earth and other elementary volumes of the same size located elsewhere.
>
> Cosmology . . . takes the large-scale viewpoint as its first approximation; and as its second approximation, it treats the fine-scale structure as a perturbation on the smooth, large-scale background.[12]

This is, pardon the pun, an astronomical chunk of space. Treating the problem by the perturbation approach is, as mentioned earlier, okay provided that the underlying differential equations do not have nasty and unpleasant branch points and multiple solutions in the region where the analysis is being carried out. There is an abundance of observational evidence in hand that says: for the universe as it appears today there is no apparent problem with doing this. Mathematically this is more or less treating the large-scale behavior as an 'average' and the 'fine-scale' perturbations as zero-mean deviations about that average. One can be suspicious whether this is still okay for a

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[11] The mathematical procedure used for this is not easy to describe without using a good deal of higher level mathematics. The reader interested in these details can consult Charles W. Misner, Kip S. Stone, and John Archibald Wheeler, *Gravitation*, San Francisco, CA: W.H. Freeman and Co., 1973, pp. 505-556.

[12] Charles W. Misner, Kip S. Stone, and John Archibald Wheeler, *Gravitation*, San Francisco, CA: W.H. Freeman and Co., 1973, pg. 703.

nascent universe too small to have regions 10^8 light years on a side. Using the balloon analogy from above, the question is "does the balloon stay spherical as it is deflated if its material composition differs a bit from place to place?" Getting back to assumption 3, *geometrical* homogeneity seems to require *material* homogeneity, so what is it that the model is assuming?

> By taking the large-scale viewpoint, one can treat galaxies as "particles" of a "gas" that fills the universe. These particles have internal structure (stars, globular clusters, etc.); but one ignores it. The "particles" cluster on a small scale . . . but one ignores the clustering. To simplify calculations, one even ignores the particulate nature of the "gas". . . One removes the particulate structure of the gas from view by treating it in the perfect fluid approximation. Thus one characterizes the gas by a 4-velocity, $\boldsymbol{u}$ (the 4-velocity of an observer who sees the galaxies in his neighborhood to have no *mean* motion), by a *density of mass-energy*, ρ (the smoothed-out density of mass-energy seen in the frame with 4-velocity $\boldsymbol{u}$; this includes the rest mass plus kinetic energy of the galaxies in a unit volume, divided by the volume), and by a *pressure*, p (the kinetic pressure of the galaxies).[13]

Obviously one is now modeling based on an analogy. This should, of course, immediately raise suspicions since inference by analogy has subjective but not objective validity. But the more immediate question is: What happens if we throw out all this nice *material* homogeneity? Do we then lose the *geometrical* homogeneity and isotropy that comes from it?

This has in fact been studied. There have been various models, e.g. the 1921 Kasner model.[14] In the case of the Kasner model an exact solution under a different (asymmetric) set of conditions was obtained. This model had a number of interesting and peculiar properties but, unfortunately for it, it also made a prediction. If any blackbody radiation were emitted at some time t and never subsequently scattered, later observers should see blue shifts in one pair of places in the sky and red shifts in most other directions. We have not seen any such thing, and this is strong evidence that the Kasner model is not a correct description. An interesting variant of this model, using the fiction of a 'pressureless' fluid, was put forth in 1958.[15] What is particularly interesting about that model is this: Although it started out with asymmetric initial conditions, it's ds^2 later evolved asymptotically to a homogeneous, isotropic model corresponding to a Euclidean geometry. Thus, while one must still wonder how well a perturbation analysis could hold up in a tiny, nascent universe, this model seems to imply it is not strictly necessary for the universe to start out in a homogeneous and isotropic state in order to end up in one later. Misner et al. comment:

> Accepting the agreement with observations, we want to understand *why the laws of physics should demand (not merely permit) a universe that is homogeneous and isotropic to high accuracy on large scales*.[16]

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[13] *ibid*., pg. 711.

[14] Kasner, E., "Geometrical theorems on Einstein's cosmological equations," *Am. J. Math. 43,* 217-221 (1921).

[15] Schücking, E. and O. Heckmann, "World models," in *Onzième Conseil de Physique Solvay*, Êditions Stoops, Brussels, pp. 149-158 (1958).

[16] Misner et al., *op. cit.*, pg. 800.

This is surely a reasonable question in regard to any alleged origin of the universe. The important point for us is this. Excepting the two cautions about projecting the RW metric all the way back to "the beginning of time" and what exactly is meant by "isotropy", there is strong observational evidence and some amount of mathematical evidence that both point to the same conclusion: The RW metric appears to be a reasonable 'large-scale' description of the cosmological dynamics in play *today* in accordance with the general theory of relativity. We do not, in particular, have to view the homogeneity assumption with great alarm because it seems to be mathematically possible to *evolve to* a reasonable approximation to this condition from more than one set of assumed initial conditions. Most particularly, **we do not have to require the RW metric to be extendable all the way back to a Big Bang to justify using it in cosmology *today***. But its present aptness does not prove it is a description of how things were 15 billion years ago.

So, without further ado, here it is[17]:

$$ds^2 = c^2 dt^2 - R^2(t) \cdot \left[\frac{dr^2 + r^2\left(d\theta^2 + \sin^2(\theta) d\phi^2\right)}{\left(1 + kr^2/4\right)^2} \right].$$

This expression is in terms of the observer's reference frame (in spherical coordinates). The constant c is the speed of light, and k is a number that determines whether the universe has a "flat" geometry ($k = 0$), a spherical geometry ($k = 1$), or a hyperbolic geometry ($k = -1$); no other values for k are mathematically possible under the derivation of the RW metric. The time variable t is the time measured by the observer's clock and is called the "proper" or "cosmological" time.

The scale factor $R(t)$ is called "the radius of the universe at time t" and this term is at the heart of Big Bang discussions. It is a function of the density of the "gas", ρ, the "pressure of the gas", p, and another term, Λ, called the "cosmological constant." It is determined from Einstein's field equation, which describes the action of gravity as a function of matter-energy in space-time. More specifically, $R(t)$ is determined by a system of coupled, non-linear differential equations. If we let $\dot{R} = \partial R / \partial t$, $\ddot{R} = \partial^2 R / \partial t^2$, and $\dot{\rho} = \partial \rho / \partial t$ then the equations determining $R(t)$ are:

$$3\left(\frac{\dot{R}}{R}\right) = 8\pi G \cdot \rho - \frac{3kc^2}{R^2} + \Lambda c^2,$$

$$\frac{\ddot{R}}{R} = -\frac{4\pi G}{3}\left(\rho + 3\frac{p}{c^2}\right) + \frac{\Lambda c^2}{3},$$

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[17] There are other mathematically equivalent ways of expressing the RW metric. For an example see Peter Coles, "The state of the universe," *Nature*, vol. 433, Jan., 2005, pp. 248-256.

$$\dot{\rho} = -3\left(\frac{\dot{R}}{R}\right)\left(\rho + \frac{p}{c^2}\right).$$

Here G is Newton's gravitational constant.

There are a number of quite different results obtained for $R(t)$ depending on the chosen parameters of these equations. I will comment here that none of these parameters are determinable except as secondary quantities of Facet B. Some of these results are surprisingly simple. Some of them allow an $R(t) = 0$ for one or more values of t. These are the solutions that permit one to talk about a Big Bang in which everything in the universe originated as a "vacuum fluctuation" (the cosmologists' miracle event) at a singularity. (Big Bang cosmology's colorful talk about so-called "dark energy" and "dark matter" pertain to these cases). Other results do *not* admit $R(t) = 0$ as a solution. One family of solutions, called a deSitter universe[18], permits Hubble's constant (which describes a relationship between Doppler red shift and the putative distance to the observed object) to be a non-zero *constant*. More generally the Hubble "constant" is not a constant but rather has to be defined in terms of the ratio $\dot{R}/R$.[19] Furthermore, because this definition of the Hubble constant is made entirely in terms of parameters in the RW metric model(s), it cannot (by definition) take into account any perturbation contributions, i.e. it can deal only with the 'large-scale picture' and not with any 'local picture'.

I think one can see from the discussion in this section that the RW metric is a *starting point*. It does not prescribe or favor any *one* particular model obtained as a consequence of some particular set of *hypotheses* made to determine model parameters. Here is where it becomes especially important for us to note and keep in mind that *all* the parameters in the RW metric systems with the exception of c (which is a principal quantity of Facet B) are nothing else than *secondary* quantities of Facet B. **They have *no* ontological import for Facet A**. The best **and only** thing one can do **with objective validity** is to say: Such-and-such a set of parameters with such-and-such a model gives us *the best fit* to observations of the universe ***as it appears today***. Until and unless it is proven there is no branch point in the underlying Einstein equations as $R(t)$ is pushed back towards zero (or towards $t = 0$), we do not *know* how far we can "run the equations backwards" nor whether we must "see" a Big Bang as a *necessary* initial condition.

The universe described by the RW metric is illustrated in Figure 24.6.2 for two epochs in time t. Figure 24.6.2A depicts an earlier point in time, figure 24.6.2B depicts a later point. An important point to note in this figure is that the size of the "galaxies" occupying the corners of the

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[18] Robertson, *op. cit.*, pp. 365-369.

[19] Misner et al., *op. cit.*, pp. 730-732.

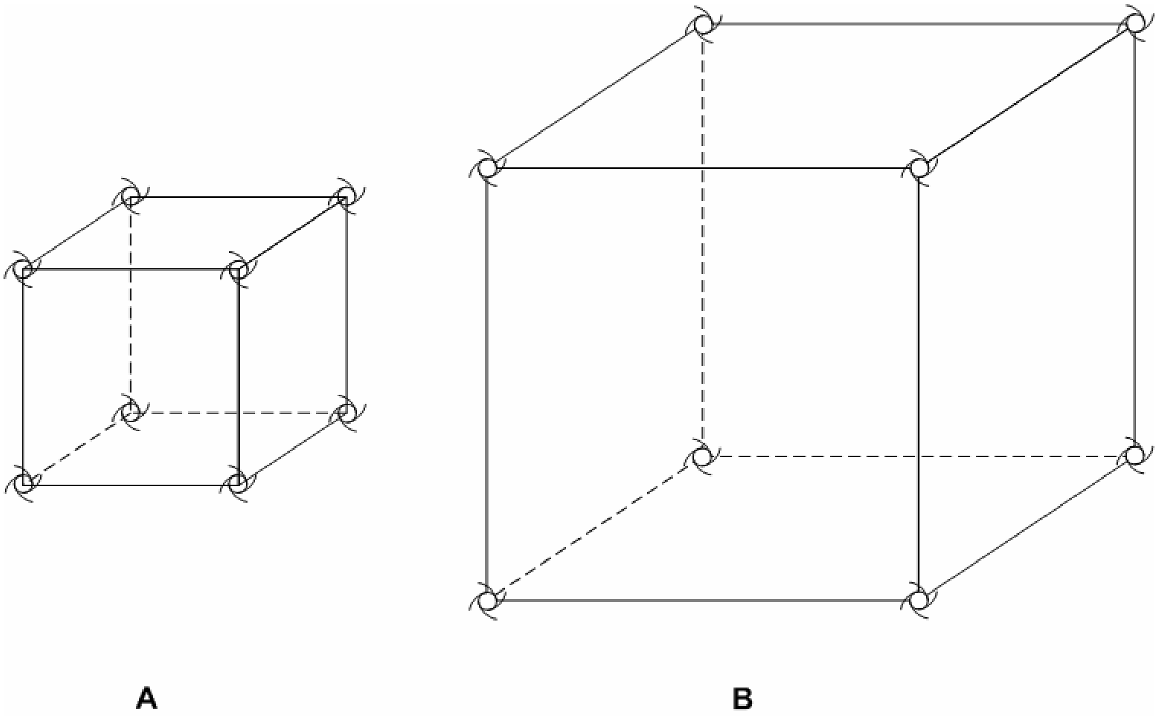

Figure 24.6.2: Robertson-Walker Universes. (A) Large-scale geometry of the universe at an earlier point in time, *t*. (B) Large-scale geometry at a later point in time. The corners of the cubes denote RW coordinate points. Note that the "galaxies" located at these coordinates do not change size as the universe expands. This is because local metrics are determined by the perturbation part of the Einstein equations and not by the part described by the RW metric.

cubes (the RW coordinate points) do not change size as the distance between them changes. This is a consequence of the "local" characteristics of the relativistic equations, i.e. the perturbation part not described by the RW metric. The RW metric only describes the "large scale" geometry of the model. The large-scale universe will look the same to every observer located at any of the cube edges. Each will see an expanding universe with himself apparently at the center of the expansion. What Robertson called the "natural motions" of objects near each other in the small scale is not depicted by the illustration. For instance, if the Milky Way were located at the lower left corner of the cube, the Andromeda galaxy (which is part of this "local picture" and also located in the lower left corner) could be observed to be getting closer to the Milky Way, presaging a future collision between the galaxies.

§ 6.4 Big Bang Enthusiasm

The preceding section has reviewed the mathematical underpinnings of relativistic cosmology. It is an impartial presentation and from an epistemology-centered metaphysic there is nothing objectionable in the pure mathematics with the exception of the implied inference of induction that we may take the definition of derivatives as valid when we extrapolate to a zero interval *h* rather than as a limit *approaching* but never actually *reaching* $h = 0$. But even this extrapolation is okay provided that we treat the quantities involved in this operation as secondary (and never principal) quantities of Facet B and *never* make the mistake of regarding them as quantities belonging to Facet A. The mathematics presents us with possible **models**, and there is nothing wrong with this either. Models are a practical necessity in science. Indeed, we saw earlier in this treatise the conclusion, coming out of Piaget's sixty years of research, that the idea (not the notion) of "necessity" itself *arises from* models we develop for understanding 'the world.' Models integrate our concepts of experience and make for these concepts the idea of a system.

But when we forget that our models *are* models and make a habit of regarding them *as being* Facet A rather than merely a *description* for understanding Facet A, this is scientific *enthusiasm*. This enthusiasm is transcendent, not transcendental, and carries one off into Kant's "broad and stormy ocean, the true seat of illusion." In the Transcendental Dialectic of *Critique of Pure Reason* Kant pointed out two errors in reasoning by which judgmentation through ratio-expression (pure speculative Reason) commonly casts us adrift on the ocean of illusion. One way, which produces transcendental antinomies, is this: That we make our ideas either **too big** or **too small** for Nature. When an inference of judgment leads us to ideas of Objects beyond the horizon of possible experience in order to arrive at an idea by which we can complete the series of conditions, our idea is too big for Nature. But when we cut off the regress in our synthesis, as e.g. occurs when we say, "This thing is a simple substance," our idea is too small for Nature (for Reason demands the unconditioned in this synthesis, and we cannot obtain this for *empirical* Nature). Between the idea that is too big and the one that is too small stands the *noumenon* at the boundary line of the horizon of experience, and here our idea can only be *practical* (practical Standpoint) and not *theoretical* (theoretical Standpoint).

The other way to err, which also produces transcendental antinomies, comes from failing to note the difference between a logical negation (A is-not B) and a logically infinite judgment (A is not-B). The negative judgment sets up contradictory propositions and forces us to choose one or the other. The infinite judgment, in contrast, sets up contrary propositions and from here we can seek a *resolution* (e.g. a determining condition). Mistaking the latter for the former carries us into irresolvable antinomy.

There is a third way to err, and this one leads us into paralogisms of reasoning. It is simply this: Failing to note the distinction between what is merely a logical division of our Objects and what is a real division of our Objects. When the former is mistaken for the latter, we inevitably will find ourselves speculating on things we can never possibly *know* with objective validity. Mistaking a model (Facet B) for a thing (Facet A) is an example of this sort of enthusiasm for paralogisms. The simple fact is: There are questions Reason drives us to pursue which are **formally undecidable** from the theoretical Standpoint.

Finally, we can err by mistaking the *subjective* validity of inferences of reflective judgment for *objective* inferences of ideal Objects. We do this when we forget that our knowledge of things is and can only be knowledge of their appearances and an understanding of their *Existenz*. Objectively valid inferences of the *Dasein* of things stops at the horizon of possible experience. Any ideas of *Dasein* beyond the point of this *noumenon* can never have objective validity, and the objective validity of our knowledge of the *Dasein* of a *noumenon* at the horizon of experience is always and only *practical* objective validity. When we reify space and make it into a *thing*, when we reify time and make it into a *thing*, when we forget that 'to be real' means nothing else than a particular condition in one's manifold of concepts and requires *real context*: when we take any of these actions of judgmentation we have committed this error and we have acted as *transcendental realists* instead of acting as *empirical* realists. Realism in science must always be the latter, never the former.

If the scientists writing books and articles on Big Bang cosmology actually mean what they tell us literally – and I am convinced that they do – then the Big Bang cosmologists are making all these errors. Let us look at some of the research questions involved in cosmology.

The Redshift Controversy and Accretion Disk Astrophysics

The redshift phenomenon is a very important observational tool for astronomers. We must accept redshift as a fact and as part of Facet A; we must either accept its Doppler shift explanation as a principal quantity, or else we must conclude that distant astronomical objects are made up of chemicals utterly unknown to us. The latter assumption appears to have no ground for objective validity and, if it were true, it would mean there is something seriously and fundamentally wrong with our understanding of chemistry and quantum physics. In view of the great scope of success both these sciences have enjoyed, a person would have to be an enthusiast's enthusiast (commonly called a "crackpot") to argue against the Doppler explanation for redshift.

Hubble's law is another matter. The most commonly accepted and used version of Hubble's law holds that the amount of red shift is directly proportional to the distance of the object from Earth. Now, this is a Facet B model of the phenomenon, and for "nearby" objects (objects close enough to make it possible for distance estimates to be made using Cepheid variable stars) there appears to be a reasonable and objectively valid ground for this model. The more distant the object, however, the more speculative is the assertion that the standard Hubble's law model is correct. This is because it is technically very, very difficult to measure distances on the RW scale of observations *and* the theoretical models of the form of the correct Hubble's law depend on *which* of the mathematically possible forms of $R(t)$ one uses. It also relies on an assumption that the models for Type 1a supernovae are reliable enough that no unknown systematic error is introduced when we use these phenomena as "standard candles" for assessing extreme distances.[1]

A Type 1a supernova is thought to occur in a binary star system when accretion matter from the companion star falls onto a white dwarf and the extra mass so acquired exceeds what is known as the Chandrasekhar limit (the theoretical stability limit, beyond which a white dwarf will collapse and explode). There are a variety of models attempting to provide explanations for the mechanisms of Type 1a supernovae, and this must temper our confidence that our knowledge of the process or processes involved is complete enough to remove any possibility of systematic error. Scientists who specialize in accretion theory do not claim to have any finished model of this process. They put forth the models, they discuss the pros and the cons, and cite observational evidence where the model mechanisms appear to fit and where they seem to be vulnerable.

> Most models for Type 1a supernovae involve in one form or another an accreting carbon-oxygen white dwarf in a binary system. This is a consequence of the fact that exploding white dwarfs are capable of producing ejecta abundances, as well as expanding envelope dynamics, which can lead to synthetic spectra which agree quite well with observations . . .
>
> It is, however, very questionable whether a white dwarf can be driven to the Chandrasekhar mass by the accretion of hydrogen rich material . . . This is a consequence of the fact that periodic mass ejections during nova eruptions can cause even a decrease in the white dwarf's mass if the accretion rate is below some critical value . . . The last conclusion seems to be supported by observations of nova ejecta which all show enrichment in material from the underlying white dwarf. Some fraction of Type 1a supernovae may still result from symbiotic stars which accrete at a rate above the critical value . . . or from recurrent novae, in which the accreting white dwarf is probably very close to the Chandrasekhar limit . . . The difficulty in accounting for the statistics of Type I supernovae in terms of hydrogen accreting white dwarfs has led to a scenario which involves the coalescence of two white dwarfs . . . with a total mass exceeding the Chandrasekhar limit.[2]

Current models do support the empirical data that suggests Type 1a supernovae are suitable "candles". Still, the state of our present understanding about accretion processes *is* incomplete. Distance measurement based on Type 1a supernovae must be regarded as reasonable hypothesis but not a fact firmly enough established to rule out any possibility of unknown systematic error.

Furthermore, there remains a nagging possibility that not all redshift is due merely to the Doppler effect. Although the majority consensus is against it, there are some troubling observations that led a few astronomers to hypothesize an anomalous redshift phenomena might exist. Probably the most well-known proponent of this view is the controversial astronomer

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[1] To appreciate the importance of the measurement problem see Wendy Freedman, "The Hubble constant and the expanding universe," *American Scientist*, vol. 91, no. 1, 2003, pp. 36-43.

[2] Mario Livio, "Merging white dwarfs, disk formation, and Type I supernovae," in *Theory of Accretion Disks*, F. Meyer et al. (eds.), NATO ASI Series C: Mathematical and Physical Sciences, vol. 290, Dordrecht, the Netherlands: Kluwer Academic Publishers, 1989.

Halton Arp. Dr. Arp is best known for his *Atlas of Peculiar Galaxies* and his arguments in the early 1970s that at least some quasars were much closer than general opinion held. On the whole, I personally tend to agree with those astronomers who think Arp's anomalous redshift probably does not exist (is not physically real). But this does not mean I think the astronomy/astrophysics community's counterarguments are altogether objective. For example,

> The cosmological interpretation of the redshifts of active galactic nuclei, which was a source of intense controversy in the early stages of quasar research because of the large energies implied, is now almost universally accepted. Doubters by and large do not accept the standard cosmological picture; but this degree of doubt is unprofitable.[3] Here we will give several arguments in support of the exclusively cosmological origin of the redshifts on the basis of standard cosmology. . .
>
> Attempts have been made to support a non-cosmological interpretation of redshifts with the claim that associations of galaxies and quasars at *different* redshifts are found with probabilities far higher than chance coincidence. Quasars might then be objects ejected from relatively nearby galaxies with at least part of the redshift attributable to Doppler motion. Early attempts[4] suffered from *a posteriori* statistics, associating improbabilities with events only after they had been found. More recent investigations also appear to be less than convincing. In any case, such an argument cannot overcome the energy problem for those objects in which the redshift is indisputably cosmological, so, if valid, it would require two populations of rather different types of object with the same observed properties, and this is generally held to be unlikely.[5]

If the subjective objection to "*a posteriori* statistics" merely serves as a reminder that statistical inferences can 'show' associations where none actually exist, it is a caution well noted. But if the objection is that Arp should have predicted the observations before the fact, this is absurd. To raise a scientific concern regarding an *habitual* way of conducting scientific business under a current paradigm based on "*a posteriori* statistics" is nothing else than an erudite way of saying, "Wait a minute. Something doesn't add up here." We would make very few new discoveries (capable of commanding objective validity) if we ignored observations merely because we didn't anticipate them. As for "two populations of rather different types of object", saying "this is generally held to be unlikely" is not a refutation. After all, there are two types of Cepheids.

Astronomy labors under the considerable disadvantage of being a non-experimental science. Like geology and paleontology, astronomy must work with whatever data is accessible to observation and must then offer explanations as best it can based on known physics. It has the additional disadvantage that its measurement techniques and methods are very hypothesis-laden

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[3] It will come as no surprise that your author does not agree with this particular statement at all. I think it very, very, very likely that most redshift observations *are* 'cosmological' in origin, but I do not think any one 'standard model' is well enough established to qualify as a fact. And even if this were so, there is still the issue of what perturbation effects there might be with regard to redshift phenomena, and *here* there is no 'standard model' at all. Doubt is only "unprofitable" if one uses it as an excuse for lazy skepticism.

[4] i.e., Dr. Arp's speculations.

[5] Juhan Frank, Andrew King, and Derek Raine, *Accretion Power in Astrophysics*, 3rd ed., Cambridge, UK: Cambridge University Press, 2002, pp. 220-221.

and that the theory it must employ is itself largely non-verifiable in a laboratory setting (which is not true of most of the rest of physics). This was Dr. Arp's main point in the original "how far away are quasars?" controversy.

> Because there is no other parameter besides redshift that is easily observable in a faint, featureless galaxy, the custom of assigning the distance to such a galaxy according to the size of its redshift has become established. If a galaxy has a faint apparent magnitude for its redshift, we say it is underluminous or a dwarf, and the reverse if it is apparently bright for its measured redshift. I wish to emphasize that there is no way of ever producing any discordance with the redshift-distance relation for even one single object when operating from the base of current assumptions. This is true because no matter where a galaxy point falls in the redshift-apparent magnitude diagram its position can be explained in terms of high or low intrinsic luminosity. For example, the quasars fall generally above the Hubble line in the redshift-apparent magnitude diagram, but they are not concluded to have excessive redshift – they are instead said to have excess luminosity.
>
> If, on the other hand, we wish to test the hypothesis that redshift is always caused by distance, we must find some different method of measuring distance. There are only two methods of measuring distance directly. One is by means of clustering or grouping. If we see a group of galaxies clustered together on the sky, we may conclude that they form a physical group at the same distance from us. This is essentially a statistical criterion of distance. That is, we must be able to show that for any objects assigned to this cluster the chance is small for them to be background or foreground objects accidentally projected into the apparent area of the cluster. In the past, of course, any discordantly high redshifts measured in a cluster were simply assumed to be background galaxies without any further investigation.[6]

I would say "in the present as well" with regard to this last statement. Arp makes a valid point here, but he was unable to mount a convincing enough statistical criterion to sway the majority of his fellow scientists. He was not able to mount a convincing enough criterion to sway your present author. Still, against a background of a universe apparently isotropic and homogeneous, he did make note of a few intriguing facts that seem at least a little difficult to reconcile with the "homogeneous and isotropic" picture.

> Although in most cases it is difficult to form an opinion as to whether a given quasar is associated with a large nearby galaxy or with a smaller peculiar or companion galaxy in the neighborhood, it appears possible to get a general idea of the distances of different kinds of quasars. The brightest apparent-magnitude quasars, which usually have redshifts in the range $z = 0.2$ to $z = 0.5$, and also the highest redshift quasars, with $z = 1.8$ or greater, seem to fall preferentially in the south galactic hemisphere.
>
> That, of course, is the direction of the Andromeda Nebula. M31 is the dominant member of the Local Group of galaxies of which our own Milky Way is a member. The distribution of the bright apparent-magnitude quasars, from 40° to 60° around the position of M31 on the sky, is such that this category of quasars must be related to M31 and the Local Group of galaxies. The remaining quasars, those with redshifts between about $z = 0.5$ and $z = 1.8$, must then fall in the remainder of the supercluster of galaxies of which our own Local Group is just a small part. This is confirmed by the fact that the distribution of these latter kinds of quasars is richer in the northern galactic hemisphere, which is just the direction of the center of the local supercluster. In fact, the local supercluster center and anticenter are one of the most important concepts in understanding the distance and distribution of objects in space around us. It is known that the number of bright galaxies is far greater in the

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[6] Halton Arp, "Evidence for discordant redshifts," in *The Redshift Controversy*, George B. Field, Halton Arp, and John N. Bahcall, Reading, MA: W.A. Benjamin, Inc., 1973, pg. 17.

> north galactic hemisphere, toward the supercluster center. It is now also known that the quasars with the largest apparent radio diameters show an excess in this direction. As mentioned, it is readily ascertainable that the optical quasars are more numerous in this direction. Recently, asymmetries in the radio-source counts have been shown to exist between the north and south galactic hemispheres. On the conventional redshift assumption, these latter counts are supposed to reflect the conditions out toward the edge of the universe, but, like quasars, they instead show relationships to the relatively local supercluster.[7]

Arp is arguing that sheer random chance does not favor the asymmetric distribution of quasar characteristics observed – especially if the quasars are 'out at the edge of the universe' – and so if the distribution is unlikely to be due to random chance, then there must be a mechanism for producing this asymmetry. Since objects 'at the edge of the universe' would fall within the region modeled by the RW metric and the 'standard cosmological model', the observable asymmetry is indeed puzzling. However, Dr. Arp's conclusion – that the explanation "must" be that the quasars therefore are more 'local' – is an error of the logical infinite vs. logical negative type mentioned above. Put another way, he sees the situation as a case of far-away vs. local, and if the statistics appear to argue "quasars are-not far away" then "quasars are local" is the only alternative left. He should have said, "quasars do not-fit-the-homogeneity-assumption," an infinite judgment.

It seems, at least to me, that perhaps Dr. Arp did not find the argument quoted above convincing enough to stand on its own legs unaided. He also looked for, and thought he had found, reinforcing evidence for his 'local quasars' hypothesis. Like most astronomers, Arp assumed that the only type of interaction possible between astronomical objects is gravity. He looked for groups of objects that appeared to exhibit gravitational interaction – implying that they were 'associated' ("close to") one another. He sought this from evidence of "filaments" appearing to link the objects and from apparent orientations that he attributed to their gravitational interaction. Figure 24.6.3 is an example of the sort of observation to which he made this attribution.

One weakness in Dr. Arp's case – and it is a substantial one – is that in a number of cases his reported observations could not be verified by other astronomers. Put bluntly, they did not see what he saw when they conducted their own observations. In those cases, standard scientific practice would, should, and did discount unverifiable reports. The observation shown in figure 24.6.3 was one such case. But there is another weakness, one considerably more subtle. It lies in the assumption that peculiar shapes of galaxy pairs must be a consequence of gravitational interaction.

The reasoning behind this assumption is simple. Astronomers tend to assume that the objects they observe are electrically neutral. The thinking is that charged particles (ions, electrons) would

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[7] *ibid*., pp. 30-31.

Figure 24.6.3: Arp's red-sensitive photograph of Stephan's Quintet. Arp concluded that galaxies NGC 7320 and NGC 7318, shown in this photograph, were gravitationally interacting on the basis of the shapes of these galaxies and the apparent presence of "filaments" running between them. NGC 7320 has a redshift of 800 km/s, whereas NGC 7318 has redshift of 5700 and 6700 km/s. Arp saw this as evidence for anomalous redshift. Other astronomers were unable to verify the filaments in this photograph.

have long ago combined, thus eliminating the presence of any significant amount of charged matter capable of influencing the formation, shape and orientation of galaxies. In 1973 there was no evidence to the contrary, at least so far as the general opinion was concerned.

That is not true today. In recent years astrophysicists who study the formation and evolution of galaxies have made a number of theoretical strides based on better computational capabilities for simulating the consequences of theoretical proposals. One such finding has to do with the theoretical consequences the presence of charged gases have on the dynamics of galaxy formations. The "new" accretion physics has been built up rather gradually. One piece of evidence has been drawn from observations of gas jets being ejected from the center of newly-forming stars perpendicular to its accretion disk.[8] It is presently thought that these jets are strong evidence of the presence of magnetic field interaction with matter in the accretion disk. Magnetic fields do not interact with charge-neutral matter.

Another piece of the picture comes from studies of intergalactic gas. The regions between galaxies are regions of high, but not total, vacuum. Matter density, in the form of gas, is the

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[8] see Thomas P. Ray, "Fountains of youth: Early days in the life of a star," *Scientific American*, vol. 283, no. 2, August, 2000, pp. 42-47.

lowest between galaxies of any place we know. Nonetheless, various observation data now strongly imply that intergalactic gas is not entirely charge-neutral. Rather, there seems to be processes of re-ionization at work, with ejection of charged matter from the galaxies into the 'intergalactic medium'. Furthermore, there is data suggesting that galactic clusters are embedded not in a charge-neutral medium but, rather, a hot plasma.[9]

It was long assumed that gas was too light and too low-density to have any significant effect on galaxy dynamics. The story was different, of course, for star formation because, after all, the theory is that stars form from accretion disks of gas. However, new studies of accretion processes in star formation paint a different picture. For a long time the prevailing view of star formation has been that it is a gravity-driven process involving cold, charge-neutral gas/dust particles. However, it has also long been surmised that there must be some sort of turbulence at work in order to explain an apparent angular momentum anomaly. As gas/dust spiral inward to form a star, conservation of angular momentum, at least in the simpler models of this process, would seem to require that the star's rotation speed increase. However, in the case of our sun just the opposite has occurred; the sun spins slower than it "should" and so something must have carried off part of the angular momentum in the accretion disk. Some sort of turbulence would be able to fill this role, but the question is: what causes the turbulence? The discovery of magnetically-aligned gas jets during star formation provided an important observational clue, and the classical picture of charge-neutral accretion disks is giving way to the "magneto-hydrodynamic" picture.[10,11]

Galaxies, however, are not stars, and so one can ask, "How is any of this relevant to what we were talking about earlier?" As it happens, the accretion disk theory for star formation appears to look very promising for galaxy formation as well. Accretion disks seem to be ubiquitous in object structures spanning vast orders of scale, and accretion disk theorists seem to be making some headway applying this theory to vastly larger structures.[12] One very interesting recent theoretical result has been that the presence of gas in a galaxy leads to a complex wave effect capable of producing the spirals in spiral galaxies, the bar-and-spiral structures seen in, for example, our own Milky Way, and, most importantly, that spirals, etc. are not permanent features of a galaxy.

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[9] Evan Scannapieco, Patrick Petitjean, and Tom Broadhurst, "The emptiest places," *Scientific American*, vol. 287, no. 4, Oct. 2002, pp. 56-63.

[10] Lee Hartmann, *Accretion Processes in Star Formation*, Cambridge, UK: Cambridge University Press, 1998.

[11] James M. Stone, "Dynamics on the edge: Star-disk interaction," in *Accretion Processes in Astrophysical Systems: Some Like It Hot!*: Proc. Eighth Astrophysics Conference, College Park: MD, Oct., 1997, pp. 53-64

[12] Omer Blaes, "A universe of disks," *Scientific American*, vol. 291, no. 4, Oct., 2004, pp. 48-57.

Instead, it would seem that galaxy formations are themselves quite dynamical, evolving structures. Some very vivid illustrations obtained through simulation have recently been provided by Combes.[13]

Taking all this together, the new developments in accretion theory appear to render Dr. Alp's key supporting argument for his 'anomalous redshift' theory rather moot. Gravitational interaction between 'peculiar' galaxies is quite unnecessary if the observed 'peculiar' galaxy formations are merely circumstantial and if, as the new accretion disk work suggests, the coincidence of such peculiar pair structures as he noted should not be all that uncommon.

But Arp's (discredited) 'redshift anomalies' are not the only ones in the sky. And this brings us to what might seem an unexpected topic: dark matter.

Redshift Anomaly and Dark Matter

Accretion disk theorists are not the only people working on the question of galaxy formation. Cosmologists are also hard at work on galaxy theory. And the theories could hardly be more different. When one reads the accretion disk theory literature on galaxy formation, one sees a lot of discussion about plasmas and gravitational/magnetohydrodynamic interactions. When one reads the cosmological literature on galaxy formation theory, one generally sees very little discussion of plasmas or non-gravitational interactions (although there is unarguably more of it now than a decade ago). What one always does find, though, is a lot of talk about 'dark matter'.[14]

Dark matter has been the silver bullet of Big Bang cosmology since the early 1980s.[15] Yet no one has any direct evidence that dark matter 'exists'; there is not even any agreement on precisely what dark matter is supposed to be except: (1) it supposedly only interacts with other matter through gravity; and (2) it is not supposed to be ordinary matter (baryons). Why, then, is it a central part of Big Bang cosmology? Whenever I have read any article about the Big Bang and the role of dark matter, I am always left with the same impression: Dark matter 'must' be there because if it does not exist the idea of a Big Bang explosion at some $t = 0$ does not work.

Now, a secondary quantity of Facet B can never establish the *Dasein* of any thing with objective validity. This is because a secondary quantity is itself not an object of any possible empirical experience. Certainly a mathematical theory can *anticipate* the actuality of an object, but until and unless this object turns up as an object of experience, its *Dasein* is merely problematical. Physicists do (and should) search for problematical objects, but in every other

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[13] Francoise Combes, "Ripples in a galactic pond," *Scientific American*, vol. 293, no. 4, Oct. 2005, pp. 42-49.

[14] For example, see Malcolm S. Longair, *Galaxy Formation*, Berlin: Springer-Verlag, 1998.

[15] More recently, so-called 'dark energy' has also been added to the arsenal.

branch of science except Big Bang cosmology, GUT, and ‘string’ theory, the hypothesis is never taken to be established *until* something in experience provides an *actual* and *unequivocal* ground for inferring the *Dasein* of the problematical thing.

Sometimes, though, the thread is very slender and enthusiasm runs past what it *can* say with objective validity and jumps straight into transcendent speculation. There is a thread in experience that points to the *Dasein* of an unknown cause of an actual effect, and it is upon this thread that Big Bang cosmology hangs its speculation of dark matter. The thread is a different sort of redshift anomaly, namely differential redshift in determining the rotation rates of objects.

In 1937 an astronomer named Fritz Zwicky noticed something peculiar. Zwicky had used redshift measurements to determine the relative speeds of galaxies in the giant cluster Coma. He determined that these galaxies rotated about each other, somewhat like a solar system but with galaxies instead of planets. But what was disturbing was that according to his interpretation of the observations, the galaxies were orbiting so fast about a center of mass that the cluster should have long since flown apart given their apparent masses and the predictions of Newtonian gravitation. Zwicky concluded that there must be a tremendous amount of unseen matter (about 20 times the apparent mass) in the cluster holding it together, assuming there is no anomalous redshift.

Zwicky’s hypothesis did not produce much reaction in the astronomy community at that time. In the 1950s and early 1960s, however, another astronomer, Vera Rubin (in collaboration with Geoffrey and Margaret Burbidge) used redshift data to measure the rotation rate of M31 (the Andromeda galaxy). She found that the galaxy was rotating “too fast” for the amount of apparent matter it contained, and therefore under Newtonian gravitation Andromeda should be disintegrating. Because this is apparently not the case, she, too, concluded there must be an enormous amount of unseen matter (10 times the apparent mass of Andromeda) holding it together (again assuming there is no anomalous redshift).

One more factor in this developing story must be mentioned before we begin an examination. In the early 1970s James Peebles (who is one of the acknowledged top experts in cosmology) and Jeremiah Ostriker published a paper ‘demonstrating’ that there must be a ‘halo’ of dark matter in which the Milky Way must be embedded. If this were not so, said the theory, the Milky Way would not be able to maintain its spiral shape and should have long ago collapsed into a bar shape. I mention this somewhat unrelated item here because it is viewed as part of the ‘case’ for the *Dasein* of dark matter.

I think we need not spend much time on the Peebles-Ostriker finding. First, it has been learned since the early 1970s that the Milky Way *is* a spiral-and-bar galaxy. Second, stability calculations of this sort have long been known to present formidable problems; even present-day accretion disk/wave theory tells us without hesitation that the whole story is not yet known. Third, the most recent theories of galaxy formation from the accretion disk camp says galaxies are not fixed structures; they evolve in and out of spiral, bar-spiral, etc. morphologies all the time. It seems to me that galaxy formation-and-structure theories must incorporate these findings.

Now let us get back to the Zwicky finding. It would seem that we face a hard choice. Either we must assume there is an anomaly in determining the mass of the Core giant cluster, or we must assume there is an anomaly in the redshift data, or we must assume something else is wrong. The second alternative would be an "anomalous redshift" of a wholly different kind than the one argued for by Arp. Big Bang theorists have chosen the mass anomaly. Because this choice is not capricious, we must understand why they make this choice.

First, for the benefit of the non-physicist readers, I think it is appropriate to provide a (very oversimplified) picture of how galactic velocity is related to the mass of a cluster. I'll present the cartoon version and then we'll let the experts give us the accepted version.

In the simplest possible version (and actually this version is far too simple; I should call it the 'simplest impossible version'), picture a cluster of galaxies having a center of mass with total mass M. Pretend there is a galaxy of mass $m << M$ making a circular orbit of radius r around the center of mass of the cluster with tangential velocity v. Let us assume we can ignore galaxy-pair interactions and need only look at the 'interaction' between one galaxy and the center of mass (a "solar system" approximation). Finally, let us assume that Newton's classical law of gravitation applies. The Newtonian force of gravity must then equal the centripetal acceleration of the galaxy times its mass, i.e.,

$$\frac{GmM}{r^2} = \frac{mv^2}{r} \Rightarrow M = \frac{v^2 r}{G} .$$

We see that the mass m of the galaxy cancels out nicely and the mass of the cluster M is directly proportional to the orbital radius and directly proportional to the square of the velocity. (This is a standard calculation required of students taking freshman physics). If the velocity doubles, the required mass increases four-fold for a fixed value of r.

Now, clusters are not solar systems, galaxies in a cluster do significantly interact with one another, 'orbits' in a cluster are not circular, and so on. Nonetheless, when the problem is worked out more accurately the end result is more or less the same as the idea conveyed by the simple model above. Even the formula looks the same except for a constant factor multiplying it and a slight re-interpretation of the variables. This standard solution makes use of what is known as the "virial" theorem. Let us see what one of the experts (specifically, Malcolm Longair) has to say:

> It might seem that the problem of measuring the masses of clusters of galaxies is relatively straight-forward. The virial theorem . . . provides a simple relation between the velocity dispersion of the galaxies . . . in the bound cluster and the characteristic radius . . . of the galaxy distribution. It turns out that there are remarkably few clusters for which a detailed analysis can be made. The reason is that it is essential to make a careful assessment of the galaxies which are truly cluster members and to measure accurate radial velocities for large enough samples of galaxies.
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> The Coma cluster is a good example of a regular rich cluster of galaxies which has been the subject of considerable study. The space density of galaxies increases smoothly towards the center, resembling to a good approximation the spatial distribution expected of an isothermal gas sphere. The inference is that the cluster has relaxed to a bound equilibrium configuration, and this is confirmed by comparing the *crossing time* of a typical galaxy in the cluster with the age of the Universe. . . For the Coma cluster . . . the crossing time is about 2×10^9 years, about a fifth to one tenth the age of the Universe. This is clear evidence that the cluster must be a bound system or else the galaxies would have dispersed long ago.
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> > [Longair next summarizes some key reports determining the calculated *M* of the Coma cluster, how they were made, and how the figures relate to mass-to-luminosity ratio. We can do without these details here and merely allow that the calculations were done correctly and the calculation method followed long-standing accepted practice in astronomy.]
>
> This is the key result. The Coma cluster is a classic example of a rich regular cluster and the population is dominated in the central regions by elliptical and S0 galaxies for which [he gives us the number for the typical mass-to-luminosity ratio]. There is therefore a discrepancy of about a factor of 20 between the mass which can be attributed to galaxies and the total mass which must be present. This is perhaps the best defined case for the presence of dark matter in any system and was first noted by Zwicky in 1937. It is also where the trouble begins. The dark matter dominates the mass of the cluster and there is no reason why it should have the same distribution as the visible matter. Likewise, there is no reason *a priori* why the velocity distribution of the galaxies should be isotropic. . . Merritt (1987) has provided a careful study of how the inferred mass-to-luminosity ratio would change for a wide range of different assumptions about the relative distributions of the visible and dark matter and the anisotropy of the velocity distribution. For the cluster as a whole, the mass-to-luminosity ratio varies from about 0.4 to at least three times the reference value [the reference value is 20; this means the conclusion is there is 8 to 60 times more dark matter than visible matter].
>
> We have considered the case of the Coma cluster in some detail because for few clusters is such a detailed analysis feasible. The velocity dispersions of rich clusters are all about 10^3 km s^{-1} and they have similar length-scales. Therefore, dark matter must be present in all of them.[16]

The Merritt study cited by Longair is known elsewhere (in system theory) as "testing the robustness of the model results." It is standard good practice in any situation where important inferences are to be drawn from uncertain data. Given the model which is the starting point for the analysis, the description we have seen reflects thorough, patient, and utterly professional work of high caliber. If we accept the model and its assumptions, we must accept the conclusions.

But a calm and disinterested spectator up in the peanut gallery is entitled to ask at this point: How do we know the Coma cluster formed long enough ago that your inference on its stability is correct? After all, on cosmological timescales the first time anyone ever saw the Coma cluster was a blink of an eye ago. If the cluster did form much more recently than is assumed, the inference that it is 'stable' and, therefore, its galaxies really are 'bound' cannot be justified. If that

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[16] Longair, *op. cit.*, pp. 85-87.

cannot be justified, then the entire mathematical exercise is problematical.

The answer we will get, naturally, is that the age of the Coma cluster is a direct consequence of the Big Bang. We assume the premise, calculate the consequences of the premise, and then – lo and behold – we find an inconsistency; therefore we must introduce dark matter. Elsewhere in science this is called "circular reasoning by adding a new hypothesis." If one assumed the Big Bang *without* dark matter, calculated the consequences, and found that the consequences matched the data with no additional hypothesis, this would constitute a major bit of documented evidence pointing to the *Dasein* of a Big Bang. But that is not what we have here.

Longair goes on to discuss some interesting X-ray emission discoveries that implicate the presence of hot gases in and around galactic clusters. (This is not the mysterious non-baryonic dark matter; it is ordinary matter). Using models and assumptions I regard as reasonable, experts estimate that the mass of the intracluster gas is on the order of about 5 times the visible mass of the galaxies. But this is still not enough according to the Big Bang premise. Dark matter is still 'required' to exist. But why 'required'?

The peanut gallery asks: if there is so much hot gas, why should we not think there might be even more cold gas? In part, of course, the answer is none has been observed. But the deeper part again has to do with the Big Bang premise. Here the 'theory of everything' (GUT, or Grand Unified Theory) people get involved. Assuming there was a Big Bang and assuming a number of other things (which we do not actually know to be true), the theorists have calculated a maximum amount of baryonic matter that could have been produced during the Big Bang 'explosion.' The conclusion is that not enough baryonic matter could have been produced, i.e. that even if there actually is more 'cold' and baryonic gas presently undetected, there still could not possibly be enough to make up for the mass discrepancy. Therefore, goes the theory, there must be non-baryonic dark matter. Again the reasoning is circular, but what we have now is *utterly new and speculative physics* introduced *ad hoc* to support the *premise*. This is Platonism.

As it happens, there are heretics in the village. In 1986 two theorists, Valtonen and Byrd, published a study in which they relaxed the assumption that all the galaxies thought to be part of a cluster really were bound to the cluster.[17] The justification for this bit of heresy is found in certain discrepancies that had already been noted. In most galactic clusters the brightest galaxy seems to be moving away from us more slowly than the cluster it 'belongs' to. This inference comes from the observation that the redshift of the brightest galaxy is usually less than the average redshift of the cluster as a whole. Using *their* model they were able to 'demonstrate' there is *no* "missing

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[17] Mauri Valtonen and Gene Byrd, "Redshift asymmetries in system of galaxies and the missing mass," *The Astrophysical Journal*, vol. 303 (1986), pp. 523-534.

mass."

I do not propose to take sides here, but unless Valtonen's and Byrd's model can be refuted *without* invoking a Big Bang and its structures, any disinterested person can reasonably conclude that we have two competing hypotheses in play. So far as I know, the Valtonen-Byrd report has not been refuted by anyone. I do know that two competing unrefuted hypotheses adds up to zero *established fact*. I also know that given a choice between an hypothesis that requires *ad hoc* physics and one that does not, I tend to favor the latter.

Now what about Rubin's observations? In the case of a nearby galaxy (and none are more nearby than Andromeda) it is certainly a stretch to argue that perhaps some of its stars are not 'bound' to it. Nor can refuge be taken in the uncertainty of its distance. Andromeda's distance can be gauged by Cepheid variable stars within it. If ever there were an observational smoking gun for inferring the *Dasein* of dark matter, wouldn't Rubin's findings be it?

The phenomenon involved here is called the "flattening of the rotation curve" of galaxies. Redshift can be used to estimate the rotational velocity of stars as a function of their distance from the center of the galaxy. These observations show an anomalous behavior, namely that stars in the periphery tend to have velocities that remain more or less constant as a function of distance. If gravity is the only force at work in these dynamics, then there is not enough visible matter in the galaxy to account for the observations (Longair, *op. cit.*, pp. 57-58). But what if gravity is not the only force at work here? Beginning in 1983 plasma physicist A.L. Peratt et al. began publishing results of simulations taking into account the effect of magnetic forces in galactic plasmas[18]. The theoretical results of this model reproduces the shape of the "flat" rotation curves observed for spiral galaxies with impressive accuracy and without having to introduce any new physics. It again seems dark matter is not necessary in order to explain the effect.

Another contender – in the sense of opposing the dark matter hypothesis – appears to be a phenomenological model put together by Mordehai Milgrom which goes by the name MOND.[19] At its root, MOND is a phenomenological modification to Newton's law of gravity that is unobservable over solar distance ranges but which is (made to be) significant at the galactic scale. There is a very serious objection to MOND, namely that it is a curve fit model and has not been deduced from more fundamental principles. On the other hand, no one has rigorously worked out what the Einstein equations would say about the local metric of a galaxy. Perhaps MOND would be a natural consequence of general relativity; perhaps it would not. It is clear that MOND has

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[18] Peratt, Anthony L., *Physics of the Plasma Universe*, NY: Springer-Verlag, 1992, pp. 128-131.

[19] Mordehai Milgrom, "Does dark matter really exist?" *Scientific American*, vol. 287, no. 2, Aug. 2002, pp. 42-52.

shortcomings as well as virtues[20], and so it is hardly in a position to claim more than problematical standing – and Dr. Milgrom claims nothing more than this for it.

§ 6.5 Control of Enthusiasm

There is much more that we could discuss – and I am tempted to discuss it. Were this critique to continue we would talk about inflation theory (or theories)[21], the cosmic microwave background radiation, the Big Bang's 'timeline' for the evolution of the universe, and other items. But it is no more my purpose here to try to 'disprove' the Big Bang than it is to 'prove' it. As I said earlier, I do not think the Big Bang ever happened; I do not think the Big Bang did not happen.

My point is merely this: The Big Bang is not a fact. It is an hypothesis for which there happens to be a great deal of developed speculation. All I actually want is a cessation of claims that it *is* a fact, an open forum for the publication of competent competing theories and contrary findings, and a general recognition that the issue is far from settled. I may be mistaken, but I think the discussion just concluded has accomplished its task, which was and is to take a stand against the surge of new Platonism that is today working its way into mainstream science.

I have beside me a clipping taken from our local newspaper dated May 1, 2001. In large, bold type the headline shouts: **Data confirm Big Bang theory**. The story goes on to read,

> WASHINGTON – Key elements of theories about how the universe expanded and developed after the Big Bang have been confirmed by data from high-flying balloons and from instruments operating in Antarctica, scientists say.

Naturally, the story has to do with probing the cosmic microwave background. This is either dishonest science reporting or else just plain dishonest science. If the headline and story had read, "Big Bang theory passes key test" I would have zero objection to it. That is what had actually happened, and that is what would have been intellectually honest to say.

Not every scientist involved with cosmology research is a purveyor of misleading information. Dr. Peebles, who is a leading expert in cosmology theory, writes

> This is an exciting time for cosmologists: findings are pouring in, ideas are bubbling up, and research to test those ideas is simmering away. But it is also a confusing time. All the ideas under discussion cannot possibly be right; they are not even consistent with one another. How is one to judge the progress? . . .

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[20] Anthony Aguirre, "Not a bad idea," *Scientific American*, vol. 287, no. 2, Aug. 2002, pg. 51.

[21] Data from the WMAP (Wilkinson Microwave Anisotropy Probe) is being hailed as providing overwhelming observational support for Big Bang predictions, but within its alleged successes (which are based on mere curve fitting to select a 'best' standard model) there are a couple disquieting data points as well. These potentially could knock over inflation theory (which itself is based on the assumption of the Higgs field). See Glenn D. Starkman and Dominik J. Schwartz, "Is the universe out of tune?", *Scientific American*, vol. 293, no. 2, Aug., 2005, pp. 48-55.

> How might one judge reports in the media on the progress of cosmology? I feel uneasy about articles based on an interview with just one person. Research is a complex and messy business. Even the most experienced scientist finds it hard to keep everything in perspective. How do I know that this individual has managed it well? An entire community of scientists can head off in the wrong direction, too, but it happens less often. That is why I feel better when I can see that the journalist has consulted a cross section of the community and has found agreement that a certain result is worth considering. The result becomes more interesting when others reproduce it. It starts to become convincing when independent lines of evidence point to the same conclusion. To my mind, the best media reports on science describe not only the latest discoveries and ideas but also the essential, if sometimes tedious, process of testing and installing the cross braces.
>
> Over time, inflation, quintessence and other concepts now under debate will be solidly integrated into the central framework or will be abandoned and replaced by something better. In a sense, we are working ourselves out of a job. But the universe is a complicated place, to put it mildly, and it is silly to think we will run out of productive lines of research anytime soon. Confusion is a sign that we are doing something right; it is the fertile commotion of a construction site.[22]

It seems to me Dr. Peebles has his enthusiasm under very good control. In this article he gave the major theories a 'report card'; it read as follows.

A+	The universe evolved from a hotter, denser state
A –	The universe expands as the general theory of relativity predicts
B+	Dark matter made of exotic particles dominates galaxies
B –	Most of the mass of the universe is smoothly distributed; it acts like Einstein's cosmological constant, causing the expansion to accelerate
Inc.+	The universe grew out of inflation

+ inconclusive

I would assign lower grades – some considerably lower – to all these items except the second and the last, but I would assign "F" to none of them, and I heartily approve of and endorse Dr. Peebles' giving them a grade. I cheer and applaud something else he says in this same article:

> That the universe is expanding and cooling is the essence of the big bang theory. You will notice I have said nothing about an "explosion" – the big bang theory describes how our universe is evolving, not how it began.[23]

If every theorist involved in cosmology theory would talk and write like this, this treatise would not have contained one single word about the Big Bang. Dr. Peebles is a *good scientist* and, more importantly, an *honest* scientist. He says what he *thinks*, not what he "knows."

Unfortunately, for every Dr. Peebles there are many New Platonists and they are not limited to the ranks of researchers in astronomical cosmology. I mentioned earlier the very interesting findings of the Brookhaven collider experiment where physicists think they succeeded in briefly liberating quarks and gluons. Their article in *Scientific American* was entitled "The first few

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[22] P. James E. Peebles, "Making sense of modern cosmology," *Scientific American*, vol. 284, no. 1, Jan., 2001, pp. 54-55.

[23] *ibid.*

microseconds" and a recurring theme interwoven with their findings of fact was that their experiment had "replicated conditions of the infant universe."[24] Well, no. They created conditions by which the transformation of energy from kinetic to thermal form meets the theoretical criteria required to liberate quarks and gluons. Their work is an important milestone in high energy physics, but it is mere romance to link this achievement to a hypothetical creation of the universe. If the Big Bang ever happened then *perhaps* conditions were similar; if it did not then clearly they did not re-create its conditions. Whether a Big Bang did or did not happen neither adds to nor takes away from the empirical knowledge their experiment has bought for us. Let us remember what Newton said:

> 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. This is certain, that it must proceed from a cause that penetrates to the very centers of the sun and planets . . . and propagates its virtue on all sides to immense distances . . . But hitherto I have not been able to discover the cause of those properties of gravitation 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 [NEWT1: 371].

It is the unbending nature of human Reason to seek for the unconditioned condition, to search for an understanding of ultimate cause, and to comprehend in one system the community of Nature. But in every determinant judgment of experience there is both truth and error. The latter is the consequence arising from the merely subjective principle of reflective judgment, from the operation of which we obtain all our general ideas. And only experience brings error into the light where we can see it and by which we better understand the contexts in which our judgments of experience hold true.

Our dearest dialectical fancies can set, in our habits of thinking, ontological presuppositions that calcify even our best and most fruitful ideas into a dogma, and when even the best of ideas reaches its old age in dogmatism it is fruitful no more. This is why it is important we understand the limitations of our ideas, that one may keep one's mind open and receptive to fresh experience. Faraday can serve as an example to us all:

> The attention of two very able men and eminent mathematicians has fallen upon my proposition to represent the magnetic power by lines of magnetic force; and it is to me a source of great gratification and much encouragement to find that they affirm the truthfulness and generality of the method of representation. . . Van Rees has published a mathematical paper on my lines of force in Dutch . . . He objects, as I understand, to what I may call the physical part of my view as assigning no origin for the lines, and as not presenting the higher principle conveyed by the idea of magnetic fluids or of electric currents: he says it does not displace the old theories, or render them superfluous . . . It was always my intention to *avoid* substituting anything in place of these fluids or currents, that

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[24] Michael Riordan and William A. Zajc, "The first few microseconds," *Scientific American*, vol. 294, no. 5, May, 2006, pp. 34-41.

> the mind might be delivered from the bondage of preconceived notions; but for those who desire an idea to rest upon, there is the old principle of the ethers.
>
> The encouragement I derive from this appreciation by mathematicians of the mode of figuring to one's self the magnetic forces by lines, emboldens me to dwell a little more upon the further point of the true but unknown natural magnetic action. Indeed, what we really want is not a variety of different methods of representing the forces, but the one true physical signification of that which is rendered apparent to us by the phenomena, and the laws governing them. Of the two assumptions most usually entertained at present, magnetic fluids and electric currents, *one* must be wrong, perhaps *both* are; and I do not perceive that the mathematician, even though he may think that each contains a higher principle than any I have advanced, can tell the true from the false, or can say that either is true. . . The notion that I have introduced complicates the matter still further, for it is inconsistent with either of the former views, so long as they depend exclusively upon action at a distance without intermediation; and yet in the form of lines of force it represents the magnetic actions truly in all that is not hypothetical. So that there are now three fundamental notions, and *two* of them at least must be impossible, i.e., untrue.
>
> It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavor to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity.[25]

§ 7. Toward a Science of Mental Physics

I intend for the just-concluded critique of some of the constituents of Big Bang theory to usefully serve as an example *in concreto* of some of the divers ways by which scientific reasoning slips unnoticed past the horizon of possible experience. For developing a science of mental physics I hold it to be true that this science must be mathematical. This is because the Objects by which we understand the phenomenon of mind are intelligible and their claim to objective validity can only therefore be settled in *practical* objective validity.

Now, our scientific understanding of Nature is mathematical, but this is not the same as saying Nature is mathematical. Our understanding of things-in-Nature is an understanding of Objects, and, because every Object refers to an object, a science of mental physics requires a careful examination of a Critical ontology for the objects of its topic. In this final section of the treatise we will look at what this implies.

What is mind? We are at the point where we can give a Critical *Realerklärung* of the answer to this question. Regarded as an object, **mind is the *noumenon* by the idea of which we understand the unity of all the capacities, powers, representations, processes, and acts that describe and explain the Nature of human experience as each of us comes to know it**. It is the intelligible aspect of the phenomenon of being human as Organized Being, just as body is the sensible aspect of the appearance of this same Organized Being. Mental physics is to be the doctrine of a science taking mind as its topic. We close with only the outline for this doctrine.

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[25] Michael Faraday, "On some points of magnetic philosophy," *Philosophical Magazine*, Feb., 1855.

§ 7.1 Slepian Dimensioning

Newton thought that through his absolute quantities he had firmly established a real connection between mathematics and physics. For the next 300 years science felt secure that its mathematics was thus safely anchored in nature. It was rare when a major figure in the history of science, such as Faraday, keenly understood and appreciated that mathematical understanding, while necessary for the growth of science, is nonetheless always a speculative understanding. The rest of science, particularly under the hypnotism of positivism, let go unnoticed the fine distinction between mathematical knowledge and knowledge of experience. This lack of distinction is revealed by reification. One example of this is provided by the story of the luminiferous æther. Another is provided by the present-day habit of reifying the geometrical space-time of relativity. I think it not unlikely we may someday find another example in the making of a real, rather than logical, division between the fermions and bosons of modern physics inasmuch as the former stands, pragmatically speaking, as that-which-acts-or-reacts (ontological seat of causes and effects) and the latter stands as the descriptive vehicle for explaining interactions (causality 'mechanism'). In psychology an example is provided by the reification of 'emotions', e.g. the postulate of some discrete set of 'primary emotions'.

When Einstein's relativity theory cut the Newtonian anchor rope of absolute quantities this break went unnoticed. Even Einstein seems not to have appreciated the full ontological scope of relativity's implications. But to abandon mathematics in science, merely because its objects are intelligible and belong to Facet B of our understanding of Nature, would be an act of supreme folly. To abandon large parts of mathematics merely because many of mathematics' axioms lack objective validity would be an act of colossal pragmatical folly. Armed with the doctrine of Critical epistemology presented here, it is wholly within the power of human Reason to forewarn us of the need to understand the objective limitations of our ideas and to likewise comprehend the relationship between our ideas of the merely intelligible objects of pure mathematics and the objective validity of our ideas of *noumena* by which science draws its power for the unification in Nature of experience.

Have we any right to pursue, have we any objectively valid justification for the practice of, using mathematics to understand Nature? Or must we see Einstein's accomplishment as both a monumental triumph and the greatest disaster ever to befall mankind's hope of understanding Nature? The answer is: There is a proper place in science for the employment of mathematics and this place has a solid anchor in transcendental principles. There is an objectively valid use for mathematical theory, but we must comprehend this use and subject it to a discipline of Reason.

We must begin this final section by showing how mathematics and experience are joined to one another, and this with *practical* objective validity, by a transcendental *necessity*. In honor of Dr. Slepian, I call this Critical doctrine of method by the name **Slepian dimensioning**.

The Critical *Realerklärung* of what it means for an Object "to be real" has both a material condition (non-problematical combination with the real in sensation) *and* a formal condition. The formal condition is: **the concept of the object must be combined with other concepts that give it a *context* (*Zusammenhang*)**. Context is necessary for the possibility of thinking the meaning or meanings of the object and for delimiting – through transcendental affirmations and transcendental negations – the *Existenz* of the object in Reality. Now, the idea of 'context' does not make reference to any object of sensuous experience. A context is a sphere of concepts, combined by judgment with the concept said to have the context, which delimits the applicable scope involving that concept in Reality. On the one hand, context goes to the determination of the *Existenz* of the object, and in this role it is objective and a determination of physical *nexus*. But, on the other hand, context also goes to the determination of the relationship between the object and the thinking Subject – i.e. in which contexts is the object held-to-be-real and in which is it held-to-be-unreal. This aspect of context is a determination of metaphysical *nexus* (Modality). **No representation can be of a real Object for us unless it has context in both kinds of *nexus***.

Now, Slepian's reference to Facet A is *a determination of transcendental place* as the former type (physical *nexus*). Physical *nexus* is the *nexus* of the empirical in human knowledge but the *determination* is one of the relationship of Subject and Object and, therefore, is a determination of metaphysical *nexus*. Reference to Facet B, in contrast, *is a determination of transcendental place* as the latter type (metaphysical *nexus*). Metaphysical *nexus* is the *nexus* of rational understanding in human knowledge but the *determination* is again a determination of metaphysical *nexus*. All objects of mathematics – all of which are represented with *made* (defined) concepts – belong to Facet B. However, **both facets are determinations of contexts of understanding in metaphysical *nexus***. Facet A and Facet B stand as divided members in the disjunction of the **practical notion of context**. The notion of context is necessary for the possibility of experience – thus is both transcendental and *objectively valid* in the practical Standpoint – and the connection of every real Object to context regarded as *a function of understanding* is a necessary connection. But context-regarded-as-function-in-general is an Object.

A real Object has its transcendental place in sensible Nature, and this is what figure 24.4.1 is depicting. We will call this **the physical dimension in understanding**. But context-as-Object is a purely intelligible Object and has its transcendental place in intelligible Nature. We will call this **the intelligible dimension in understanding**. These two dimensions are "orthogonal" insofar as

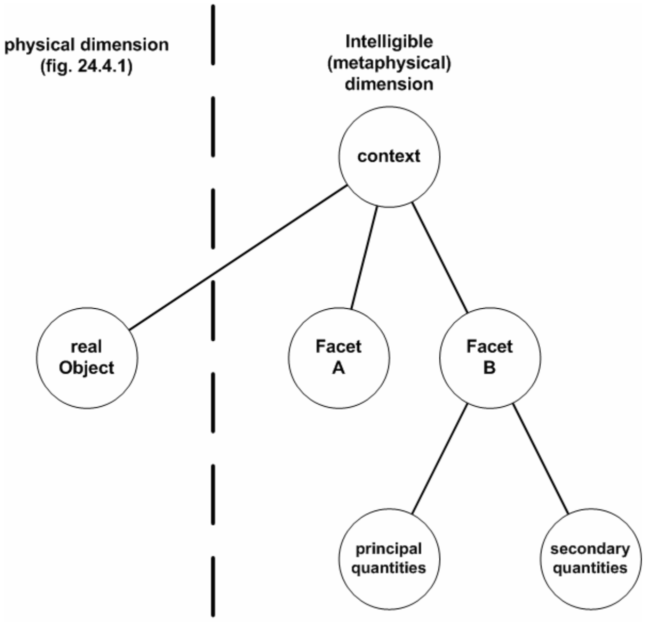

Figure 24.7.1: Slepian Dimensioning in Understanding. The physical and the intelligible (metaphysical) dimensions pertain to the two different poles of *nexus* in understanding. In this sense the two dimensions are 'orthogonal' to each other, much like for a complex number the real part and imaginary parts of the number are regarded as orthogonal. The physical dimension is what is represented in figure 24.4.1. The combination in understanding of the concept of a real Object with the transcendental context is a combination made necessary in judgment and is called a purposive combination (*Zweckverbindung*). Facet A and Facet B as concepts are divided members of a disjunction under the idea of context, and, likewise, principal and secondary quantities are divided members of a disjunction under Facet B.

how they contribute to the possibility of understanding is concerned. The physical dimension is the dimension of *the understood phenomenon*. The intelligible dimension is the dimension of *the comprehended phenomenon*. These two **Slepian dimensions** are depicted in Figure 24.7.1.

Now, unity in Slepian dimensioning is a necessary condition under the Critical acroam of the metaphysic proper of Rational Theology: Absolute unity of the condition of *all* objects of thinking in general. Consequently the rule of combination for real-Object-and-context is a practical rule of pure speculative Reason, and we call this the rule of *Vernunftmäßigkeit* (moderation of Reason). The determinant judgment of combination of the concept of the real Object with context we call a *Zweckverbindung* (purposive combination). *Vernunftmäßigkeit* is a rule under the principle of formal expedience of Nature, but *Zweckverbindung* as a combination in determining judgment is necessitated by pure practical Reason and expressed through ratio-expression by pure speculative Reason under the principle of Conformity to Law.

The objects of mathematical concepts are intelligible objects. Comprehension in knowledge always calls upon concepts of intelligible objects for the logical formal expedience in judgments of *a system of Nature* (a task laid to the process of teleological reflective judgment). When once humankind could avail itself of mathematics for comprehension, its objects are made necessary in the understanding of context, and they augment and perfect comprehension by means of ideas of practical objects. The Critical Methodology of the discipline of pure Reason has only to ensure our *use* of these ideas remains practical and not dialectical. We next proceed to examine the methodology for this accomplishment.

§ 7.2 Precepts of Ontology

Plato bequeathed to humankind the notion of *orthodóxa* (“right opinion”). In the context of his metaphysics this notion lacked any objective validity, but we can apply Kant’s Copernican turn to this notion and make it an idea with meaning for science. Let us call the ideas of Critical *orthodóxa* by the name **precepts of ontology**, and let us examine what these precepts are to be at the foundation of speculation in science.

A logical place to begin is with Slepian’s division of human knowledge into a Facet A of actual experience and a Facet B of mathematical comprehension. The Objects of the highest objectively valid sphere of unity in this two-fold view are the *noumena* at the horizon of possible experience because these Objects are the junction points where the last combinations with concepts are made that still contain under them a non-problematical real in sensation. The unity of objects of mathematics and objects of experience in the ideas of these *noumena* is the highest construct, from the theoretical Standpoint, where empirical realism and mathematical rationalism come together. However, as any scientist working in one of the mathematical sciences can appreciate, mathematical constructs are not found exclusively in these highest objectively valid ideas. Such a combination occurs, for example, whenever we write an equation of motion for a physical object (since these equations involve the mathematical concepts of time derivatives).

Thus, combinations of Facet A observables and Facet B constructs occur in scientific theories throughout the structure of the manifold of concepts in the context of scientific theories. Such conjunctions involve combinations of concepts in Facet A with Slepian’s principal quantities of Facet B. But these principal quantities have their own connections to secondary quantities of Facet B (mathematical unity), and in no case do secondary quantities combine immediately with concepts of objects of real sensuous experience. One way to describe this is to say that our scientific comprehension of Nature contains two complementary “conceptual universes” (Facet A structures and Facet B structures) with many bridges of determinant judgments where principal quantities of Facet B are combined with concepts of appearances in Facet A. Where we make combinations in determinant judgments of this type, objects of Facet B are understood as intelligible aspects of objects of experience, whereas the sensible concepts for the object of Facet A are understood as empirical objects (empirical aspects).

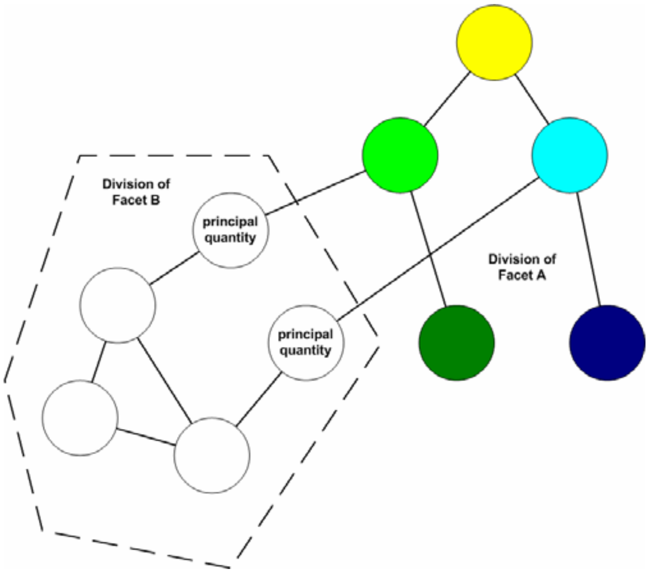

Figure 24.7.2: Illustration of facet cross-combination in the manifold of concepts.

An illustration of this division of concept structure is given by Figure 24.7.2. In interpreting this figure, the explanation of this construct does not imply mathematical concepts spring up independently of experience. That proposition is provably false, as Piaget's studies have shown.

> To know is to assimilate reality into a system of transformations. To know is to transform reality in order to understand how a certain state is brought about. . . To my way of thinking, knowing an object does not mean copying it – it means acting upon it. It means constructing systems of transformations that can be carried out on or with this object. Knowing reality means constructing systems of transformations that correspond, more or less adequately, to reality. . . The transformational structures of which knowledge consists are not copies of the transformations in reality; they are simply possible isomorphic models among which experience can enable us to choose. Knowledge, then, is a system of transformations that become progressively adequate.
>
> It is agreed that logical and mathematical structures are abstract, whereas physical knowledge – the knowledge based on experience in general – is concrete. But let us ask what logical and mathematical knowledge is abstracted from. There are two possibilities. The first is that, when we act upon an object, our knowledge is derived from the object itself. This is the point of view of empiricism in general, and it is valid in the case of experimental or empirical knowledge for the most part. But there is a second possibility: when we are acting upon an object, we can also take into account the action itself, or operation if you will, since the transformation can be carried out mentally. In this hypothesis the abstraction is drawn not from the object that is acted upon, but from the action itself. . . This knowledge is what I call logical mathematical knowledge and not physical knowledge [PIAG17: 15-17].

Insofar then as objects of scientific theory are concerned, their understanding concepts fall into one or the other of our two divisions (Facet A and Facet B), and objects of concepts of Facet B, being purely intelligible objects, must always seek the grounds for their objective validity from the practical Standpoint. We will say they are *practical objects*. Consequently, they are not and cannot be regarded as *physical* things. Reification is the act of holding an object to be a physical thing, and therefore our first precept of ontology is: **intelligible objects may not be reified**.

Now, in the logical division of mind-body the division of mind contains the intelligible aspect of an Organized Being. It follows from the first precept that all objects in the divisions of *nous* and *psyche* are intelligible objects grounded in an objective validity that is merely practical. The immediate consequence of this is: these intelligible objects belong to Facet B and thus will have only a mathematical description in a theory of mental physics. Objects of *nous* are those objects that stand with no immediate connection to objects of *soma*, and, consequently, the mathematical objects that describe them can only be Slepian secondary quantities. The ideas of *nous-soma* reciprocity belong to the division of *psyche*, and therefore in regard to the divisions of *nous* and *psyche* only *psyche* will contain Slepian principal quantities of Facet B.[1]

The connections between the facets occur only between concepts of objects of appearances on the one side and principal quantities on the other. It is, therefore, mandatory that concepts of principal quantities be objectively valid. But as these objects are mathematical, this requirement carries with it a rule, namely that the mathematical definitions and descriptions of principal quantities belong to that division of mathematics I have previously named Critical mathematics. This is our second precept of ontology: **principal quantities can only be based upon axioms of mathematics deduced from Critical acroams with demonstration of objective validity**.

The present day doctrines of mathematics and mathematical logic are not organized around the division between Critical and hypothetical mathematics. Insofar as Critical mathematics is regarded as a system of logic, it must be developed as a **Logic of meanings** for it is only thus that we can forge the conjunction of principal quantities with objectively valid concepts of appearances. Meanings are fundamentally grounded in action schemes (as we have previously seen) and action schemes connect practically to the observable actions of an Organized Being, and thereby acquire a practically necessary relationship to actual experience. What Piaget called the logic of actions is, consequently, the starting point for deductions of the rules of correspondence that must be set down as the objective grounds for judgments where objects of appearances and objects of principal quantities are combined. This is our third precept of ontology: **Critical mathematics is a Logic of meanings**.

Slepian secondary quantities belong to hypothetical mathematics. Here we must concern ourselves with the possible relationships between secondary and principal quantities. We earlier saw Slepian illustrate this point with his principle of indistinguishability. However, his

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[1] This does not mean that objects of *soma* do not also have representations employing secondary quantities. As neuroanatomy, neurophysiology, neuropharmacology, etc. move down the road to becoming ‘exact’ subdisciplines within mental physics, we may expect and require them to become more mathematical in their explanations. Wherever and whenever this takes place, their mathematical entities belong to Facet B.

presentation of this principle is very much a special case aimed at the science and art of signal processing theory. It requires more generalization to make it applicable to a broader scope of science. Here what we may note is that his principle explicitly makes use of the idea of multiple possible representations indistinguishable in Facet A at a given level of perceptibility. We must further note that this level does not refer merely to the immediate capacity for sensible perception of the Organized Being but, rather, allows for and presumes the employment of measuring instruments to 'extend' our senses. The consequence of this is: the objectively valid determination of principal quantities in relationships to secondary quantities must be expected to be dynamical (in the sense that advances in measurement capabilities implicate changing capacities for making distinctions among the quantities). This view of the principle of indistinguishability lets us understand Slepian's principle more generally as a principle of Modality in objective judgments. For this principle the precept of ontology is: **all secondary quantities representing a principal quantity are possible if they are sensibly indistinguishable in the Slepian sense**.

We can understand these four precepts of ontology respective of Critical metaphysics proper as precepts of: (1) Quality in reification (Rational Psychology); (2) Quantity in Critical axioms of mathematics (Rational Physics); (3) Relation in meanings (Rational Cosmology); and (4) Modality in Reality (Rational Theology). The application of these precepts in the development of scientific theory are acts of general synthesis and, therefore, the general synthesis in application must account for all three Critical Standpoints (the 'poles' of synthesis illustrated in figure 24.4.3). Now, this accounting goes to the understanding of the epistemological underpinnings in the development of scientific theory, and so it is here that we find a new role for Critical philosophers in the community of science. The role is that of *critique* in theory development. More specifically, science development always presumes some metaphysic or pseudo-metaphysic. A Critical science – i.e. an epistemology-centered science – requires a Critical applied metaphysic. But such a metaphysic must be grounded in Critical metaphysics proper and the method for its development is critique:

> In order that [metaphysics] henceforth, as science, be able to lay claim, not merely to deceptive persuasion, but to insight and conviction, a critique of reason itself must set forth the entire stock of concepts *a priori*, their division according to the different sources (sensibility, understanding, and reason), further, a complete table of these and the analysis of these concepts with all that can be deduced from them, and then, especially, set forth the possibility of synthetic knowledge *a priori* by means of a deduction of this knowledge, the first principles of its use, and finally also the boundaries of that use, and all this in a complete system. Therefore critique, and that alone, contains within itself the whole well-tested and verified plan, indeed all the means of accomplishment of metaphysics as science; it is impossible by any other ways and means [KANT2a: 154 (4: 365)].

Kant is not speaking here of Critical metaphysics proper. The topic of the *Prolegomena* is not

Critique of Pure Reason but, rather, ***applied metaphysics in application to science***. The concepts *a priori*, knowledge *a priori*, etc. of which he speaks in this passage are those pertaining to the topic of the science in question and the epistemologically-grounded proper methods available to that science. This is the significance of the word "future" in the full title of that work: *Prolegomena to any* **Future** *Metaphysic that can Stand as Science*. I wish for mental physics to begin along this pathway, but there are many special sciences and they, too, will benefit from the development of their respective applied metaphysics. To my colleagues in philosophy I say: There is much work to be done.

§ 7.3 The Principle of Formal Undecidability in Science

Mark Twain wrote,

> In the space of one hundred and seventy-six years the Lower Mississippi has shortened itself by two hundred and forty-two miles. That is an average of a trifle over one mile and a third per year. Therefore, any calm person, who is not blind or idiotic, can see that in the Old Silurian Period, just a million years ago next November, the Lower Mississippi was upward of one million three hundred thousand miles long, and stuck out over the Gulf of Mexico like a fishing rod. And by the same token any person can see that seven hundred and forty-two years from now the Lower Mississippi will be only a mile and three-quarters long, and Cairo and New Orleans will have joined their streets together, and be plodding along under a single mayor and mutual board of aldermen. There is something fascinating about science. One gets such wholesome returns of conjecture out of such a trifling investment of fact.[2]

Speculative conjecture is science's equivalent of quoting someone out of context. It takes a relationship established empirically under a set of conditions and speculates on what this relationship implies if projected out to extreme conditions. Oftentimes this projection involves a regress to infinity or a regress to the infinitesimal. Prior to the success of quantum mechanics, if conjecture led to what seemed to be an absurd conclusion, it was usually presumed there was something wrong with the theory. After quantum mechanics, the situation became much less clear cut because some implications of quantum mechanics seem very absurd, but then the absurd result turns out to be true in the laboratory. One connotation for 'absurd' in science is, "Well, this could not be!" The issue, though, is this: When a conjecture is made that passes beyond conditions that can be tested, how are we to know whether or not the implication really *is* absurd?

It is the dialectical character of human Reason to seek always for the *practically* unconditioned in one's understanding by which a conceptual equilibrium can be achieved and the mandate of pure practical Reason's categorical imperative can be satisfied. It is an important lesson of the Critical Philosophy that the means for seeking this satisfaction lie with the process of reflective judgment, the judgments of which employ inferences of judgment (ideation, analogy,

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[2] Mark Twain, *Life on the Mississippi*.

induction) taking their ground solely from the subjective side of human Nature. Furthermore, syncretism is the trademark of the development of new and general concepts. This is clearly shown in the study of children's mental development but it also is betrayed by the rush to judgment and by jumping to conclusions. One important difference distinguishing scientific judgment from non-scientific reasoning is the employment of discipline and the diminishment (but not elimination) of ignórance in examinations of factual relationships to theories. Feynman refers to this with his metaphor of "imagination in a straitjacket". It is far more usual in human behavior to find recourse to lazy reasoning. For example, if one accepts the possibility of miracles then one can "explain" everything one does not understand by recourse to miraculous mechanisms. The history of humankind shows us this in every recorded era and every culture that has left records behind.

Science at least makes it a fundamental practice to reject miraculous explanations. But this does not mean that scientists are immune from committing a *saltus* in reasoning or leaving behind a *hiatus* in knowledge or method. When a scientist reifies "probability" (a secondary quantity) he likewise introduces *casus* ("chance") as a crutch for understanding. By his training a scientist does this less often than does the non-scientist, but the scientist is still a human being and no less subject to the unremitting demands of a pure practical Reason that knows no objects and regulates only for the perfection of equilibrium. I hold it to be true that the New Platonism is a behavioral manifestation of this unceasing push by the fundamental law of pure practical human Reason. The discipline of the Critical Philosophy provides an antidote to the dialectic of pure Reason by supplying an alternative to understanding, namely a different concept of assimilation capable of providing *Zweckmäßigkeit* for impulsive reflective judgment. This is the concept of ***undecidability***.

The human habit of rushing to judgment was noted long ago, at the dawn of the Age of Reason, by Francis Bacon. In *Novum Organum* he wrote:

> 19. There are and can exist but two ways of investigating and discovering truth. The one hurries on rapidly from the senses and particulars to the most general axioms, and from them, as principles and their supposed indisputable truth, derives and discovers the intermediate axioms. This is the way now in use. The other constructs its axioms from the senses and particulars, by ascending continually and gradually, till it finally arrives at the most general axioms, which is the true but unattempted way.
>
> 20. The understanding when left to itself proceeds by the same way as that which it would have adopted under the guidance of logic, namely, the first; for the mind is fond of starting off with generalities, that it may avoid labor, and after dwelling a little on a subject is fatigued by experiment. But those evils are augmented by logic, for the sake of the ostentation of dispute.
>
> 21. The understanding, when left to itself in a man of steady, patient, and reflecting disposition (especially when unimpeded by received doctrines), makes some attempt in the right way, but with little effect, since the understanding, undirected and unassisted, is unequal to and unfit for the task of vanquishing the obscurity of things.

> 22. Each of these two ways begins from the senses and particulars, and ends in the greatest generalities. But they are immeasurably different; for the one merely touches cursorily the limits of experiments and particulars, whilst the other runs duly and regularly through them; the one from the very outset lays down some abstract and useless generalities, the other gradually rises to those principles which are really the most common in nature.
>
> 23. There is no small difference between the idols of the human mind and the ideas of the Divine mind; that is to say, between certain idle dogmas and the real stamp and impression of created objects, as they are found in nature [BACO2: 108].

The challenge for any science is, of course, to recognize when an "indisputable truth" truly is indisputable. Bacon classified the "idols of the human mind" into four categories:

> 38. The idols and false notions which have already preoccupied the human understanding, and are deeply rooted in it, not only so beset men's minds that they become difficult of access, but even when the access is obtained will again meet and trouble us in the instauration of the sciences, unless mankind when forewarned guard themselves with all possible care against them.
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> 39. Four species of idols beset the human mind, to which (for distinction's sake) we have assigned names, calling the first idols of the tribe, the second idols of the den, the third idols of the market, and the fourth idols of the theater.
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> 41. The idols of the tribe are inherent in human nature and the very tribe or race of man; for man's sense is falsely asserted to be the standard of things; on the contrary, all the perceptions both of the senses and the mind bear reference to man and not to the universe, and the human mind resembles those uneven mirrors which impart their own properties to different objects, from which rays are emitted and distort and disfigure them.
>
> 42. The idols of the den are those of each individual; for everybody (in addition to the errors common to the race of man) has his own individual den or cavern, which intercept and corrupts the light of nature, either from his own peculiar and singular disposition, or from his education and intercourse with others, or from his reading, and the authority acquired by those whom he reverences and admires, or from the different impressions produced on the mind, as it happens to be preoccupied and predisposed, or equable and tranquil, and the like; so that the spirit of man (according to its several dispositions), is variable, confused, and, as it were, actuated by chance; and Heraclitus said well that men search for knowledge in lesser worlds, and not in the greater or common world.
>
> 43. There are also idols formed by the reciprocal intercourse and society of man with man, which we call idols of the market, from the commerce and association of men with each other; for men converse by means of language, but words are formed at the will of the generality, and there arises from a bad and unapt formation of words a wonderful obstruction to the mind. Nor can the definitions and explanations with which learned men are wont to guard and protect themselves in some instances afford a complete remedy; words still manifestly force the understanding, throw everything into confusion, and lead mankind into vain and innumerable controversies and fallacies.
>
> 44. Lastly, there are idols which have crept into men's minds from the various dogmas of peculiar systems of philosophy, and also from the perverted rules of demonstration, and these we denominate idols of the theater: for we regard all the systems of philosophy hitherto received or imagined, as so many plays brought out and performed, creating fictions and theatrical worlds. Nor do we speak only of the present systems, or of the philosophy and sects of the ancients, since numerous other plays of a similar nature can be still composed and made to agree with each other, the causes of the most opposite errors being generally the same. Nor, again, do we allude merely to general systems, but also to many elements and axioms of sciences which have become inveterate by tradition, implicit credence, and neglect [BACO2: 109-110].

Bacon's "opposite errors" is what Kant called "antinomies of Reason." An antinomy consists of two contradictory propositions, each of which is "proven" to be true by "demonstrating" that the other proposition leads to absurdities or is self-contradictory. Demonstration, however, is only possible for mathematics. As Kant showed in the case of the four general antinomies discussed in *Critique of Pure Reason*, the dialectical error lies in mistaking an infinite judgment (A is not-B) for a negative judgment (A is-not B) or with making one's ideas "too big" or "too small" for Nature.

One modern-day example of a very Kant-like antinomy can be found in opposing views of physicists in regard to the "origin of the universe" question. Those who hold with a Big Bang "explosion" speculate that the universe began out of "nothing" as a result of a "vacuum fluctuation" (which, miraculously, "created" everything in the universe at one mathematical "point", and thereafter "space" began to expand). Opposing this view is the equally fanciful "explanation" that "our universe" is merely one of many "universes".[3,4]

The ideas of "vacuum fluctuations", the "vacuum state", and "vacuum polarization" are some of the most exotic ideas to emerge from the theory quantum electrodynamics. The origin of these terms predated the present-day QED theory, being first used in the context of the original Dirac theory of the electron. If we say the 'meaning' of an idea in physics is how that idea is used in the actual practice of physics, terms like "vacuum state" and "vacuum polarization" refer to purely mathematical operations on secondary quantities. However, the adjective "vacuum" opens these ideas up to becoming what Bacon called idols of the market. Indeed, I find that physicists are somewhat hard-pressed to present any non-mathematical explanation of these ideas. For instance, the *Oxford Dictionary of Physics* tells us the following:

> **vacuum state** The ground state in a relativistic quantum field theory. A vacuum state does not mean a state of nothing. Because one is dealing with a quantum mechanics, the vacuum state has a zero-point energy, which gives rise to *vacuum fluctuations*. The existence of vacuum fluctuations has observable consequences in quantum electrodynamics.
>
> **zero-point energy** The energy remaining in a substance at the absolute zero of temperature (0 K). This is in accordance with quantum theory, in which a particle oscillating with simple harmonic motion does not have a stationary state of zero kinetic energy. Moreover, the uncertainty principle does not allow such a particle to be at rest at exactly the centerpoint of its oscillations.

If, after reading these definitions, you are asking yourself "what kind of 'particle' are we talking about when we're talking about a vacuum state?" and finding this hard to imagination . . . well, welcome to my world. "Geometrodynamicists" are fond of telling us that "space itself" is what is "oscillating" in "the vacuum state," but this is not how QED theorists use this term.

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[3] Gabriele Veneziano, "The myth of the beginning of time," *Scientific American*, vol. 290, no. 5, May, 2004, pp. 54-65.

[4] Max Tegmark, "Parallel universes," *Scientific American*, vol. 288, no. 5, May, 2003, pp. 40-51.

The best known of the "observable consequences" mentioned by the *Oxford* is the Lamb shift, which is a small energy difference measured in the spectrum of hydrogen. At the time of its discovery, it was a phenomenon unexplainable by pre-QED quantum theory[5]. But the *Oxford* does have things a little backwards here in the sense that the 'vacuum state' idea was first proposed as a way to explain the Lamb shift. It was a triumph for QED theory that the same mathematical formalization that described the Lamb shift also describes other measurement phenomena. One example is "noise" in lasers and in detectors used in communication systems.

In quantum electronics one way of describing an electromagnetic field (called 'the Heisenberg picture') involves the use of a mathematical operator we will denote by the symbol q_ν. The Heisenberg principle (i.e., the uncertainty principle) states that there is a limit to how closely we can 'measure' q_ν. (We do not directly measure q_ν. What we actually do is measure something else and then work backwards through the mathematics to get q_ν.) The mean-squared value of the 'uncertainty' in determining q_ν, denoted by the symbol Δq_ν^2, is called the vacuum fluctuation. I will let Dietrich Marcuse, a scientist formerly at Bell Telephone Laboratories, take up the description from here.

> We mentioned . . . that the term "quantum noise" is reserved for the noise originating in the detection process. This quantum noise manifests itself as shot noise in phototubes and photodiodes. However, its origin can be traced to field quantization. It is, therefore, a fundamental quantum phenomenon and as such deserves the name "quantum noise." Some authors use the term quantum noise for the spontaneous emission noise of a maser amplifier. This noise is indeed also caused by a quantum phenomenon and is furthermore closely related to what we call quantum noise. . .
> It is the object of this section to show that the electromagnetic field can not be measured with arbitrary precision owing to its quantum nature. Our inability to measure the field vectors with arbitrary accuracy will be related to an equivalent noise of just the right magnitude so that the uncertainty inherent in such measurements is attributed to the presence of this equivalent noise which we call quantum noise.
>
> [Marcuse follows with a lot of detailed mathematics that I will not reproduce here]
>
> Now we take the step of introducing the equivalent quantum noise. [The equation for the vacuum fluctuation Δq_ν^2] has the meaning of a measuring uncertainty of the amplitude q_ν. We can, however, assume that it is not the measurement that is inherently uncertain but rather blame the fluctuation of the result of the measurement on the presence of some noise, the quantum noise, the square of whose amplitude is just Δq_ν^2.[6]

The first key thing to note here is that this theory involves a defined "rule of correspondence" for going from what is actually observed in the measurement to the underlying secondary quantity (the vacuum fluctuation). The second key thing to note, which is much less obvious unless you happen to work in this area of science, is that the rule of correspondence is

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[5] For this discovery Willis E. Lamb was awarded the 1955 Nobel Prize in physics.

[6] Dietrich Marcuse, *Principles of Quantum Electronics*, NY: Academic Press, 1980, pp. 232-235.

fundamentally tied to the idea that the measurement is measuring something that can be projected to a single 'point in (objective) space' at a particular 'instant in (objective) time'. This projection process is where Heisenberg's principle enters the picture, and this is where we pass from appearances (the measurement in Facet A) and principal quantities (the mathematical description tied directly to the concept of that-which-is-measured) to secondary quantities.

But, one might protest, there are many equivalent ways of expressing things in mathematics. Can we not juggle these equations around and move the vacuum fluctuation into position as a principal quantity? Alas, the answer is no. We will let Nobel laureate Julian Schwinger explain:

> The post-war developments of quantum electrodynamics have been largely dominated by questions of formalism and technique, and do not contain any fundamental improvement in the physical foundations of the theory. Such a situation is not new in the history of physics; it took the labors of more than a century to develop the methods that express fully the mechanical principles laid down by Newton. But, we may ask, is there a fatal fault in the structure of field theory? Could it not be that the divergences – apparent symptoms of malignancy – are only spurious byproducts of an invalid expansion in powers of the coupling constant and that renormalization, which can change no physical implications of the theory, simply rectifies this mathematical error? This hope disappears on recognizing that the observational basis of quantum electrodynamics is self-contradictory. The fundamental dynamical variables of the electron-positron field, for example, have meaning only as symbols of the localized creation and annihilation of charged particles, to which are ascribed a definite mass without reference to the electromagnetic field. Accordingly it should be possible, in principle, to confirm these properties by measurements, which, if they are to be uninfluenced by the coupling of particles to the electromagnetic field, must be performed instantaneously. But there appears to be nothing in the formalism to set a standard for arbitrarily short times and, indeed, the assumption that over sufficiently small intervals the two fields behave as though free from interaction is contradicted by evaluating the supposedly small effect of the coupling. Thus, although the starting point of the theory is the independent assignment of properties to the two fields, they can never be disengaged to give those properties immediate observational significance. It seems that we have reached the limits of the quantum theory of measurement, which asserts the possibility of instantaneous observations, without reference to specific agencies. The localization of charge with indefinite precision requires for its realization a coupling with the electromagnetic field that can attain arbitrarily large magnitudes. The resulting appearance of divergences, and contradictions, serves to deny the basic measurement hypothesis. We conclude that a convergent theory cannot be formulated consistently within the framework of present space-time concepts. To limit the magnitude of interactions while retaining the customary coördinate description is contradictory, since no mechanism is provided for precisely localized measurements.
>
> In attempting to account for the properties of electron and positron, it has been natural to use the simplified form of quantum electrodynamics in which only these charged particles are considered. Despite the apparent validity of the basic assumption that the electron-positron field experiences no appreciable interaction with fields other than electromagnetic, this physically incomplete theory suffers from a fundamental limitation. It can never explain the observed value of the dimensionless coupling constant measuring the electron charge. Indeed, since any charge renormalization is a property of the electromagnetic field, and the latter is influenced by the behavior of every kind of fundamental particle with direct or indirect electromagnetic coupling, a full understanding of the electron charge can exist only when the theory of elementary particles has come to a stage of perfection that is presently unimaginable. It is not likely that future developments will change drastically the practical results of the electron theory, which gives contemporary quantum electrodynamics a certain enduring value. Yet the real significance of the work of the past decade lies in the recognition of the ultimate problems facing electrodynamics, the problems of conceptual consistency and of physical completeness. No final solution can be anticipated until physical science

> has met the heroic challenge to comprehend the structure of the sub-microscopic world that nuclear exploration has revealed.[7]

Who is in a better position than Schwinger to appreciate the limits (as well as the fact that there are limits) to what QED can explain? We can now better understand why Feynman so often, and right up to his last days, called renormalization "this dippy process." Despite the impressive advances made since Schwinger wrote these words in 1956, the theory of quantum chromodynamics (QCD) has *not* resolved these issues, pompous-sounding talk of "grand unified theory" notwithstanding. Schwinger's (and Feynman's) cautions about fundamental limits often seem to go unheeded by the younger generation, who followed in their footsteps with understandable, but nonetheless "idolatrous", enthusiasm for these highly successful theories (QED, QCD).[8]

The moral of the story is this: conjecture based on extrapolation of secondary quantities leads eventually to antinomies and to transcendental illusion. Recognition of a paradox or of multiple possible explanations are two means at our disposal to alert us to the possibility that our reasoning has become dialectical. It is also here where Slepian's indistinguishability principle comes into practical use.

It is a long-standing maxim in science that there can be only one correct version of a theory. Where two inequivalent theories exist, one or the other or both are regarded as having to be necessarily incorrect. More cautious and philosophically-minded scientists call any theory a "model" (and so they are) and refrain from making official commitments to conjectures lying far outside the region where the theory has been put to the test. The boundary of this region is what is referred to as "the frontier of science." But the maxim that there can only be "one true theory" rests upon a dialectical idea of Truth, which is to say the idea of "material truth." However, the only objectively valid understanding of the word 'truth' is congruence of the concept with its object. There is no objectively valid criterion for ascertaining 'material truth.'

Now, our understanding of Nature in science (or, at least, in the 'exact' sciences) is mathematical and it involves concepts of secondary quantities. But given limits in our ability to make observations, there is always a multiplicity of possible expressions of secondary quantities that cannot be distinguished at the level of our observational capabilities. Over time these capabilities are improved, but this merely changes the threshold of distinguishability in observation and, potentially, allows some, but not all, secondary quantities to be ruled out in explanation. Below this threshold of distinguishability the remaining (and sometimes some new)

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[7] Julian Schwinger, "Preface," in *Selected Papers on Quantum Electrodynamics*, J. Schwinger (ed.), NY: Dover Publications, 1958, pp. xv-xvii.

[8] Changing the "present space-time concepts" is a central focus of "string theory."

mathematical concepts of secondary quantities are equally congruent with observable appearances. In this sense and understood in this context, *all* these possible models are 'true' because all are congruent with the object of appearance.

Tucked away in one tiny and largely neglected corner of the science of system theory is a sub-discipline known as "set membership theory" (SMT).[9] SMT originated in 1968 within the control systems community as a pragmatic response to some particularly nasty technical issues at the heart of so-called "optimum control" methods. The SMT community is tiny, publishes not very many papers per year, has no conferences of its own, and generates next to no publicity. It has no elected "leading expert", although if a vote were called I would probably vote for Dr. John Deller of Michigan State.

SMT explicitly takes into account the consequences of limitations in measurements. Its fundamental paradigm has roots in information theory and can be stated as follows: ***All models consistent with all a priori knowledge of the (real) system and all observational data taken from measurements on that system are solutions for that system***. In addition to its original applications in control systems, SMT has been applied to problems in communication theory, signal processing theory, system (model) identification, and system parameter estimation. It has even been shown to be capable of spotting inconsistent mathematical structures used to model a complicated physical system (i.e., incorrect system models).[10]

Although it is virtually unknown outside its "parent applications" just mentioned, the SMT paradigm has this significance for our present discussion. It demonstrates that there is at least one mathematically rigorous way of dealing with the thorny issue of indistinguishability between mathematical models involving secondary quantities. SMT denies the maxim of one single unique model (theory).[11] The paradigm can be alternatively stated in the following way: ***All the members of the SMT solution set are equally consistent with all our knowledge of the system and all data obtained from measurements of the system and therefore we cannot make an objectively valid decision in choosing one solution over any other in the set***.

Unlike the usual case in competing theories, where evaluation is based on logical judgments of the positive (A is B) and negative (A is-not B) type, SMT evaluates on the basis of the infinite logical judgment (A is not-B) and merely *excludes* proposed models (theories) from the set of possibilities. If in the end this results in such findings as "Signals are time-limited AND signals

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[9] Recall from Chapter 23 that the idea of a 'set' is objectively valid in mathematics.

[10] S.G. McCarthy and R.B. Wells, "Model order reduction for optimal bounding ellipsoid channel models," *IEEE Transactions on Magnetics*, vol. 33, no. 4, 1997, pp. 2552-2568.

[11] If one has a finite number of models and it so happens that SMT methods reject all but one of these, then SMT does return a unique finding. However, it does not *presume* there to be a unique finding.

are band-limited" (resolution of the bandwidth paradox problem), well then such a finding is true by the only objectively valid meaning we can give the word "truth": congruence between the concept and its object. The SMT principle is **a principle of formal undecidability**.

In light of Gödel's theorems, this will come as no surprise to, and not even rate a shrug from, the mathematicians. They have known about formal undecidability for almost eighty years. But it is likely to be a hard idea to become used to for the rest of science. Nonetheless, the principle of formal undecidability is a necessary principle for both the development of an applied metaphysic and for the evaluation of scientific theories. We set it down here as part of the Critical Method in science.

As it presently stands, SMT is not yet developed fully enough to serve all areas of science. A task to be accomplished, both in the development of a science of mental physics as well as for the development of applied metaphysics for other special sciences, is to extend the reach of SMT. This task I see as one of the key tasks in the development of Critical mathematics.

§ 7.4 Concluding Remarks

The time has come to bring this treatise to a close. Your author appreciates your tolerance and your patience in suffering through this very large and complex work. I apologize that I could not find a way to make this treatise more brief while also making it self-contained.

There will be, I am confident, many learned objections raised to this or to that part of the theory presented here, and they will likely come from 360° of directions on the map of scholarly disciplines. Some I already anticipate but left un-dealt-with in these pages, deeming that there is little productive point to be gained in responding to them prior to the publication of this theory. Others, I am sure, I have not anticipated at all and so these must wait until I hear about them. Some issues, I expect, will be deep and subtle and will require very keen analysis in order to give a proper Critical response; the history of science teaches us to expect this situation. Some people, much like some students encountering quantum mechanics for the first time, will find Kant's radical re-appraisal of metaphysics too much at odds with their own sense of "how things are" and will be unable to accept the theory on that subjective basis. This, too, is to be expected. There is no theory in all of history that has won universal acceptance upon first appearance. The old joke in science is, "New theories are not accepted until the older generation dies off." Kant's theory is, of course, not new; but it is not very well understood by very many people, either. The number of people who misunderstand it is far greater than the number of people who do not, and the number of people who have never heard of it is far greater still. If this treatise has helped bring it to a wider community of people, we may call it "new" in that context.

In regard to the genesis of a new science of mental physics, a work to which I hope to be able to devote my own efforts for however many productive years remain to me, I see the first steps as these. We must develop a Critical applied metaphysic, distinguishing its objects, detailing our sources of knowledge of these objects, and delimiting the boundaries of objective validity of these objects. A key undertaking in this is, I claim, the development of a transcendental Logic of Meanings. This accomplishment would fulfill a long-standing goal of Piaget's and the topic of his last work. This metaphysic must ground the linkage between biology, psychology, and the other disciplines participating in neuroscience. It must also serve as a metaphysic for Critical Mathematics.

The development of Critical mathematics is a task going hand in hand with the development of the metaphysic because the objects of *nous* are secondary quantities. It is my hope that this work will attract the efforts of gifted mathematicians (who are capable of carrying it out far better than I). I hope the potential for providing to mathematics the fulfillment of its old dream – namely, what some call the ideal of the Euclid myth, the ability of mathematics to bring forth true and certain knowledge of Reality to the greatest extent objectively possible – will prove to be an irresistible attraction. The benefit to science – not just mental physics but to all of science – will come in the form of a clear delimitation of which mathematical constructs have objective validity (principal quantities) and which belong to hypothetical mathematics (secondary quantities).

The system of transcendental Logic and Critical mathematics, combined, will comprise the applied metaphysic of mental physics. The science of psychology will be one direct, and I hope immediate, beneficiary. Psychology has never enjoyed a common base either in the form of a metaphysic or even in the form of a shared paradigm that stood up for long. The consequence of this is visible today. It may be unpleasant to hear talk of psychology as being in a crisis of disintegration, but one who doubts this should page through Reber's *Dictionary* and read Morton Hunt's *The Story of Psychology*. Psychology has never been an "exact science"; I say it can be. I say we should bring it to this.

Mental physics cannot ignore the interrelationships of mind-body (*nous-soma*) reciprocity. It therefore cannot stand apart from the biological and pharmacological units of neuroscience, nor can it stand apart from computational neuroscience – a discipline tasked with supplying quantitative models for the science but the findings of which are in the majority *ex post facto* with few demonstrated successes in making predictions or in steering the direction of laboratory research in the neurobiology or neuropsychology wings of neuroscience. We must bring system theory to neuroscience.

This treatise presents foundations, both through the underlying Critical system and the principles of method, i.e. the precepts of ontology and the principle of formal undecidability. But let us make no mistake: It will be no small task to bridge between the general theory and its consequences for science by means of applied metaphysics. We shall need the efforts of those gifted philosophers attracted to metaphysics and willing to see it and treat it as Critical *science*. Either we do this, or we leave philosophy to languish in the doldrums, largely ignored and ignorable by the wider communities of human intellectual endeavor. Either we succeed in re-making philosophy into not merely a science but the *first* science – the science of the general – or face the inevitable dismissal flung at us by science: *Who needs you?* The benefits to humankind will be great when we succeed; the price to be paid for not trying is ignominy.

It is said that a professor does not know when to stop talking – something I think has likely crossed the mind of more than one reader. But our present journey of discovery is now at its end. So let us pretend the bell has rung and set down the word your author has been longing to write – and perhaps you yourself have been longing to read – for many, many pages:

finis.

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