second assumption and find it to be false. Accelerations do affect the rates of ideal clocks. This brings us face to face with a problem similar to that which confronted Einstein in 1911 (*8*).

If it is now known that, as a result of his accelerations, an observer's time scale is scaled by an amount $1 + U$, where U is the acceleration potential, and if we are to retain the spirit of relativity and insist that even accelerated observers must measure c for the velocity of light, we must be prepared, as Einstein was, to appropriately scale the distance measure for inertially accelerated observers. This implies that inertial accelerations will affect an accelerated observer's perception of the world geometry with

$$g_{\mu\nu} \rightarrow g_{\mu\nu}'$$

where $g_{\mu\nu}'$ differs from $g_{\mu\nu}$ due to the effects of acceleration.

Such a viewpoint is consistent with the theory of gravitation. The world geometry described by the tensor $g_{\mu\nu}$ is fundamental and absolute and would be deduced from measurements with ideal clocks and rods by any observer in an inertial frame, that is, in free fall. Since the geometry of the space-time manifold is absolute, any departure of an observer's motion from the path of a timelike geodesic is absolute. Accelerated observers do not travel timelike geodesics of the space-time manifold. The departure of an observer from free fall (a state of zero acceleration) is an absolute condition which can be measured locally with accelerometers. The results presented in this article indicate that such inertial accelerations, uniquely and absolutely determined for each observer, will affect the observer's measurement of space and time. Such effects on an observer's measurements are not described by the present theory of gravitation since they have been assumed away. The present theory of gravitation deals correctly with observers in free fall and cannot be generalized to inertially accelerated observers without the introduction of an additional hypothesis (*4*, p. 234).

In searching for a first-order theory which would account for the effects of acceleration on an observer's perception of the world geometry we have been guided by the principle of equivalence and the necessary correspondence between the limiting case of zero acceleration (motion along a timelike geodesic of the space-time manifold) and the standard theory of gravitation. According to our view, the observer's perception of the world geometry depends on his state of acceleration, and we believe that a theory which describes these effects correctly would constitute true "general theory of relativity," which would include the theory of gravitation as a special case.

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15. We are indebted to D. E. Smylie for his encouragement and critical reading of the manuscript and for the provision of the BIH reports. This research was supported by National Research Council of Canada operating grants to W.H.C. and D. E. Smylie.

On the Role of Themata in Scientific Thought

Gerald Holton

When the historian of science studies a product of scientific work—a published paper, a laboratory record, a transcript of an interview—he is dealing first of all with an event. A number of different facets of the event can engage his attention. One can distinguish at least eight such facets, corresponding to different types of interesting questions:

First is of course the understanding of the scientific content of the event (E) at a given time, both in contemporaneous terms and, separately, in terms of what we now believe to be the case. What did the scientist claim was at issue? What was he in fact confronted with? For this we need to establish the awareness, within the area of public scientific knowledge at the time of the event, of the so-called scientific facts, data, laws, theories, techniques, lore. I would include under this heading the larger part of historical research on what are called scientific world views, paradigms, and research programs; chiefly, however, historians and scientists are still concerned with digging out the concepts and propositions embodied in the event studied and with rendering them in empirical and analytical language.

Second is the time trajectory of the state of shared (that is, "public") scientific knowledge (let us call it S_2) that led up to and perhaps goes beyond the time chosen above. Establishing this means, so to speak, the tracing of the world line of an idea or a subject of research, a line on which E is a point. Whether we are studying the problem of falling bodies from Kepler to Newton, or the flowering of quantum electrodynamics from Feynman to the last issue of *Physical Review Letters*, under this heading we are dealing with antecedents, parallel developments, continuities and discontinuities, and the like.

This tracing of conceptual development and of the "context of justification" is the most frequent and strongest activity of historians of science and historically inclined science educators.

Third is the more ephemeral personal aspect of the activity in which E is embedded. Here we are in the context of discovery, trying to understand the "nascent moment," which may be poorly documented and not necessarily appreciated or understood by the agent himself. Except for work on a few such figures as Kepler or Einstein, scientists until recently have been rather impatient with such studies, and so have philosophers. The very institutions of science—the methods of publication, the meetings, the selection and training of young scientists—are designed to minimize attention to this element. The success of science itself as a shareable activity seems to be connected with this systematic neglect of what Einstein called the "private struggle." Moreover, the apparent contradiction between the often "illogical" nature of actual discovery and the logical nature of well-developed physical concepts is perceived by some as a threat to the very foundations of science and rationality itself.

The alternative path is not easy. In one of his interviews, Einstein urged historians of science to concentrate on comprehending what scientists were aiming at, "how they thought and wrestled with their problems." But he pointed out that the scholar would have to have sufficient insight, a kind of *Fingerspitzengefühl* both for the content of science and for the process of scientific research, as solid facts about the creative phase are likely to be few; and that, as in physics itself, the solution to historical problems may have to come by very indirect means, the best outcome to be hoped for being not certainty but only a good "probability" of being "correct anyway."

One of the nicest testimonials on the contrary orientations of scientists and of historians of science was provided by P. A. M. Dirac not long ago. He agreed to lecture about his work at the summer school on the History of 20th-Century Physics at Varenna in 1972. At the end of the first week, after having sat in on the lectures by the historians of science, Dirac began his own first lecture in this manner (*1*):

> . . . I have learned a great deal here, not only individual facts about the history of science, which I have picked up from various lectures, but I have learned to appreciate the point of view of the historian of science. It is really a very different point of view from that of the research physicist. The research physicist, if he has made a discovery, is then concerned with standing on the new vantage point which he has gained and surveying the field in front of him. His question is, Where do we go from here? What are the applications of this new discovery? How far will it go in elucidating the problems which are still before us? What will be the prime problems now facing us?
>
> He wants rather to forget the way by which he attained this discovery. He proceeded along a tortuous path, followed various false trails, and he doesn't want to think of these. He feels perhaps a bit ashamed, disgusted with himself, that he took so long. He says to himself, What a lot of time I wasted following this particular track when I should have seen at once that it will lead nowhere. When a discovery has been made, it usually seems so obvious that one is surprised that no one had thought of it previously. With that point of view, one doesn't want to remember all the work that led up to the making of the discovery.
>
> Now, that is just the opposite to what the historian of science wants. He wants to know the various influences at work, the various intermediate steps, and he may have some interest in the false trails. These are rather contradictory points of view, and most of my life has been spent with the point of view of the research physicist, and that involves forgetting as quickly as possible the various intermediate steps.
>
> However, with the understanding of what the historians of science are concerned with, I have tried to think over the past, and have done my best to remember the various incidents, things that happened 50 years ago. I have tried to figure out the influences, the effect of the various teachers that I had and the training that I received, to see how these things led me to the style of work which I followed in later life.

A fourth component of historical research is indeed the establishment of the time trajectory of this largely private scientific activity (S_1)—the personal continuities and discontinuities in development. Now the event E at time *t* begins to be seen as the intersection of two trajectories, of two world lines, one for public science and one for private science, to use a shorthand terminology which is useful enough if not pushed too hard.

Fifth, parallel to the trajectory of S_1 and shading into it as one of its boundaries is a band tracing the psychobiographical development of the person whose work is being studied. We are dealing here with the new and tantalizing field which explores the relation between a person's scientific work and his intimate style of thought and life. Frank Manuel's *A Portrait of Isaac Newton* (*2*) is perhaps the best example available.

Sixth is unavoidably the study of the sociological setting, conditions, influences, arising from colleagueship, the dynamics of teamwork, the state of professionalization at the time, the institutional means for funding, for evaluation and acceptance, and quantitative trends. Here we deal with the fields of science policy studies and sociology of science in the narrower sense.

Seventh, a similar band, parallel to and shading into the trajectories of S_1 and S_2, deals with cultural developments outside science that influence science or are influenced by it—with questions concerning the feedback loops, science-technology-society and science-literature, on which some of the most interesting work today is being done.

Finally, there is the logical analysis of the work under study. In my own development, first as a student of P. W. Bridgman and Philipp Frank and later as their colleague, interest in and respect for a valid analysis of the logic of science in fact preceded work in the analysis of the more strictly historical aspects of a case.

These eight areas of study are not separated by hard barriers. To be sure, each has invited its own specialization and thereby its own operational self-definition. For each we could quickly put forward the names of heroes and the shape of future hopes of development—though we might now all agree, with various intensities of regret, that the resolution of a real case in the history of science in all its ambiguities and interdisciplinary connections into separable components is, after all, a reductionistic strategy which our human limitations force or doom us to employ.

Toward Thematic Analysis

The method of dealing with complex entities by resolution or reduction found its use in science itself very early. One recalls, for example, the passage in the Second Day of Galileo's *Dialogo*,

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where Salviati and Simplicio are discussing the motion of an object released from the mast of a moving ship. Simplicio refuses Salviati's proposal to resolve the motion into a horizontal and a vertical component, one for free fall straight to the center of the earth, the other with constant velocity in the direction of initial motion. Perhaps we should credit Simplicio's resistance to a premonition that the whole method of resolution and reduction is precarious and has no more necessity than any other methodological thema—that is, it is neither verifiable nor falsifiable, and its usefulness depends entirely on how soon you are satisfied with your results.

As we now know, Salviati was grossly exaggerating. Resolving the motion of the falling object into two components in order to understand motion and its causes is only the first step in an essentially infinite chain of resolutions. If one wants more detail about the motion, other laws enter. The appearance of the Coriolis force is responsible for an eastward deflection of the object. The laws for falling bodies in real media at various Reynolds numbers have to enter to calculate the effect of friction and turbulence. The more detail one wants to know, the more resolutions become necessary. The process would have become infinitely regressive if an Occam's Razor had not been invented in our century for cutting off all side effects below a certain limit. Quantum physics did give us a way to stop, owing to the uncertainty principle and the finite size of Planck's constant; they extinguish the meaningfulness of all further questions.

And there is another lesson. The two components Salviati chose, while they were plausible enough and even turned out to be useful, were not endowed with any provable necessity over any other set of two or more components of motion that might have been imagined. I mention this to acknowledge that my list of components is not to be taken as the recital of an unchangeable, sacred Eightfold Way. On the contrary, one reason for making the list was to be able to conclude that it is incomplete in an important respect. That is to say, there remains a set of questions which are irresistible (to me, at any rate); which cannot be handled naturally in this eightfold scheme at all; and which lay bare a link between scientific activity and humanistic studies, a link that few have studied so far.

Any listing of such questions will have to include these: What is constant in the ever-shifting theory and practice of science—what makes it one continuing enterprise, despite the apparently radical changes of detail and focus of attention? What element remains valuable in theories long after they have been disproved? What are the sources of energy that keep certain scientific debates alive for decades? Why do scientists—and for that matter also historians, philosophers, and sociologists of science—with good access to the same information often come to hold so fundamentally different models of explanation? Why do some scientists hold on to models of explanation that are contrary to the evidence, sometimes at enormous risks?

Why do scientists privately not accept a dichotomy between the context of verification and that of discovery, and publicly often accept it? If it is true, as Einstein believed, that the process of formulating laws purely by deduction is "far beyond the capacity of human thinking," what may be guiding the leap across the chasm between experience and basic principle? What is behind the obviously quasi-esthetic choices which some scientists make—for example, in rejecting as merely "ad hoc" a hypothesis that to other scientists may appear to be a necessary doctrine? Are the grounds from which such choices spring confined to the scientific imagination, or do they extend beyond it?

To handle such questions I have proposed a ninth component in the analysis of a scientific work. I have used for it a time-honored term, "thematic analysis," familiar from somewhat related uses in anthropology, art criticism, musicology, and other fields. In many (perhaps most) past and present concepts, methods, and propositions or hypotheses of science, there are elements that function as themata, constraining or motivating the individual and sometimes guiding (normalizing) or polarizing the scientific community. In the scientists' own public presentations of their work, and during any ensuing scientific controversy, these elements are usually not explicitly at issue. The discussion seems to concern chiefly the empirical content and the analytical content, that is, the repeatable phenomena and the propositions concerning logic and mathematics. By way of a very rough analogy, I have suggested that those two elements be considered the x and y coordinates of a plane within which the discussion seems chiefly to proceed, since the "meaningfulness" of concepts is tested by the resolution of concepts or propositions into those elements—"meaningful" in the sense that agreed-upon rules generally exist for verifiability or falsifiability of the statements made in that language.

Thus, in R. A. Millikan's famous oil drop experiment the question whether or not the electric charges on small objects always come in multiples of some fundamental constant, called the charge of the electron, could in principle have been resolved quickly by coming to terms on how and what was being observed through the telescope or ultramicroscope when a particle was seen to move in the view field, and whether and how to amend the equation for Stokes' law for the fall of small objects by extrapolation of a correction term. If that were all, the lengthy fight about the existence of a postulated "subelectron" would never have happened. But in 1910, and continuing for some years afterward, the controversy between Millikan and Felix Ehrenhaft was joined—at the intersection, as it were, of two sets of world lines.

Analysis of the published research reports, of the expressed motivations, and of the ever-hardening attitudes of the protagonists on opposite sides of the question shows here, as in other cases, the strong role of an early, unshakable commitment to opposite themata on the part of the opponents; the one to the thema of discreteness as the fundamental explanatory principle operating in electricity, the other to the thema of the continuum and hence antiatomism. In Millikan's case the commitment to an atomistic explanation of electricity predated his experimental verification and indeed helped him to pick his way through initially indifferent data to support his contention. In the other, the growing dedication to the antithema led to a veritable flood of counter-experiments.

The themata that appear in science can, in our very rough analogy, be presented as lying along a dimension orthogonal to the (xy) plane in which verification and falsification can take place, hence somewhat like a z axis rising from it. While the xy plane does suffice for most discourse within science in the sense of a public, consensual activity, it is the three-dimensional (xyz) space which is required for a more complete analysis—whether historical, philosophical, or psychological—of scientific statements, processes, and controversies. (My argument is not to introduce thematic discussions or even a self-conscious awareness of themata into the practice of science itself. It is indeed one of the great advantages of scientific activity that in the *xy* plane many questions—for example, concerning the "reality" of scientific knowledge—cannot be asked. Only when such questions were ruled out of place in a laboratory did science begin to grow rapidly.) It is fruitful to make distinctions between three different uses of themata: the thematic concept, or the thematic component of a concept (examples I have analyzed are the use of the concept of symmetry and of the continuum); the methodological thema (such as the preference for expressing the laws of science where possible in terms of constancies, or extrema, or impotency); and the thematic proposition or thematic hypothesis (exemplified by overarching statements such as Newton's hypothesis concerning the immobility of the center of the world, or the two principles of special relativity theory).

The attitude I have taken in the task of identifying, ordering, and categorizing thematic elements in scientific discussions is to some degree analogous to that of a folklorist or anthropologist who listens to the epic stories for their underlying thematic structure and recurrences. While the analogy leaves much to be desired, there are more than superficial relations. For example, the awareness of themata which are sometimes held with obstinate loyalty helps one to explain the character of the discussion between antagonists far better than do scientific content and social surroundings alone. The attachment of physicists such as H. A. Lorentz, Henri Poincaré, and Max Abraham to the old electromagnetic world view and their discomfort with Einstein's relativity theory become a good deal more understandable when the ether is thought of as operating as the embodiment of thematic concepts (for example, of the absolute and the plenum). Thus in their obituary for Abraham, Max von Laue and Max Born wrote perceptively (*3*):

> [Abraham] found the abstractions of Einstein disgusting in his very heart. He loved his absolute ether, his field equations, his rigid electron, as a youth loves his first passion whose memory cannot be erased by any later experience. . . . His opposition was grounded in physical, fundamental persuasions to which he, purely in accord with his feelings, held on as long as possible. . . . [As Abraham himself once said] against the logical coherences he had no counterarguments; he recognized and admired them as the only possible conclusion of the plan of general relativity. But this plan was to him thoroughly unsympathetic, and he hoped that the astronomical observation would disconfirm it and bring the old, absolute ether again into honor.

A finding of thematic analysis that appears to be related to the dialectic nature of science as a public, consensus-seeking activity is the frequent coupling of two themata in antithetical mode, as when a proponent of the thema of atomism finds himself faced with the proponent of the thema of the continuum. Antithetical ($\Theta\bar{\Theta}$) couples—such as evolution and devolution, constancy and change, complexity and simplicity, reductionism and holism, hierarchy and unity, the efficacy of mathematics (for example, geometry) versus the efficacy of mechanistic models as explanatory tools—are not too difficult to discern, particularly in cases that involve a controversy or a marked advance beyond the level of common work (*4*).

I have been impressed by how few themata there are—at least in the physical sciences. I have found about 50 singlets and doublets and occasional triplets so far, and I suspect the total will turn out to be less than 100. The appearance of a new thema is rare. Complementarity in 1927 and chirality in the 1950's are two of the most recent such additions in physics. Related to that is the antiquity and persistence of themata, right through scientific evolution and "revolution." Thus, the old antithesis of plenum and void surfaced in the debate early this century on "molecular reality"—indeed, it can also be found in the work of contemporary theoretical physicists. One may even predict that, no matter how radical the advances will seem in the near future, they will with high probability still be fashioned in terms of currently used themata.

The persistence in time, and the spread in the community at a given time, of these relatively few themata may be what endows science, despite all its growth and change, with what constant identity it has. The interdisciplinary sharing of themes among various fields in sciences tells us something both about the meaning of the enterprise as a whole and the commonality of the ground of imagination that must be at work.

An Illustration

To illustrate some of these points, and to show that current as well as historical cases are amenable to this analysis, I want to focus on an example in one of the liveliest fields of physics today, as embodied in publications of Steven Weinberg (*5*). The line tracing the development of Weinberg's thoughts intersects the trajectory of a stream of developments in quantum electrodynamics initiated by Enrico Fermi in 1934 and now basing itself on techniques started independently in the late 1940's by R. P. Feynman, Julian Schwinger, Freeman J. Dyson, and Sinitiro Tomonago. Other points on the trajectory include recent discoveries by groups at CERN, Argonne Laboratory, and the National Accelerator Laboratory. In thematic terms, the "event" we shall study is only the latest in a very old sequence of attempts, reaching past many revolutions and heady victories back to the first scientist of recorded history; for the main preoccupation is the identification of the fundamental constituent of which all matter is presumed to be made.

To put it briefly, Weinberg, his collaborators, and other groups have been working on the problem of finding common ground between the four types of interaction ("forces") that are now believed to account for all physical phenomena: the gravitational interaction that all particles experience, the electromagnetic force that accounts for phenomena involving charged particles and the interaction of light with matter, the "strong" nuclear force that acts between members of the large family of elementary particles called hadrons, and the "weak interaction" postulated to describe extremely short-range interactions of some elementary particles (such as the scattering of a neutrino by a neutron and the radioactive decay of a neutron into a proton, an electron, and an antineutrino).

In 1967, Weinberg (and, independently, Abdus Salam of Trieste) proposed that the electromagnetic force and the weak interaction are essentially connected. Each of the four types of interaction has been considered to be the result of processes analogous to radiation or absorption between two interacting objects, the particle radiated or absorbed being characteristic for each of the interactions. Thus electromagnetic phenomena are due to the exchange of the massless photon, and the weak interaction is mediated by the so-called intermediate vector boson (IVB) which, if it is found to exist, will have to be exceedingly massive. Weinberg's proposal was that the massless photon and the very massive IVB are close relatives—that the IVB's are by and large members of the photon family but get their mass (the appearance of their difference) by virtue of being associated with broken gauge symmetry groups.

At the time Weinberg proposed the theory, "there was," he now notes (*6*), "no experimental evidence for or against it, and no immediate prospects for getting any." To this day the IVB's cannot be produced directly (for instance, in accelerators), but indirect evidence for their existence has been reported. In a paper published under the names of 55 investigators from seven institutions in a pan-European collaboration at the CERN laboratory (*7*), two events were found in which a muneutrino was scattered by an electron, and several hundred events in which a mu-neutrino was scattered by a neutron or a proton. (The latter reaction showed up nicely also on more recent experiments at Argonne National Laboratory and the National Accelerator Laboratory.) This is evidence that the "neutral current" reaction, a new kind of weak interaction predicted by Weinberg involving the postulated neutral IVB, may be taking place, and so indirectly supports the theory which makes these particles a member of the same family as the photons.

Moreover, strong interactions also become amenable to calculations with the same methods as are used for weak and electromagnetic interactions. It is possible, therefore, that the strong interactions are caused by exchange of particles that belong to the same family as the photon and the IVB. "If these speculations are borne out by further theoretical and experimental work," Weinberg says in the last sentence of his most recent survey (*6*, p. 59), "we shall have moved a long way toward a unified view of nature."

Now let us go to the beginning of this same report, which is entitled "Unified theories of elementary-particle interaction," and look at it through eyes alert to themata. What, then, are the thematic conceptions, methodological themata, and thematic suppositions that inhere in this search for the IVB's and their photon-like family membership?

We can readily catalog a few of the more evident themata when we carefully scan just the first page of the article (*8*). It begins:

> One of man's enduring hopes has been to find a few simple general laws that would explain why nature, with all its seeming complexity and variety, is the way it is. At the present moment the closest we can come to a unified view of nature is a description in terms of elementary particles and their mutual interactions. All ordinary matter is composed of just those elementary particles that happen to possess both mass and (relative) stability: the electron, the proton and the neutron. To these must be added the particles of zero mass: the photon, or quantum of electromagnetic radiation, the neutrino, which plays an essential role in certain kinds of radioactivity, and the graviton, or quantum of gravitational radiation. . . .

What strikes us at once is the acknowledgment that "one of man's enduring hopes has been to find a few simple general laws" and thereby obtain a "unified" (the first word of the title) theory. Unification or synthesis, with its promise of increased understanding through increased economy of thought, is a member of a triplet of themata, one of its antithetical aspects being multiplicity (or complexity, variety), the other being the theme we discussed before, that of resolution rather than synthesis. Each of these three members of the triplet has its uses. Here, clearly, unification is taken to be preeminent.

". . . Why nature, with all its seeming complexity and variety, is the way it is." Kepler, who asked in the preface of the *Mysterium Cosmographicum* why the planets are at the distances they are, of the number and with the motions which we find them to have, "and not otherwise," would have agreed with this description of one of man's enduring hopes. So would most scientists since. The second sentence, however, bares a preconception which not all scientists will share. We find here a new thematic commitment, that of constructing the desired unified view of nature out of "elementary particles and their mutual interactions." We hear the echo of Democritus' "all is atoms and void." But Schrödinger and his followers, to whom the fundamental tool of explanation was the continuum, would not have agreed; nor would Einstein; nor would Heisenberg agree in his latest phase, since he now claims that one cannot build matter out of matter but must seek the base in formal principles, along lines he ascribes explicitly to Plato. Nor, of course, would biologists, psychologists, or social scientists be satisfied with this particular unified view of *nature*, in terms of particles and their interactions. A choice has been made here, though a choice that promises indeed a breathtaking view of *a* nature.

What is being conveyed in Weinberg's opening by "elementary"? A few sentences later it is defined to mean that there is not now "any successful theory that explains the elementary particles in terms of more elementary constituents." Some day, to be sure, one may find "still more elementary constituents, named quarks"; but until that time, so long as "strenuous efforts" make it "impossible to break particles," they are elementary.

Their quality of being elementary anchors the whole arrow of explanation, upward from these presumed elementary particles to the antithetical entities, constructs (such as nuclei, atoms, or ordinary matter, all of which are "composed" of elementary matter). The antiquity of that quest, from Thales to Prout to J. J. Thomson to our day, is evident. These elementary particles, then, are today's true "atoms" in the sense of the Greek *atomos*. They form one leg of another triplet of themata, the second being the construct made of and explained by these atoms or elementary quanta, and the third being the notion of the continuum, the indefinitely cuttable (*9*).

The list of elementary particles then consists of the electron, the proton, and the neutron. "To these must be added the particles of zero mass: the photon . . . the neutrino . . . and the graviton." We are here clearly in a world of particulate discreteness; although the wave property that inheres in such particles is of course not doubted, it simply is not part of the image that has captured attention and primacy (*10*).

The number and variety of elementary particles, Weinberg says, is "bewildering." But there are ways of retaining sanity and gaining insight by mastering the bewildering variety. The ordering of chaos by means of the concept of hierarchy or levels or categories—a manageable few, just four—comes to the rescue as a methodological theme. The division into four categories —gravitation, electromagnetic interaction, strong interactions, and weak interactions—is not merely a separation into separate pigeonholes for very different birds. There is a real hierarchy here which orders the subsections, showing a sequence of ranges of interactions, from infinity to much less than 10^{-14} centimeter.

Already one can see from this brief outline that, as Weinberg puts it, "a certain measure of unification has been achieved in making sense of the world." Helping to make sense of the world in a way not possible through the demands of logicality alone is indeed one of the chief functions of a thema. "We are still faced, however, with the enormous problem of accounting for the baffling amount of elementary-particle types and interactions." Methodologically, the theory evokes more than an echo of an older scheme of fourfold categories, one so magnificently successful that it helped to rationalize the observable phenomena for some 2000 years: the four Elements, with their own internal hierarchy, from lightest to heaviest, and their own rules of interaction. However, the new unification through hierarchical ordering promises among its many advantages that two and perhaps three of the forces in the four categories "have an underlying identity."

The way to discover this identity is through analogies in behavior which would collapse the superficially different entities to a state in which they share something more than membership in a hierarchical order. This quest for something more is answered by turning to the conception of family [for example (*6*, p. 55): "Our hopes of perceiving an underlying identity in the weak and electromagnetic interactions lead us naturally to suppose that there may be some larger gauge symmetry that forces the photon and the intermediate vector boson into a single family"]. The chief explanatory tool on the road to greater simplicity is this "family" connection, existing despite an "appearance" of great differences—for example, the difference between the photon's zero mass and the necessarily very large mass of the intermediate vector boson. Throughout this article of Weinberg's and many others in this field, one of the recurring conceptions is precisely this splendid one of groups, families, and superfamilies ("superfamilies of eight, ten, or even more members") (*11*). The familial relationships between the elementary particles are far more profound than in the ad hoc families that were discovered in the chemical periodic table in the last century or in, say, the work of Linnaeus. But the methodological use as a tool of explanation is not qualitatively different.

Let me take the occasion of the surfacing of this fine anthropomorphic word to go back to Weinberg's opening page, where reference is made to "a few additional short-lived particles" and we are told that "we can create a vast number of even shorter-lived species." Elementary particle physics is sometimes wryly referred to as zoology; and it is shot through and through with themes that may well have, as many themes seem to me to have, their origins in a part of the imagination that was formed prior to the conscious decision of the researcher to become a scientist. The technical report of, say, the analysis of a bubble chamber photograph is cast largely in terms of a life-cycle story. It is a story of evolution and devolution, of birth, adventures, and death. Particles enter on the scene, encounter others, and produce a first generation of particles that subsequently decay, giving rise to a second and perhaps a third generation. They are characterized by relatively short or relatively long lives, by membership in families or species.

Listening to these village tales told by physicists, one is aware that the terminology may initially not have been "seriously" meant. Yet the life-cycle thema works, and so do a number of other themata imported into the sciences from the world of human encounters. It has, incidentally, always amused me to see how strenuously the psychologists of the period around the turn of the century tried to gain added respectability by borrowing concepts from physics for the description of human relationships. Evidently they were unaware that they were reimporting conceptual tools when they themselves were closer to the real thing. One is reminded of the story of the bank building in Athens, under the Acropolis, which looked like a particularly bad copy of a Greek temple. It turned out that the architect had not taken as his model one of the great temples right at hand, but had gone to a much more fashionable source. He was basing himself on the design of a bank in Berlin which in turn had been derived from a distant, third-rate copy of an idealized Greek temple.

We have not yet finished with Weinberg's first page. Several other magnificent themata begin to show themselves: isotropy and homogeneity (for example, particles of the same species are, as far as we now know, "absolutely identical, whether they occupy the same atom or lie at opposite ends of the universe"); symmetry—a concept which I believe was first used explicitly and seriously in modern physics on the first page of Einstein's 1905 paper on relativity; and conservation ("of energy and momentum at every instant").

On later pages we would encounter the following additional themata, among others: the efficacy of geometrical representation (such as Feynman diagrams), the efficacy of integers as explanatory tools (the debt of modern quantum mechanics to the holiest precept of Pythagoras), again conservation (of charge), infinity and finiteness (of mass), more on symmetry principles (*12*). And above all, models (*6*, p. 57); the word "model" is probably one of the most frequently used words in the writings by theoretical physicists.

In this manner we are brought to the last sentence in the paper. It has been quoted above, but we can now look at it in a somewhat new light: "If these speculations are borne out by further theoretical and experimental work [meaning, by analytical or formalistic as well as empirical content, or by *y*- and *x*-axis representations] we shall have moved a long way toward a unified view of nature"—that is, toward the fulfillment of one of man's enduring hopes, hopes that find expression in his themata, some new and many ancient.

Caveats

Lest it be thought that I have come as John the Baptist: I have not, and would indeed like to avoid his fate. Let me therefore end with a list of limitations I see in the thematic analysis of scientific work.

1) While themata can have a strong grip on the scientist or the community, and can be the most interesting aspect of a given case, there exist important parts of the history of science and of current work where themata do not seem to enter prominently. In studying the case of the work of Enrico Fermi and his group, I found it no great help to think of it thematically.

2) Even if this were not true, I would not like it to be thought that the themata in a scientific work are its chief reality. Otherwise, work in the history of science would degenerate into descriptivism, and scientific findings would seem to be on a par with

the tales of the old men in the hills of Albania, to whom today's story is just about as good or as bad as yesterday's. There is in science evidently a sequence of refinements, a rise and fall, and occasionally the abandonment or introduction of themata. But also there undoubtedly has been on the whole a progressive change to a more inclusive, more powerful grasp on natural phenomena.

3) The hold of a thema on a scientist does not make him right. It can mislead. Nor, for that matter, does the grip of a thema make him necessarily wrong.

4) We need to know more about the origins of themata. It is rather clear to me that an approach like Peter Medawar's (*13*), stressing the connections between cognitive psychology and individual scientific work, is a proper place to start.

5) The thematic commitment of a scientist typically is remarkably long-lived. But it can change. Examples are Wilhelm Ostwald, who first turned against atomism and then reversed himself once more; Planck; Einstein; and a few others. Moreover, embracing a thema such as atomism in one field of physics occasionally has not prevented the embrace of the opposite thema by the same person for another field of physics. A case in point is Millikan's championship of the "atom" in electricity, even while he was struggling fiercely against the quantum of light. Poincaré was conservative and ether-bound when it came to relativity theory, but quite oppositely directed in quantum theory.

6) Themata are shared by members of a community, with minor variations among the individual scientists, in whom I see the primary repository of themata. But some themata have a career that can be conveniently understood in life-cycle terms. That is, they rise, atrophy, and fade away; explanatory devices such as macrocosmic-microcosmic correspondence, inherent principles, teleological drives, action at a distance, space-filling media, organismic interpretation, hidden mechanisms, and absolutes of space, time, and simultaneity—they all once ruled strongly in physics. The detailed study of the mechanism of such rise and decay is much needed.

7) There is always the danger of confusing thematic analysis with something else: with Jungian archetypes, with metaphysics, with paradigms and world views. (It might well be that the latter two contain elements of themata. But the differences are overwhelming. For example, thematic oppositions persist during "normal science," and themata persist through revolutionary periods. To a much larger degree than either paradigms or world views, thematic decisions seem to come more from the individual than from the social surrounding.) While the thematic analysis may be limited by the requirement of some firsthand experience with the scientific material (again, *Fingerspitzengefühl*), the rewards of doing more specific work on real cases seem to me far more evident than those to be had from engaging in comparisons among different historiographic schools, or in psychological or rational "reconstructions," or in the preparation of overarching, general treatises.

8) Finally, there is a need for self-awareness. The search for answers in the history of science is itself imbued with themata, just as is the search for a unified theory of elementary particles. One must be prepared for the critique of those who are afflicted, not with one's own themata, but with their antithemata, and one must be ready to run up against the limitations within which one necessarily works—as Einstein did in his frank way when he said, "Adhering to the continuum originates with me not in a prejudice, but arises out of the fact that I have been unable to think up anything organic to take its place" (*14*). His own work is of course testimony to the fact that one can turn such inherent limits of the scientific imagination into strengths, rather than merely deploring or neglecting them.

References and Notes

1. P. A. M. Dirac, in *Topics in the History of 20th-Century Physics: Proceedings of the International School of Physics "Enrico Fermi" at Varenna, 1972*, C. Weiner, Ed. (Academic Press, New York, in press).
2. F. E. Manuel, *A Portrait of Isaac Newton* (Harvard Univ. Press, Cambridge, Mass., 1968).
3. M. von Laue and M. Born, *Phys. Z.* **24**, 52 (1923).
4. Examples are given in G. Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein* (Harvard Univ. Press, Cambridge, Mass., 1973), particularly parts 1 and 2.
5. For examp "Recent progress in unified gauge theory of the weak, electromagnetic, and strong interactions," *Rev. Mod. Phys.* **46**, 255 (1974).
6. S. Weinberg, "Unified theories of elementary-particle interaction," *Sci. Am.* **231** (No. 1), 56 (1974).
7. F. J. Hasert *et al.*, *Phys. Lett.* **46B**, 121, 138 (1973).
8. Since not all themata appear in so many words, we would properly need a second scanning, proceeding in larger units. The fact that this paper appeared in a more popular journal rather than a professional archival publication helps our purpose; somehow, scientists are more likely to reveal their thematic assumptions when addressing general audiences.
9. The atom as thema does not have to refer to a natural physical object such as the discrete elementary entities, the gamma particle, the neutron, and the proton. It can be an element from which much more formalistic entities are constructed. For example, later (*6*, p. 58) Weinberg notes that the weak interactions, if they really have an intrinsic strength comparable to that of the electromagnetic interactions, "can provide additional corrections to isotopic spin symmetry." Theoretical entities, no less than the now more palpable nuclei or atoms or, for that matter, crystals, can be thought of as a sum or aggregate composed of various terms, for example, a core term and a number of correction terms.
10. As for the graviton, Weinberg remarks parenthetically that it "interacts too weakly with matter for it to have been observed yet, but there is no serious reason to doubt its existence." It is a splendid and daring dismissal of the weary Simplicio in Galileo's dialogues who, since 1632, has been exclaiming at just such a point: "What? So you have not made a hundred tests, or even one? And yet you so freely declare it to be certain?" [Galileo Galilei, *Dialogue Concerning the Two Chief World Systems*, S. Drake, translator (Univ. of California Press, Berkeley, 1953), p. 145].
11. "Families of elementary particles are believed to be a consequence of a symmetry principle known as isotopic spin symmetry, analogous to the rotational symmetry that produced the family of quantum states within the hydrogen atom. The grouping of these families of elementary particles into superfamilies (octets, decimets, and so on) was proposed independently in the early 1960's by Murray Gell-Mann and Yuval Ne'eman" (*6*, p. 55).
12. Symmetry principles provide "information about the laws of nature on the deepest possible level," symmetries that are "broken," chiral symmetries, and so on (*6*, pp. 55–56).
13. P. B. Medawar, *Induction and Intuition* (American Philosophical Society, Philadelphia, 1969).
14. A. Einstein, in *Albert Einstein, Philosopher-Scientist*, P. Schilpp, Ed. (Harper, New York, 1959), vol. 2, p. 686.