(and coincidentally) provide all the conditions required, in an artificial mixture of cells, for sorting out to occur, and for its morphological result to imitate the anatomy normally produced by mass tissue movements. The foundation for such a thermodynamic analysis, like much of the empirical groundwork upon which it rests, was laid by Holtfreter (*32*), whose treatment of the subject has been discussed separately (*33*).

Our recognition of the organization which is everywhere present in the living world has played a prominent role in the development of our biological concepts. It is not surprising that apparent meaningfulness or complexity in the design and functioning of organisms should have led us to assign corresponding attributes to the mechanisms governing the functioning and the design. Yet, as knowledge has grown, complex explanations have had a way of succumbing to relatively simpler ones. Thus, overt vitalism is gone from the scene. Organic molecules, it later developed, could be synthesized by the chemist after all. Proteins were not so simple as to preclude the possibility of their functioning as enzymes; nor was DNA, at a later stage, too simple to provide the vast stores of "information" for which the proteins, now recognized to be complex, might have seemed a more fitting receptacle.

While the *adaptedness* brought about through evolution appears complex, the *adaptiveness* which makes evolution possible is born of simplicity. The entire genetic code (and more) is expressible with an alphabet containing only four elements. It would appear that a not inconsiderable amount of the "information" required to produce, through morphogenetic movement, the anatomy of a body part may be expressed in a code whose sole element is quantity: more versus less. There is, I think, reason to expect that as more realms of biological specificity yield to analysis, their most impressive feature may be the simplicity of the terms in which specificity—information, if you will—can be expressed (*34*).

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Revolutions in Physics and Crises in Mathematics

There is no simple, single formula for the course of revolutions in science.

Salomon Bochner

In this article I deal with two topics which, although separable, are closely connected with each other. The first and larger part of the article is concerned with the conception of a revolution in physics, as recently blue-printed in a provocative book by Thomas Kuhn (*1*). I make observations which are seemingly in conflict with those of Kuhn, but I really intend to amplify and qualify some of Kuhn's theses rather than to dissent from them, and my approach is somewhat different anyhow. After that, I make some observations on revolutions in physics as far as the underlying mathematics is concerned. And, finally, I make some remarks on so-called "foundation crises" in mathematics, which may be viewed as a kind of revolution, and especially on a major crisis of this kind which is presumed to have taken place in the 5th century B.C.

Kuhn, in his investigations into the nature of revolutions in science, analyzes both the inward ontological and epistemological nature of such revolutions and the psychological and behaviorist attitudes, resistances, and responses of practitioners of science, before, during, and after a revolution. Kuhn finds that revolutions in science are mostly internal revolutions, brought about by some scientists and then forced by the initiators on the scientific community at large. There is even an implied suggestion that, in the beginning, a revolutionary innovation may be both desired and resisted by the same group of scientists, ambivalently. Kuhn makes a point of emphasizing that most scientists all the time, and all scientists most of the time, prefer peace to revolution, normalcy to anomaly, and the preservation of their "paradigms" to changes of paradigms, a "paradigm," according to Kuhn, being more or less a sum of "universally recognized achievements that for a time provide model problems and solutions to a community of practitioners" (*1*, p. x.)

This finding is indeed meaningful, and as already noted by Gillispie (*2*), it is one that can be easily accepted. For my part, I found nothing singularly disturbing in the realization that among scientists, as in other groups of human beings, the revolutionaries of today are likely to be the conservatives of tomorrow; that paradigms are not readily abandoned or changed unless anomalies make it imperative; and that there may be diehards who will not give in even then. But if one *is* surprised and disturbed to find that resistance to innovation is widespread and even dominant among "professors" who are expected to be professionally pledged and conditioned to emphatically seek the truth and nothing but the truth, I think that there is no clear reason for singling out scientists from among scholars in general. Kuhn's diagnosis of innate conservatism does attach a certain stigma, and it is restrictive to the entire study to stigmatize scientists for something which philosophers and humanists also practice.

Perhaps we can see evidence of the humanists' concern to preserve a paradigm in a well-attested event (*3*) which occurred in the Neoplatonic school at Athens during the last 50 or 60 years before its dissolution by the Emperor Justinian in A.D. 529. At the time, leading circles in the school were opposed to the increase in Aristotelian features in the "official" world picture, the result of certain influences from within. A leading exponent of Aristotelian ideas was Marinus, born in Neapolis (the Hebrew Shechem) in Samaria, who eventually became head of the school, succeeding the much-adored "divine" Proclus (A.D. 411–484). Marinus, when still in the junior position of tutor, was in charge of Isidorus, a student. One day Marinus showed Isidorus a commentary on the platonic dialogue "Philebus," which he had just composed. But, on the authority of Damascius, biographer of Isidorus, the latter prevailed upon Marinus to destroy the commentary, on the strange grounds that their great master Proclus, then head of the school, had already composed a commentary on the "Philebus" to end all such commentaries. Present-day scholarship maintains convincingly that the true motivation for this request was apprehension lest the work of Marinus inevitably show a bias against the Neoplatonic "paradigm" even if Marinus made an effort to keep it out.

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Max Planck

One of Thomas Kuhn's star witnesses is none other than the physicist Max Planck. In *Scientific Autobiography*, written in 1937 (*4*), Planck remarked that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." This harsh statement has the ring of authenticity, but it so happens that it hardly applies to Planck's own discovery. It is true that Planck's discovery was not hailed on the instant as the great breakthrough that it was, but it is equally true that Planck did not have to wait for anybody to die before a considerable measure of recognition was meted out to him.

Although Planck's first papers on quanta began to appear only in the latter half of 1900, as early as 1911 renowned physicists of the day convened at a Solvay Congress in Brussels with an agenda devoted entirely to the new theory, and with Planck as the honored speaker (*5*). Before that, in an encyclopedic article (*6*) dated May 1909 and entitled "Theorie der Strahlung," the radiation specialist W. Wien, although unsympathetic to Planck's theory, gave it exhaustive coverage. On the other hand, Planck himself, as late as 1922, in his Nobel address, almost tried to play down the originality of his achievement by pointing out how his intense preoccupation with the then relatively new Hertz vibrator (or "resonator") was the catalytic setting that eventually brought forth the quantum hypothesis (*7*). In fact, in the list of references to the written version of the address, Planck cites the paper of Heinrich Hertz in *Annalen der Physik* (*8*), in which Hertz discusses the vibrator which he had constructed for purposes of testing the Maxwell theory. With this vibrator Planck, in 1900, by a kind of "ideal" construction in the black body, devised his quantization. It is regrettable that, in after years, Planck and his biographers (especially von Laue) rarely restated this illuminating biographical fact, if they ever mentioned it again.

In general, I would say that between 1900 and 1925, in the quantum theory "revolution" which was brought about by Planck himself and then by Niels Bohr and others, the changeover from one paradigm to another was peaceful and evolutionary, without any of the characteristics of a revolution by *bouleversement,* and without manifestations of a forced normalization of an anomaly, as Kuhn envisages it. One might retort that, although this particular transition was a peaceful one, yet the emergence of the theory of relativity within the same period, 1900 to 1925, and in fact within the narrower period 1905 to about 1918, did indeed conform to the Kuhn pattern, and that the second half of the quantum theory revolution, which began soon after 1925 and in which Heisenberg, Schrödinger, Dirac, and others were the protagonists, did also. To this I would reply that what gave the emergence of relativity the character and status of a revolution was not its effect on physics proper but its effect on the notions of space and time, which, long before that, had become objects of paramount importance to philosophy in general. I have been trying to show in recent articles (*9*) that space and time did not become paramount in general philosophy until after the Renaissance, whereas in classical Greece, for instance, in spite of a strong trait of spatiality in the general imagery of rational thinking, space and time, as specific notions, were notions of physics and of physics only. If the concepts of space and time had not attained the preeminence in general philosophy which they had through developments that had occurred since 1600, relativity would have been much less of a revolution than it was.

Furthermore, the sudden outburst of interest in quantum theory and in Planck's constant came after the proclamation of the uncertainty principle by Heisenberg, in 1925, and here again the interest arose from the fact that Heisenberg, in his "popularization" of his principle, emphasized the involvement of the notion of ordinary space. The overall fact that the uncertainty principle applies to pairs of conjugate operators in general, if stated without emphasis on particular pairs which correspond to ordinary Newtonian coordinates of position and momentum, would hardly have caused philosophers the malaise they felt when the emphasis was placed on these coordinates.

Also, while the cosmological revolution in the 16th century did indeed replace one paradigm with another—namely, the geocentric with the heliocentric—it cannot be said that the theory of relativity replaced Newtonian space with other spaces, in the sense of making Newtonian space obsolete or antiquarian. Planetary and particle mechanics, vast stretches of "phenomenological" mechanics of continua, and much of statistical mechanics, of thermodynamics, and even of electrodynamics continues to be Newtonian; any serious attempt to de-Newtonize them would make most of mechanics, much of physics, and virtually all of engineering unrealistically complicated and would be widely resisted. And even within the heart of physics, which did indeed become genuinely relativistic, there is no single paradigm in control, such as a Kuhn revolution by *bouleversement* would have terminated in. In fact, the Lorentzian space of quantum field theory and the substratum space of most cosmological models now in vogue are totally contradictory.

Finally, I wish to point out that, to many physicists, the theories of relativity, whatever their éclat, were terminal phases of the era of Newton, Lagrange, Hamilton, and Maxwell rather than initial phases of a new era. But Planck's original quantum hypothesis, even if its advent was rather peaceful and even if its antecedents in 19th-century radiation theory were comparatively unspectacular, was apparently the prologue to a tremendous epic-still-to-come, of which only the introductory scenes have been playing thus far.

Kuhn's notion of a scientific revolution may subsume too many possibilities under a single formula. The formula apparently is meant to apply to the emergence of modern science in the 17th and 18th centuries, viewed as one giant revolution, in spite of its size and in spite of the fact it was much more the direct emergence of something entirely novel than the transformation, by revolution, of something old into something new. But the formula is seemingly also meant to apply to many particular events, which occurred in succession but are viewed separately, such as the many turns and twists and even vagaries in the presystematic phases of electricity and chemistry in the 17th and 18th centuries, especially the 18th century. And finally, as has already been noted somewhat apprehensively by Gillispie (*2*), the formula might end up by being applied, through a circular mode of identification, to any kind of changeover which has a visible and recognizable trait of originality and creativity associated with it. Now, the question of what constitutes originality and creativity in *Homo sapiens* is probably one of the most difficult problems in philosophy and philosophical sociology, and not much would be gained by reducing the problem of what is a revolution of knowledge to the problem of what constitutes creativity in man. Also, it is probably feasible to distinguish between latent and active phases within creativity. This would lead to a corresponding distinction, during nonrevolutionary periods in science, between genuine "normalcy" and "latency" of anomaly, and the evaluation of this distinction, within Kuhn's schema, might become very troublesome indeed. Furthermore, in physics there seem to be periods of concentrated creativity which are made up of rapid successions of many small but sharply defined discontinuities of achievement, and to such periods Kuhn's formula of a "normalization of an anomaly" can be applied only with difficulty.

Scheme of Mathematics Underlying Physics

A novel situation arises, in the case of physics, if we turn our attention away from physics proper and toward the scheme of mathematics that underlies it. This mathematics seems to have paradigms of its own, and they are more inward ones. These inward, structural paradigms seem to behave differently from the outward, purely physical ones, sometimes even disturbingly so. Thus, neither electricity nor magnetism had any mathematical paradigm at all until, toward the end of the 18th century, Cavendish and Coulomb formulated the so-called Coulomb law, and until, at the beginning of the 19th century, Poisson initiated magnetostatics and electrostatics by introducing a mathematical theory of potentials into the context. In this sense, in spite of Lavoisier, one must say that chemistry began in earnest only with the laws of Dalton, Avogadro, and Dulong and Petit all in the beginning of the 19th century. I do not mean to suggest that there were no disciplines of electricity, magnetism, and chemistry before the 19th century. But I do mean to suggest that, to somebody in the field of mathematics, the cataloging of many separate revolutions which these disciplines are supposed to have gone through in the 17th and 18th centuries is bewildering and unconvincing.

A similar observation could even be made about the course of "classical" Greek physics in its entirety. The developments from Thales to Aristotle are frequently presented as a "motivated" succession of revolutions in which an emergent would-be physical or cosmological system was knowingly and militantly put forward to supersede an earlier system. But the fact is that Archimedes, who was the only mathematical physicist Greece ever produced, seemingly refused to be involved in these crazy-quilt developments, and to his sober mind they probably appeared to be "irrational" and unmotivated.

On the other hand, in the case of mechanics, physicists and other scientists are wont to view the 17th and 18th centuries as one unit of development. And yet, mathematically (*10*), Newton's *Principia* has the appearance and many of the attributes of the works of Archimedes and Apollonius, whereas Lagrange's *Mécanique Analytique* (1788) is not radically unlike textbooks of today, 20th-century modernisms notwithstanding. Also, the mathematicophysical subject matter of the various divisions of mechanics which took shape in the 18th century was not at all a mere explication of what had already been presented in the *Principia*, implicitly if not expressly, even though the effect of Newton's treatise on later developments was an overwhelming one. In fact, by "juggling" mathematical paradigms one could make out a case for the assertion that there was a much greater distance between the mechanics of Euler and Lagrange and the mechanics of Newton than there was, in the 19th century, between the electrodynamics of Maxwell and Hertz and the hydrodynamics of, say, Helmholtz.

Apparently the mathematization of a science affects the role and nature of revolutions that may and do occur in it. And since, on the other hand, most of science is tending toward mathematization, even determinedly so, one should guard against generalizing from the shape of premathematical revolutions to the shape of revolutions in general.

Mathematics itself also has its revolutions, and developments in the 20th century have led to the singling out among them of revolutions of a particular kind, which are termed "foundation crises." It has been asserted, with emphasis and even with a dash of sensationalism, that the classical Greek mathematics which is known from the works of Euclid, Archimedes, and Apollonius went through such a crisis in an early Pythagorean phase in the 5th century B.C. The cogency of this assertion may be questioned, and the assertion has indeed been contested. But the clamor for a retroactive crisis in antiquity has been such that temperate counterassertions have not been able to mute it. An "anomaly" in Greek mathematics did indeed emerge; it was the discovery that the square root of 2 is irrational, or rather that, in a square, the diagonal is incommensurable with the side (*11*). Greek mathematics did certainly react to this discovery, with attentiveness and resourcefulness as is evidenced by the reference to the problem in the platonic dialogue "Theaetatus," and by the erection of a Greek theory of incommensurables, apparently attributable to Eudoxus, which is the subject matter of the 5th book of Euclid. However there is no indication in Greek doxography of a "crisis" or of a "mathematical scandal," except perhaps for the late-Pythagorean tradition that Hippasus of Metapontum, an unruly, early member of the Pythagorean sect, was violating rules of the sect by divulging to outsiders details about research-in-progress on incommensurables and other problems, and that he suffered divine retribution for his indiscretions (*12*). One might perhaps also adduce the fact that, in Archimedes's work on "Sphere and Cylinder I," in the prefatory letter to Dositheus, the puzzling assertion that the mathematical procedure of Eudoxus is "most irrefragable" (*13*) indicates that even at that time there were some "philosophers" who were not satisfied with the manner in which Eudoxus resolved the crisis of incommensurables. Against these very slender items of support one has to note that there is no allusion to a mathematical crisis or "scandal" in any of the passages in Aristotle from which, with due caution, most of what is known about Pythagorean mathematics and principles of science has to be abstracted.

A systematic theorizing about foundation crises in mathematics was begun about 50 years ago in response to the challenging discovery, around 1900, that there are paradoxical situations in George Cantor's "naive" set theory and hence in mathematics as such. In fact, the first foundation crisis was identified, in substance rather than in name, in 20th-century mathematics itself, and past crises were then uncovered in the wake of this one. The "Greek crisis" theory was received very attentively and sympathetically all around, perhaps in remembrance of Zeno's puzzles, which to some philosophers are inexhaustibly provocative. But I should point out that the authors of a recent book (*14*) are trying to arouse interest in a third foundation crisis, and the "anomaly" which underlies this crisis is the one whose normalization consisted in the "rigorization" of analysis in the 19th century by Cauchy, Weierstrass, Cantor, and others. This last alleged crisis is the one least deserving of the name. Perhaps it was "anomalous" for the infinitesimal calculus to pile up achievement upon achievement in the 17th and 18th centuries, without being frustrated by inadequacies of mathematical rigor, and to become introspective as to its rigor only afterwards; if so, this was the healthiest and the most wonderful "anomaly" that could have occurred. Newton, the Bernoullis, Euler, d'Alembert, Lagrange, Laplace, and others made advances beyond anything one might have asked for.

To say that their achievements landed mathematics in a "crisis" is incongruous, unless one is prepared to aver that Thales and Pythagoras plunged rational thinking into a crisis-in-perpetuity, inasmuch as they introduced mathematics into rational thinking inseparably, and (as Plato and Aristotle felt) inasmuch as there is no prospect of constructing a logico-ontological foundation of mathematics that will be absolutely and unqualifiedly satisfactory to all, forever.

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