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## On Kuhn's *The Structure of Scientific Revolutions*

### 1. Introduction

Kuhn's influential book, *The Structure of Scientific Revolutions*,<sup>1</sup> is often viewed as a revolt against empiricist philosophy of science. However, Friedman has reminded us lately<sup>2</sup> that the book was commissioned by logical positivists, who were delighted with the result. In fact, the book was part of the International Encyclopedia of United Science initiated by members of the Vienna Circle, whose first volumes were published in 1938.<sup>3</sup> The project aimed at providing a systematic positivist perspective on all the sciences, from logic and mathematics through linguistics and on to psychology and sociology.

The publication of *Structure* as volume 12 of the encyclopedia was greeted enthusiastically by the editor, Carnap, as can be learned from the letters he wrote to Kuhn. There are several reasons for this reaction. First, as noted by Friedman, there is a resemblance between Kuhn's notion of changing paradigms and Carnap's philosophical ideas. Logical empiricism is "logical" because of its central tenet that scientific knowledge is the organization of facts within a conceptual structure; the existence of an appropriate conceptual structure is a precondition for the very possibility of scientific inquiry. Unlike Kant, however, the logical empiricists believed that the conceptual

<sup>1</sup> University of Chicago Press, 2nd edition with postscript, 1970. Subsequently I will refer to it as *Structure*. The paper is adapted from the addendum to the second Hebrew edition of the book, translated and edited by Yehuda Melzer (Books in the Attic, 2005). The research leading to this paper is supported by the Israel Science Foundation, grant 879/02.

<sup>2</sup> Michael Friedman, "Kuhn and Logical Empiricism," in Thomas Nickles (ed.), *Thomas Kuhn* (Cambridge University Press, 2003).

<sup>3</sup> The separate books were eventually collected in two thick volumes under the title *Foundations of the Unity of Science* (University of Chicago Press, 1969).

system, what Carnap called the “linguistic framework,” is largely a matter of convention; different and perhaps even mutually incompatible formal frameworks can describe the same empirical findings. What is more, a change of linguistic frameworks is precisely the process that occurs when a scientific revolution takes place. In this regard, Carnap’s approach is somewhat similar to Kuhn’s concept of incommensurable paradigms, a similarity that he himself noted in his later writings.

Another aspect in which Kuhn retains the positivist outlook is his view of science as chiefly a conceptual system. Even though he emphasizes that paradigms do not always operate in the framework of precise definitions and clearly distinguished laws, he views a scientific revolution as being first and foremost a conceptual change, in which one way of looking and thinking is replaced by another, and one text is supplanted by another. For Kuhn a dramatic invention of a tool or a technique is not considered in itself a revolution. Take for example X-ray photography: in the first thirty years of the twentieth century five Nobel prizes in physics went to experimental work based on X-rays. Many more crucial steps in all the natural sciences followed. Kuhn considers (section 6) whether Röntgen’s discovery should be called “revolutionary” because of its limited conceptual innovation. His emphasis on the conceptual aspects of science neglects its other goal, namely, to control and manipulate nature in the service of various human interests. The picture of experimental work as subservient to “theory” has been corrected to a large extent by later historiography.

However, there are profound differences between Kuhn and the positivists, and chief among them is the role that history plays in their respective approaches. The positivists were not deeply interested in the history of science, they did not believe that the sweat and tears through which science plods ahead are germane for understanding and analyzing philosophical questions. Philosophy, according to Carnap, is the logical analysis of the language of science. Kuhn, by contrast, has a quasi-Hegelian view that equates philosophy of science with its history. Although *Structure* contains philosophical ideas, and although it is often presented as a book in philosophy, it advances almost no philosophical arguments. Kuhn’s line of thought, expressed at the outset, is that (his version of) the history of science is *ipso facto* the argument supporting his philosophical conclusions.

However, Kuhn’s historical outlook is colored by a “revolutionary zeal,” that is, an urge to see revolutions as the engines that pull the history of

science (and probably the history of ideas in general). In a brilliant paper Mara Beller traced the roots of this passion to the way the story of quantum mechanics was told by the Copenhagen school.<sup>4</sup> I think that Mara, while disagreeing with Kuhn's historiographical outlook, did share his conception that philosophy of science can be somehow inferred from its historical unfolding. I do not share this view. For example, I think that in philosophy of science "Copenhagen" should stand for the most charitable case one can *presently* make for this view. The philosophical task is not to distribute praise or blame, but rather weigh this interpretation against its (not the least less problematic) alternatives.

In memory of Mara, a friend and a colleague, I chose to focus on the history of science, not its philosophy. I hope this paper complements her analysis of Kuhn's revolutionism. My aim is to demonstrate how such a view can blind one to important developments in the history of science.

## 2. Paradigms: Classical Mechanics in the Twentieth Century

Since the publication of *Structure*, the term "paradigm" has entered everyday language to describe world-embracing theories, but also to inflate almost-negligible ideas. Kuhn's use of the term was famously vague. In the Postscript added to the second edition he acknowledges the "cogent criticism" of Margaret Masterman, who counted at least twenty-two different ways in which he used the term. For my aim here no precise definition is required, only one single example of a paradigm, perhaps the most important offered by *Structure*: the classical mechanics that developed on the basis of Newton's laws of motion.

Readers of *Structure* are apt to gain the impression that Newtonian mechanics is a dead paradigm, of interest only to historians. Much is said about the replacement of the Newtonian paradigm by relativity (special and general) and quantum theory, implying that classical mechanics has faded away and subsided. However, a fairly superficial glance will disclose that thousands of scientific articles are written every year on topics directly associated with Newton's laws.

<sup>4</sup> Mara Beller, "Criticism and Revolutions," *Science in Context* 10 (1997); also chapter 14 of her *Quantum Dialogue: The Making of a Revolution* (University of Chicago Press, 1999).

There is no mystery here. Newton's laws are the underpinning of a huge number of engineering problems: static computations (structural engineering, strength of materials), the dynamics of rigid bodies (mechanical engineering), fluid dynamics (aeronautical engineering, marine engineering, motor car engineering, weather forecasting). Indeed, the latter is the most notable example. Fluid dynamics—the study of the motion of liquids and gases—has become over the last hundred years a very large branch of applied mathematics, physics, and engineering.<sup>5</sup>

But what about Newtonian *theory*? As we have seen, Kuhn is interested in the conceptual scaffolding on which science is built, not in technological applications. From the picture painted by *Structure* we would expect that interest in Newton's theory would have declined after alternate paradigms emerged; in particular, problems that were part of Newtonian "normal science" would have been set aside. Here too the situation is quite different: classical mechanics continued to be a research topic throughout the twentieth century and beyond. The investigators included some of the leading lights of mathematical physics, who achieved new and important results, including some of the most important mathematical theorems of the twentieth century. What is more, as we shall see, the Newtonian paradigm gained a new lease of life in the 1980s.

By way of illustration consider one facet of mechanics, which deals with the dynamics of point masses moving under the influence of the forces that act between them according to Newton's laws of motion. The nineteenth century left two major unsolved problems in this domain: stability and ergodicity.

The stability problem involves the characterization of the stable and unstable states of systems of particles. It has been part of mechanics at least since Laplace (if not Newton himself). The state of the system is stable if a slight perturbation of the particles' motion does not produce a major change in their future trajectories. The state is unstable if there is some small perturbation that can cause the particles to switch to paths that are very different than their original trajectories. Of course the terms "small perturbation" and "similar trajectories" have precise mathematical definitions. An important case of instability has to do with the distances

<sup>5</sup> The basic equation of fluid motion is named for Navier and Stokes, the physicists who gave it its final form in the first half of the 19th century. The derivation of the equation is based on Newton's Second Law.

among the particles. A particular state of a system may determine future trajectories such that the distances among the particles remain bounded. But a small perturbation of the state may cause the trajectories to diverge and the system to disintegrate. This case is abstracted from the "concrete" question concerning the stability of the solar system, which is as old as mechanics.

The stability problem was first studied with regard to the motion of bodies under the influence of their mutual gravitational attraction and labeled according to the number of masses in the system: the three-body problem, the four-body problem, and so on. Newton himself solved the two-body problem. The three-body problem posed a much greater challenge; the attempts to describe the particles' trajectories in various conditions led Henri Poincaré to discover states of unstable motion. Some of this work is reported in his monumental *New Methods of Celestial Mechanics*.<sup>6</sup> The general mathematical methods he developed, along with the parallel work by the Russian mathematician Lyapunov,<sup>7</sup> serves as the foundation of stability theory to the present, with much of the terminology kept intact. Perhaps the most radical example of instability in Newtonian gravitational theory was advanced as a conjecture by the French mathematician Paul Painlevé: there are initial states of a many-body system which lead one of the bodies to escape to an infinite distance within a finite length of time.<sup>8</sup>

The conceptual implications of these theoretical developments have not escaped attention. In Newtonian physics, the initial conditions of a system uniquely determine its future state (singularities notwithstanding); despite this determinism, and because of various reasons, human beings possess only a limited capacity to forecast the future. One obvious reason, noted by Laplace, is the sheer number of particles which prevents any precise knowledge of their initial state, and any calculation of their future state.

<sup>6</sup> English translation edited and introduced by Daniel L. Goroff (American Institute of Physics, 1993). The three volumes of the French original appeared in 1892–99. The famous story of the discovery of the unstable trajectories is told in the introduction to the English translation.

<sup>7</sup> A. M. Lyapunov, *The General Problem of the Stability of Motion*, tr. and ed. A. T. Fuller (Taylor & Francis, 1992; first published in Russian in 1892).

<sup>8</sup> That is, the particle simply disappears from space, a situation which is called "singularity without collision"; see Paul Painlevé, *Leçons sur la théorie analytique des équations différentielles* (Paris, 1897). The Painlevé conjecture calls the determinism of Newtonian physics into question, although it is not clear whether Painlevé was aware of this; see John Earman, *A Premier on Determinism* (Reidel, 1986)

Instability adds another cause that hampers prediction, even in systems with a small number of particles. Poincaré noted<sup>9</sup> that “a very slight cause, which escapes us, determines a considerable effect which we cannot help seeing, and then we say this effect is due to chance.” Poincaré’s contemporary, Jacques Hadamard, discovered and studied unstable systems independently.<sup>10</sup> Hadamard explicitly mentions that a small error in our knowledge of the initial conditions makes the long-term behavior of the system unpredictable. Émile Picard added that instability is the rule for dynamic systems, and stability the exception.<sup>11</sup> Here we should note that in the second half of the nineteenth century, skepticism about the limits of knowledge was common to many European intellectuals in many different fields. For example, an influential lecture in 1872 about the limits of knowledge by the German physiologist Emil du Bois-Reymond launched a broad and enduring debate, and his credo, *ignoramus et ignorabimus* became a common slogan.<sup>12</sup> As we have seen, not only was this skepticism compatible with classical mechanics, it was supported by it.

The second problem remaining from the legacy of classical particle mechanics is the ergodic problem, which was introduced by Boltzmann in the 1870s. His idea was to define the expectation of observables in statistical mechanics as the limit of their time averages. He set out to prove two results: (a) the Newtonian dynamics of the molecules in a gas guarantees the convergence of the time averages, and (b) the limit of the time averages equals the phase-space averages (which are used in statistical mechanics). Boltzmann’s hope was that these two steps will complete the reduction of thermodynamics to classical mechanics. To accomplish these steps Boltzmann conjectured that multipartite Newtonian systems possess a dynamical property, called ergodicity, which he hoped would suffice to generate the desired behavior. His formulation was plagued with mathematical inconsistencies, and consequently the definition of ergodicity

<sup>9</sup> In his last philosophical book, *Science and Method*, tr. Francis Maitland (Dover, 1952; first published in French in 1914).

<sup>10</sup> Notably, in 1898, unstable free motion in hyperbolic space—an important mathematical example throughout the 20th century; see Jacques Hadamard, “Les surfaces a courbures opposés et leurs lignes géodesiques,” in *Oeuvres de Jacques Hadamard* (Paris, 1968).

<sup>11</sup> Émile Picard, *Traité d’analyse*, vol. 3 (Paris, 1901).

<sup>12</sup> Emil du Bois-Reymond, *Über die Grenzen des Naturerkennens* (Leipzig, 1873).

changed several times before settling on its present-day form.<sup>13</sup> Two problems remained: first, to prove that realistic physical systems have the property of ergodicity, and second, to show that ergodicity indeed implies that the limit of time averages equals phase-space averages.

As we shall see later, these developments were extremely important for twentieth-century science (which discovered deep connections between instability and ergodicity). But Kuhn never even hints at them. The last theoretical contribution to Newtonian particle mechanics mentioned in *Structure*, and even then only in passing, is the work by Hamilton and Jacobi in the 1830s and 1840s. *Structure* never refers to any of the important developments in mechanics during the seventy years from then until the emergence of special relativity.

As stated, the stability problem and the ergodic problem were studied intensively throughout the twentieth century by scientists who were perfectly aware of the failures of Newtonian physics. Significant breakthroughs were made every decade or two. Here is an extremely sketchy and incomplete list: an analytical solution of the three-body problem (Sandman in 1913; the solution was completed by Siegel in the 1940s);<sup>14</sup> the convergence of long-term averages in ergodic systems (for different notions of convergence: Birkhoff, von Neumann, and Wiener in the 1930s);<sup>15</sup> the link between instability and ergodicity (Morse in the 1930s, and for hyperbolic spaces by Hedlund and Hopf);<sup>16</sup> the characterization of general conditions under which Hamiltonian systems are stable and non-ergodic (Kolmogorov in the 1950s, Arnold and

<sup>13</sup> See, e.g., Jan von Plato, *Creating Modern Probability* (Cambridge University Press, 1994).

<sup>14</sup> K. F. Sandman, "Memoire sur le problème des trois corps," *Acta Mathematica* 36 (1913); C. L. Siegel and J. K. Moser, *Lectures on Celestial Mechanics*, tr. C. I. Kalme (Berlin and New York, 1971; rev. and enl. ed. of Siegel's *Vorlesungen über Himmelsmechanik*, 1956).

<sup>15</sup> G. D. Birkhoff, "A Proof of the Ergodic Theorem," *Proceedings of the National Academy of Sciences* 17 (1931); John von Neumann, "Proof of the Quasi Ergodic Hypothesis," *Proceedings of the National Academy of Sciences* 18 (1932); Norbert Wiener, "The Ergodic Theorem," *Duke Mathematical Journal* 5(1) (1939).

<sup>16</sup> Marston Morse, "Does Instability Imply Transitivity?" *Proceedings of the National Academy of Sciences* 20(1) (1934); Gustav A. Hedlund, "On the Metrical Transitivity of the Geodesics on Closed Surfaces of Constant Negative Curvature," *Proceedings of the National Academy of Sciences* 20 (1934); E. Hopf, "Fuchsian Groups and Ergodic Theory," *Transactions of the American Mathematical Society* 39(2) (1936).

Moser in the 1960s; the combined result of their work is known as the KAM theorem);<sup>17</sup> the proof of the ergodic hypothesis for an ideal gas composed of tiny billiard balls;<sup>18</sup> the characterization of sets of unstable states in general dynamical systems.<sup>19</sup> Finally, we should mention two demonstrations of the Painlevé conjecture in the four- and five-body problems.<sup>20</sup>

Some of these results rank among the most notable mathematical theorems of the twentieth century, and their authors among the greatest figures in mathematics and mathematical physics. The high concentration of mathematicians among them is part of the tradition of classical mechanics since the seventeenth century. Nevertheless, the earlier developments mentioned above also appeared in the textbooks of mechanics used by the students of theoretical physics, for example, the dominant English textbook of theoretical mechanics in the first half of the twentieth century by Whittaker, which went through four editions and many printings.<sup>21</sup> In the Soviet Union, there was active research in classical mechanics throughout the century. In the United States it was mainly mathematicians, following in the footsteps of Birkhoff, who were aware of this work.<sup>22</sup> American physicists were not:

<sup>17</sup> A. N. Kolmogorov, "On the Conservation of Conditionally Periodic Motions under Small Perturbations of the Hamiltonian," *Proceedings of the USSR Academy of Sciences* 98 (1954, in Russian), English translation in *Lecture Notes in Physics* 93 (1979); V. I. Arnold, "Proof of a Theorem by A. N. Kolmogorov on the Invariance of Quasi-Periodic Motions under Small Perturbations of the Hamiltonian," *Russian Mathematical Surveys* 18(5) (1963); J. K. Moser, "On Invariant Curves of Area-Preserving Mappings of an Annulus," *Nachrichten der Akademie der Wissenschaften in Göttingen* (1962); see also J. K. Moser, *Stable and Random Motion in Dynamical Systems* (Princeton University Press, 1973).

<sup>18</sup> Y. G. Sinai, "Dynamical Systems with Elastic Reflections," *Russian Mathematical Surveys* 25 (1970).

<sup>19</sup> S. Smale, "Differentiable Dynamical Systems," *Bulletin of the American Mathematical Society* 73 (1967).

<sup>20</sup> J. N. Mather and R. McGehee, "Solutions of the Co-Linear Four-Body Problem which become Unbounded in a Finite Time," in J. Moser (ed.), *Dynamical Systems, Theory and Applications* (Berlin and New York, 1975); J. L. Gerver, "A Possible Model for a Singularity without Collisions in the Five-Body Problem," *Journal of Differential Equations* 52 (1984).

<sup>21</sup> E. T. Whittaker, *A Treatise on the Analytical Dynamics of Particles and Rigid Bodies* (Cambridge University Press, 1904; 4th ed., Cambridge, 1937). On several occasions Kuhn mentions another book of his, on the history of the theories of aether and electricity.

perturbation theory and the stability of dynamical systems are not mentioned in the leading American textbook of classical mechanics of the second half of the twentieth century. The textbook by Goldstein appeared in three editions (and several printings) and only the most recent ones, from 1980, include new chapters on the subject.<sup>23</sup> The reason for this sudden reversal will soon become clear.

The educated lay public was totally unaware of these discoveries, just as it is normally unaware of scientific advances. But in the late 1970s there was a significant change in the popular perception. In a brilliant and perhaps not quite intended exercise in public relations, stability theory, by that time at least eighty years of age, was reborn as “chaos theory” and proclaimed to be a new paradigm. (Around the same time the name “fractals” was given to a certain type of sets, well known by other names in mathematics and mathematical physics for more than a century.) Unstable dynamical systems and the problems they raise for prediction became the hallmark of the postmodern age, especially the never ending tale about the butterfly whose flapping wings produce a hurricane.

What can we learn from this story about paradigms? First of all, that they do not necessarily die so easily; rather, they continue their career alongside the theories and practices that supplanted them. For Kuhn, classical mechanics faded and died sometime during the nineteenth century; he finds it inconceivable that scientists might be engrossed in a theory that, to the best of their knowledge, is manifestly false. This is one point where Kuhn's “revolutionary zeal” leads to a distortion of history.

Second, the relationship between scientists and scientific theories cannot really be expressed by the dichotomy “acceptance/rejection,” the situation is much more complex. The scientists mentioned above include several scholars who made simultaneous contributions to competing paradigms without batting an eyelid (we shall see more examples of this below). The drive to engage with a particular problem is too complicated to be described merely as the practice of “normal science.”

Third, we learn something about Kuhn's unintended influence on the public's image of science. The idea that science moves by revolutionary

<sup>22</sup> G. D. Birkhoff, *Dynamical Systems* (New York, 1927; rev. ed., 1966), is probably the most important text on classical dynamics after Poincaré.

<sup>23</sup> Herbert Goldstein, *Classical Mechanics* (Cambridge, Mass., 1950); and see, e.g., 3rd ed., by Goldstein, Charles Poole, John Safko (San Francisco, 2002).

leaps, which are intimately related with general social developments, is seductive; so much so, that some find a paradigm change wherever they turn. The theory of dynamical systems provides a good example of this. There is no doubt that in the 1970s and 1980s there were some real advances in this area.<sup>24</sup> In addition, the increasing power of computers permitted impressive simulations of various dynamical systems. The use of stability theory was also extended to dynamical systems in chemistry and biology (a move that had begun earlier). Ultimately, all these contributions are an organic continuation of the long history that I have briefly and incompletely described here. Even though the facts are well documented some still view chaos theory as a new paradigm of the postmodern age.

Such statements resound in various academic disciplines, especially the social sciences and humanities. This misconception stems, in part, from developments in the history and sociology of science since the publication of *Structure*. I am referring to frequent unproven linkage made between factors such as Zeitgeist, social status, political views and the cognitive content of science. In particular, I am thinking about the historiographical error, which has become a cliché, that only as a result of the First World War did scientists come to have doubts about the predictive power of science and the possibility of controlling Nature. But as fate would have it, the roots of chaos theory go back to the nineteenth century and to the work of the leader of the scientific establishment of the day, the president of the French Academy of Sciences and cousin of the President of the Republic—Poincaré. His colleague, the mathematician Painlevé, who dreamt about the most unstable system of all, was himself twice Prime Minister of France.

### *3. Incommensurability: Special Relativity*

For Kuhn, science progresses by a great leap from one paradigm to its successor, located on the opposite sides of an impermeable barrier. More moderate views about barriers between historical periods are shared by

<sup>24</sup> For example, Feigenbaum's brilliant application of statistical physics to the description of unstable systems; see M. J. Feigenbaum, "Quantitative Universality for a Class of Non-Linear Transformations," *Journal of Statistical Physics* 19 (1978). Or, for the discovery of strange attractors; see E. N. Lorenz, "Deterministic Nonperiodic Flow," *Journal of Atmospheric Science* 20 (1963). The last discovery is rooted in fluid dynamics. Instability is very common to fluid flow, and Lorentz equations are obtained from an approximation of the Navier Stokes equation (see note 5 above).

many scholars. For example, the French historian Lucien Febvre maintained that unbelief and denial of God's existence were impossible in the sixteenth century (and, he thought, became possible only in the seventeenth century, especially after Descartes).<sup>25</sup> For deep cultural, social, and psychological reasons, people could not quite bring themselves to atheism. But what about the *conceptual* possibility of unbelief? Febvre describes at length the difficulties facing the idea of atheism in this period. However, he does not go so far as to claim that people could not understand such a concept; on the contrary, he brings plenty of evidence of people blaming others for being non-believers, with a detailed account of what it means.

Kuhn is dealing with a far more restricted domain: the transition from one scientific paradigm to the next. Nevertheless, he goes beyond Febvre and introduces a conceptual wall between one paradigm and its successor, to the extent that believers in the old paradigms cannot even grasp the new one. For him, incommensurability is a logical relation that has psychological and historical implications. Consequently, his discussion mixes philosophical, historical, and psychological issues. At one point, however, he does provide an argument, perhaps the only philosophical argument in all of *Structure* (section 9). He illustrates his general point using the transition from classical mechanics to special relativity. Here is the way it goes:

(1) The positivists hold that relativity is an extension of classical mechanics and that Newton's laws can be derived from Einstein's as approximations (so long as we are dealing with velocities much smaller than the speed of light).

(2) The positivist analysis stems from an error, because, despite the use of common terminology, the two theories attach different meanings to the basic physical concepts (e.g., mass, momentum, and energy).

(3) The fact that the concepts of the two theories have different meanings creates a conceptual gulf between them: they are incommensurable.

I think that (2) is a valid criticism of the positivist position in (1); however (3) does not follow logically from it, nor does it carry much weight historically. There are many threads that link the meaning of the relativistic concepts to the meaning of their classical counterparts. These links have been recognized in real time by the scientists involved, and they played a key role

<sup>25</sup> Lucien Febvre, *The Problem of Unbelief in the Sixteenth Century, the Religion of Rabelais*, tr. Beatrice Gottlieb (Harvard University Press, 1982; first published in French in 1942).

in the analysis, and eventual acceptance of special relativity as a valid theory. We can illustrate this by following one of these threads that runs through mathematics.

In the 1870s the German mathematician Felix Klein formulated a broad synthesis of geometry, which became known as the Erlangen Program.<sup>26</sup> Klein was a prominent mathematician and leader of the scientific community. For many years he headed the Department of Mathematics at the University of Göttingen, helping make it the most prominent center of mathematics and theoretical physics at the beginning of the twentieth century. Klein proposed that geometry be understood as the study of objects that remain invariant under a group of transformations, and he classified the different geometries according to their different groups. Klein also applied his method to classical Hamiltonian mechanics. (The application of groups of transformations in mechanics has a long history. In this case the transformations are referred to as “canonical” and their invariants were studied by Poisson and Liouville in the middle of the nineteenth century.)

Klein’s outlook was shared by many mathematicians of the age and similar accounts were given around the same time by others, including Lee and Poincaré. The classification of geometries by their transformation groups had a major impact on the history of mathematics. Poincaré’s contributions to the program included the analysis of non-Euclidean (hyperbolic) geometry in those terms, and most importantly, the invention of algebraic topology. He even brought some of this work to the attention of the general public in his philosophical book *Science and Hypothesis*.<sup>27</sup>

The Erlangen Program exerted decisive influence on how special (and later general) relativity was developed, understood, and assimilated.<sup>28</sup> From a conceptual point of view the link is direct. Physics studies observable

<sup>26</sup> The Erlangen Program derives from a lecture given by Klein in 1872, “Vergleichende Betrachtungen über neuere geometrische Forschungen” (A comparative review of recent researches in geometry), published more than two decades later in *Mathematische Annalen* 43 (1893). (An English translation by Mellen Haskell appeared in the *Bulletin of the New York Mathematical Society* 2 (1892/93).) The program is also presented in greater detail in his *Einleitung in die höhere Geometrie*, written with Friedrich Schilling (1893).

<sup>27</sup> Dover, 1952, first French edition, 1902. Poincaré gives Klein only small credit.

<sup>28</sup> J. D. Norton, “Geometries in Collision: Einstein, Klein and Riemann,” in J. Gray (ed.), *The Symbolic Universe* (Oxford University Press, 1999).

independent magnitudes, that is, those that remain invariant under the transformations from one inertial frame to the other. If we change the set of invariants the transformation should change too; this is what happened in the transition from classical physics to relativity. The terms in the two theories refer to different concepts, but in both theories they are just examples of invariants in possible geometries. The historical point is that *this description is not anachronistic*. Rather, it is precisely the way many members of the “revolutionary generation” understood the change, including veterans who had invested much effort in classical mechanics. This is also the standard view of relativity today.

Einstein himself was influenced by the conception of geometry of his day. The non-invariance of the Maxwell equations under the “old” Galilei transformations, which manifests itself in a variety of ways, is certainly a significant motivating component behind special relativity. His first article on the subject<sup>29</sup> begins with a description of “asymmetries which do not appear to be inherent in the phenomena,” which are by-products of this non-invariance. He continues, in the sixth section, to demonstrate how the asymmetries are removed by the new (Lorentz) transformations. However, in his 1905 derivation, Einstein did not take geometric invariance as a starting point, and proceeded to derive the theory from what he considered to be more fundamental assumptions.

This was not the case for many members of that generation, in particular around the very influential Göttingen circle. Klein himself wrote about the new theory, and even suggested calling it the “theory of invariants.”<sup>30</sup> There were numerous references to this connection, but the peak is reached with the important article by Hermann Minkowski,<sup>31</sup> another famous mathematician from Göttingen, who gave special relativity its geometrical expression that remains standard to the present day—all very much in the spirit of the Erlangen Program. A few years later Einstein also adopted the geometrical perspective, which proved essential for developing general relativity.

<sup>29</sup> A. Einstein, “On the Electrodynamics of Moving Bodies,” English version in *The Principle of Relativity*, tr. W. Perrett and G. B. Jeffery (Dover, 1952; first published in German in 1905).

<sup>30</sup> Felix Klein, “Über die geometrischen Grundlagen der Lorentzgruppe,” *Deutsche Mathematiker Vereinigung* 19 (1910).

<sup>31</sup> “Space and Time,” in *The Principle of Relativity*, see note 29 above (first published in German in 1909).

Nor is it true that the only scientists to engage with the new theory were young men ablaze with a revolutionary fervor. Klein himself belies this; he was already in his mid-fifties and, as noted, a central pillar of the scientific establishment, when Einstein published his papers on special relativity. He had invested significant energy in the development of classical mechanics, as exemplified by his *On the Theory of the Top*, which deals with the Newtonian physics of the motion of rigid bodies. This vast work of almost a thousand pages was published in four volumes between 1897 and 1910.<sup>32</sup> What makes this point even more interesting is that it is far from clear whether the concept “rigid body” has a clear meaning in relativity theory. Klein’s coauthor, the theoretical physicist Arnold Sommerfeld, also moved later to another paradigm and made major contributions to the development of quantum theory. Klein, for his part, also worked on general relativity.

Poincaré occupies an intermediate position in this story. Much of his mathematical work deals with geometry, computing groups of transformations and their invariants. Simultaneously with Einstein, and independently of him, Poincaré presented the Lorentz transformations in their modern symmetrical form, and gave them their name.<sup>33</sup> Like Einstein, he proved that Maxwell’s equations are invariant under the Lorentz group; in fact, he went further and derived the basic invariant magnitude of special relativity, which was later used by Minkowski (and is now known as Minkowski’s metric). However, Poincaré could not quite bring himself to see the theory as an expression of a new space-time structure. In other words, he did not accept the relativity principle that asserts the equivalence of all inertial frames of reference. It is not clear whether and to what extent he may have done so in later years (he died in 1912). But there is no doubt that Poincaré took a very great step beyond the classical mechanics to which he had devoted so many years of his life.

Geometry is only one of the many threads that connects relativity to classical physics, and as such a key for understanding the historical transition. There are many such cases in the history of science where the meaning of concepts in the old and the new theory are related in deep and non-trivial

<sup>32</sup> Felix Klein and Arnold Sommerfeld, *Über die Theorie des Kreisels* (Leipzig, 1897–1910). The fourth volume appeared when Klein was also involved in relativity theory.

<sup>33</sup> H. Poincaré, “Sur la dynamique de l’électron,” *Comptes Rendus Académie des Sciences* (Paris) 140 (1905).

ways, and are perceived to be so related while the transition occurs. It is not that Kuhn simply chose an unfortunate example to illustrate his philosophical argument; this case is the rule, not the exception.

The general philosophical issue underlying Kuhn's work is whether there can be two conceptual systems that are so radically different that their users cannot but fail to understand each other. Here Kuhn explicitly follows in the footsteps of Wittgenstein, and to some extent Quine. This issue remains central to analytical philosophy today. However, the value of Wittgenstein's position notwithstanding, it seems to me quite irrelevant to our concern. The reason is that it is not clear that the "language games" problem, or the indeterminacy of translation, make any appearance in the history of science. I do not want to argue that there are no radical and dramatic changes in science and that progress is "linear." But the drama is manifested in the cognitive content of the proposed changes and in the experimental means and technology that enables the corroboration of that content. These aspects are transparent to the scientists of the generation in question, although often fiercely argued. The competing sides usually share background, education, and knowledge to such an extent that the debated issue forms a small part of their conceptual makeup. Maybe it is exactly the issue to fight over fiercely because its consequences are well understood. However, there is no historical basis for the contention that the older generation usually runs up against a conceptual wall that prevents it from grasping the new paradigm.

The key point is that scientists generally do not "accept" or "reject" the proposed change, they usually defer a decision until the matter has become clearer. In the interim, some of them contribute to both paradigms at the same time, without much ado. Others continue to work in both paradigms even after the verdict has been rendered, so long as the questions associated with the old paradigm still strike them as interesting. Here, as in many other aspects of human behavior, reactions to the innovation are distributed normally: beginning with those few who oppose the new idea vigorously, through the majority who suspend judgment, all the way to the few who adopt it enthusiastically. This is also what tends to happen with regard to major developments in normal science that do not involve an extreme change of theory. For example, when Gell-Mann and Zweig first advanced the notion of quarks (as part of the quantum theory of fields and particles, whose basic tenets they left untouched), some jumped on their bandwagon at once, others thought it was no more than a mathematical model, while the

majority adopted a wait-and-see attitude. Another example is the Aharanov-Bohm effect. Here too those who were enthusiastic about the prediction were countered by those who held that it was merely a superfluous by-product of the quantum formalism. The majority, again, waited for experimental and theoretical clarification. If there is any real distinction between normal science and a scientific revolution, then, it is not manifested in incommensurability, but rather in the way in which we quantify, after the fact, the gulf between the old and the new.

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