On Thomas Kuhn's Philosophical Significance

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In the first section of this paper, I will by way of introduction survey Thomas Kuhn's bestseller *The Structure of Scientific Revolutions*. This introduction is necessary because it is by no means clear how a book of *Structure's* type can be philosophically significant at all. In the second section, I will then discuss some aspects of its philosophical significance. The most important change in Kuhn's views after *Structure* is his turn away from a more perceptual view of scientific revolutions toward a more conceptual one. This new view, together with its basis, a new theory of meaning for empirical concepts, will be the subject of my third section. In the final section I will discuss some philosophical consequences of Kuhn's new theory of meaning.

I. The Structure of Scientific Revolutions—Survey

Probably the most important single work contributing to the turmoil in philosophy of science in the early 1960s was *The Structure of Scientific Revolutions*. Let us look at its outline in order to survey its subject, which will prepare the discussion of its philosophical significance. The table of contents of *Structure* presents a developmental scheme for scientific fields in the basic sciences. I note in passing that this sort of structuring occasionally makes the close reading of a particular topic very difficult, as passages scattered through the whole book must be considered.

The first chapter of *Structure* opens with a much-quoted sentence that concisely describes what the book is all about:

Configurations, 1998, 6:1–14

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.[1]

Here, Kuhn refers to two different approaches to the history of science, or two different historiographic traditions.[2] For the older tradition, the primary goal was to reach a deeper understanding of *contemporary science* by displaying its historical evolution. This was to be achieved by a chronological presentation of the discoveries of today's concepts, theories, experimental methods, and so on. The image of scientific development that resulted was necessarily *cumulative:* science grows step by step, adding new pieces of knowledge to those already in place. But this form of historiography invariably distorts the presentation of the older science by projecting contemporary science into the past. The "new historiography of science," as it was called, attempts instead to display the historical integrity of a science in its own time, mainly following Alexandre Koyré's model. More specifically, the concepts, the research problems, and the standards of evaluation of an older science must be reconstructed in a historically adequate way—that is, understood in their own terms, not from today's perspective. Kuhn's project is irretrievably tied to this second mode of historiography. The aim of *Structure,* as Kuhn puts it, is to delineate a new image of science "by making explicit some of the new historiography's implications" (p. 3).

The image of science that emerges from this kind of historiography contains a developmental scheme for scientific disciplines. Before reaching maturity, nascent scientific fields are typically characterized by controversies between competing schools; there is no general consensus among the practitioners of an emerging field about its foundations. This competition may eventually end when one group produces an exemplary solution to a preeminent research problem with two characteristics: the solution is both sufficiently unprecedented, and sufficiently suggestive for further scientific work, that it attracts members from the other schools. These model solutions are called paradigms; they implicitly guide research in the following period called "normal science." Normal science is characterized by a broad consensus of the practitioners in that field about

1. Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2d ed. (Chicago: University of Chicago Press, 1970), p. 1. Subsequent quotes from this work will be identified by page numbers in parentheses.

2. For a summary of the two traditions, see Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* (Chicago: University of Chicago Press, 1993), sect. 1.2.

fundamental questions, and consequently, by a particular mode of research. This mode of research can be described by its analogy to puzzle-solving, where exemplars of puzzles include chess problems and crossword puzzles. The most interesting property shared by puzzle-solving and normal science is that in both cases there are regulations that are constitutive of the respective activity, which therefore cannot be called into question by the activity—and consequently, neither activity can be described as a test or a confirmation of the guiding regulations. Thus, in solving a chess puzzle the rules of chess are neither really tested nor confirmed, and nor, by analogy, are the rules of quantum mechanics in calculating certain properties of an atomic nucleus.

Normal science is always confronted with (ordinary) anomalies—that is, with phenomena or problems that behave contrary to the expectations supplied by the paradigm. Usually, the validity of the guiding regulations of normal research is not called into question because of an (ordinary) anomaly. But under special circumstances, that validity may be challenged, and then the anomalies become *significant* anomalies, at which time the practice of science changes into "extraordinary science" or "science in crisis." In these periods the aim is to amend or even overthrow the regulations that had previously been binding. Research tends to focus on the significant anomalies and their context. If this research leads to a new theory that is accepted by the scientific community and replaces the old theory, a scientific revolution has occurred. Scientific revolutions in Kuhn's sense are, as he puts it, "the tradition-shattering complements to the tradition-bound activity of normal science" (p. 6). The rejection of the old theory is accompanied by a change of the problem-field and its related standards of solution, and by a sometimes subtle change in basic scientific concepts. Revolutions can even be described as transformations of the world in which scientific work is done. In another sense however, the world is still the same—but quite some effort is needed to reconcile these two aspects of revolutionary change.[3] Kuhn compresses these features of revolutions into the concept of "incommensurability": this relation holds between successive traditions of normal science. In *Structure*, the concept of incommensurability is not entirely clear; it was therefore the subject of much criticism and also of much misunderstanding. Most of Kuhn's philosophical work after *Structure* implicitly or explicitly aims at clarifying and developing the incommensurability concept. I will return to this topic later.

3. See ibid., chaps. 2, 3, and 6.

According to Kuhn, incommensurability forces us to rethink the concept of scientific progress. First, progress in science is not cumulative, due to the conceptual changes during revolutions. Furthermore, Kuhn denies that it is a process of approximating truth—an idea that has been widely held at least since C. S. Peirce. Instead of conceiving of scientific progress as a teleological process (i.e., one that is goal-directed), we should think of it in the same way that Darwinians think of evolution: Darwinian evolutionary theory states that there is no goal of evolution toward which it is directed. In a similar way, in scientific development there is no "set goal," no "permanent fixed scientific truth" that science approaches (p. 173). According to Kuhn, such a thing simply does not exist. However, he stresses that this does not imply that there is no progress in the sciences. There *is* progress, but it is not in the form of an increase of verisimilitude or an approach to The Truth; rather, it is in the form of "an increase in articulation and specialization" of scientific knowledge (p. 172). Therefore, the widespread characterization of Kuhn's theory as entirely relativist is simply false.

II. Some Aspects of the Philosophical Significance of *Structure*

Why is Kuhn's theory of scientific development philosophically significant? Why did philosophers even take notice of a theory that appears essentially historical, or at most an inductive generalization from historical episodes? The reason for the philosophical interest in *Structure* is that Kuhn's theory ran counter to many philosophical convictions about science that were held in the early sixties. I will briefly discuss six of them. First, I have already mentioned Kuhn's altered view of scientific progress. Second, this view implies the untenability of those forms of realism which assert that science at least approximately describes what is really "out there," independently of any observer. Instead, theories describe the world in terms of concepts that are historically contingent and that may change in the future. Third, because of the change of basic scientific concepts through revolutions, the classical conception of reductionism is also hardly tenable. According to this conception, theories may be reduced to more fundamental theories without changing their meaning by redefining their concepts using the concepts of the reducing theory, and then deriving their laws from the laws of the reducing theory. But if incommensurability prevails between the pair of theories in question, the reduction relation cannot hold because some of the required redefinitions are impossible due to meaning shifts. In fact, as has become clearer in Kuhn's work after *Structure*, mutual untranslatability of some of the key terms is the hallmark of incommensurability between theories.

Fourth, many of *Structure*'s assertions stood in marked opposition to Popper's critical rationalism, which was, at the time, the only philosophical position that dealt seriously with scientific development. For instance, Kuhn's normal science as seen from Popper's perspective just seems like bad science because it is directed, not at a critical test of the guiding regulations, but at a quasi-dogmatic exploitation of their potential. Yet in Kuhn's view, the critical evaluation of fundamental theories is restricted to periods of extraordinary science—and even then, scientific practice is not simply an attempt to falsify theories by confronting them with basic statements about nature, as Popper would have it. Rather, theory evaluation is always a comparative procedure in which at least two theories are assessed with respect to their cognitive abilities, especially as to whether they can cope with the significant anomalies that caused the crisis state in the first place. Thus according to Kuhn, theory falsification as described by Popper is a stereotype that is not found in the actual history of science.

A fifth consequence of Kuhn's theory is the abolishment of the idea that science is guided by the scientific method, construed as a set of rules rigorously to be followed. Primarily due to the influence of Bacon and Descartes, this idea has dominated the understanding of modern science from its very beginning. But according to Kuhn, it is exemplary problem solutions that guide scientific research in its normal phase. Their cognitive potential for research is not exploited by explicit (or fully explicable) rules, but rather by implicit analogies; new problems are identified in the light of solved ones, and new solutions are judged as legitimate in a like manner.

Finally, the sixth reason why *Structure* significantly provoked philosophical interest is that for Kuhn the principal agents of science are communities, not individuals. In the philosophy-of-science tradition before Kuhn, no one explicitly questioned the identity of the principal agent of science—but, armed with the distinction between individuals and communities, it is quite obvious that in the older tradition, the individual scientist was taken to be the principal actor. The possibility of two scientists' rationally disagreeing was not permitted, nor were there philosophical discussions about the gradual formation of scientific consensus out of disagreement, or about the development of disagreement from a previously established consensus. In all forms of meaning theories, confirmation theories, and theories of scientific testing, there was invariably at most one rational choice or procedure. No one had developed a useful, let alone an indispensable, function for a *community* of scientists—that is, for individuals who disagree about certain scientific matters without at least one of them being necessarily irrational. No one had allowed for the possibility that two scientists might adopt the same theory, but for different reasons (and by "different reasons," I mean here "different *good* reasons"). Science was seen as a one-person game—but for Kuhn it is, at its heart, a social enterprise. To determine exactly how this sociological perspective enters his theory is no trivial matter, nor is it easy to determine the relationship between this sociological component and his epistemological claims. Certainly, Kuhn's sociological perspective had been badly misunderstood in the beginning, especially by Imre Lakatos. Furthermore, it made it possible to see many scientific events as social facts—which is, of course, the basis for the more radical sociological approaches to science that have emerged within the last decades. Kuhn was not particularly happy about most of them because, for him, the sociologists missed or dismissed the epistemological element, but the sociologists were happy with Kuhn for having opened (or reopened) an entire field with many subspecialties and directions—the social studies of science.

III. Developments after *Structure,* Specifically Concerning Empirical Concepts

It is not only that Kuhn opposed many doctrines of the established philosophy of science: an additional factor in the sometimes quite vehement reactions to *Structure* was a deep and widespread misreading of its main theses. Kuhn was thus constantly challenged to articulate and refine his thesis in greater detail, in the course of which his position also shifted in some important respects.

During the early reception of *Structure,* the main target of criticism was the paradigm concept, which was seen as highly equivocal. Kuhn responded in the 1970s by distinguishing a narrow sense of paradigm, meaning exemplary problem solutions, from a wide sense that comprised all the components of scientific consensus called the "disciplinary matrix."[4] The latter includes, among other elements, scientific values like accuracy, consistency, fruitfulness, scope, and simplicity, which operate constantly but become especially visible during theory choice. Since these values only guide, but do not dictate, theory choice, individual scientists can rationally *disagree* concerning theory evaluation. This would be impossible if theory choice

4. See especially Kuhn's "Postscript" in the second edition of *Structure* (above, n. 1), pp. 174–210.

were determined by some fixed set of rules that defined the canon of scientific rationality.[5]

In my view, however, the most important change in Kuhn's position was a shift from the description of scientific revolutions in *Structure* predominantly in terms of visual metaphors, to a description by means of a linguistic or conceptual framework. In *Structure*, revolutions were depicted as something like visual gestalt switches, and their outcome as a changed way of seeing the world. Though meaning shift already played some role in *Structure*, meaning was, to put it pointedly, parasitic or dependent on perception: perception in its literal sense was the basis for the meaning of observational terms, and perception in its metaphorical sense was the basis for the meaning of more theoretical terms. But from the late sixties on, the linguistic aspect of scientific development gained some independence from perception. Of course, this should not be misunderstood as a statement about the elimination of the role of perception in Kuhn's theory. Linguistic change in the sciences is a response to pressure generated by observation or experiment, but the nature of the induced change itself is linguistic, not perceptual.

There are two main reasons for this change of perspective. The first is that there are serious methodological problems concerning the investigation of specific perceptions and how they change. This holds specifically for those perceptual changes that may be relevant for a philosophical study of the history of science. When Kuhn attempted to investigate perceptual change in the late sixties, he devised a promising model but ended up trying to invert 50-by-50 matrices or so (which, in 1969, was not a fun thing to do).

The second reason why Kuhn switched from the perceptual to the linguistic perspective is not methodological, but substantive. Kuhn's principal metaphor describing scientific revolutions in *Structure* was the gestalt switch metaphor: in a scientific revolution, the world or the worldview changes like the picture in a visual gestalt switch. In a perceptual gestalt switch, what one sees changes substantially even though the elements of the picture, taken separately, remain stable (although they may represent different things when integrated in the respective gestalt). A scientific revolution, according to the early Kuhn, is like such a visual gestalt switch in that while the whole picture of the world changes drastically, most of its elements are incorporated by the new view, though they will have a different meaning. Thus, the metaphor makes intelligible, to some degree, why the

5. On this topic, see Hoyningen-Huene, *Reconstructing Scientific Revolutions* (above, n. 2), sect. 4.3.c.

world changes in some sense in a revolution, while in another sense it remains the same (I have already mentioned this strange characterization of revolutions). The important difference between visual gestalt switches and scientific revolutions is that in a revolution, contrary to the visual gestalt switch, we have no access whatsoever to the elements of the worldview when abstracted from the whole.

The main problem with the gestalt switch metaphor lies in the fact that gestalt switches are events that individuals and only individuals experience, whereas a revolution is something primarily social—I mentioned earlier that the principal actors of science are communities, not individuals. To use an individual's perceptual event, like a gestalt switch, as a metaphor for scientific revolutions amounts to committing a category mistake—namely, of misdescribing something essentially social by analogy to an individual's experience—as Kuhn realized.[6] Although perception is socially conditioned, it is possessed by individuals; whereas, although language is possessed by individuals, it is essentially social by nature.

These were the two main reasons why Kuhn tried to reformulate the theory of *Structure* on the basis of a sketch of a new theory of meaning for empirical concepts. His construction of this theory begins with the question, In what way are empirical concepts learned, and at what point is the learning process complete?[7] The idea driving this question is that the nature of meaning should be discoverable by investigating the process of concept acquisition. When someone does not know a concept and then, after some learning process, does know it, that person must have acquired the meaning of the concept in the process. So, how does concept learning proceed, and what exactly is learned?

First of all, it is an empirical fact that empirical concepts are generally not learned by a set of necessary and sufficient conditions that define the concept in the traditional sense. Rather, the pupil is typically exposed to objects that are instances of the concept and to objects that are not. Usually this exposure to the exemplars is done by the ostension of appropriate objects. How this ostension works in detail is somewhat tricky, because the presuppositions for successful acts of ostension are not clear. At any rate, the pupil is then told whether or not the concept applies to the object in question. Then, pupils have to do the required coordination of objects and concept them-

6. T. S. Kuhn, "Response to Commentators," in *Possible Worlds in Humanities, Arts, and Sciences: Proceedings of Nobel Symposium 65*, ed. Sture Allén (Berlin: de Gruyter, 1989), p. 50.

7. For references, see Hoyningen-Huene, *Reconstructing Scientific Revolutions* (above, n. 2), p. 70, n. 22 and sect. 3.6.g.

selves, and they will be either reassured or corrected depending on whether they are right or wrong. After a short time, they will apply the concept in the same way as the teacher—that is, they will have learned the concept. In other words, they will have acquired its meaning.

Here is a real-life example that Kuhn liked very much and wanted to include in his book. The main actor of this example is my son Alexander when he was about twenty months old. On our daily trips to the kindergarten, Alexander quickly became interested in the big moving things on the road, and we taught him—more or less unconsciously—the German equivalents of the terms *truck*, *bus*, and *tram*. This training went on without any traits of these vehicles being mentioned, let alone definitions. We said only "Yes, this is a truck," or "No, this is not a tram." His mastery of the three terms was soon perfect, and he began to be interested in other vehicles, especially motorcycles. I do not know which traits of the vehicles he used for identification, but at first they were surely visual (given his head and eye movements and pointing). But a little later, in our garden where traffic cannot be seen, he heard the sound of a bus and said "Bus! Bus!" He therefore had extended the range of traits used to pick the referents of the different classes of big vehicles to include acoustic ones.

Now, what is it that someone like Alexander has learned when he masters such terms as "bus," "truck," and "tram"? The core elements to be learned for concept acquisition consist in similarity relations that apply to objects that are picked out by the same concept, and, equally important, dissimilarity relations that apply to objects belonging to neighboring concepts. In Alexander's case, in order to apply the terms in question correctly, he had to learn the similarity that exists between buses, and their dissimilarity to both trucks and trams. This learning process is far from being purely receptive. On the contrary, he was not told what makes all buses similar to each other and dissimilar to trams and trucks—he was only told which vehicle was a bus and which was a tram or a truck. He had to detect for himself similarities and dissimilarities that were sufficient to both identify and distinguish these vehicles; the learning process was complete once he had discovered such traits. Obviously, in the beginning he used exclusively visual traits. But as the example shows, he quickly learned that he could also use the typical sound that buses produce in order to identify them. Therefore, we can conclude that in principle any trait can be used to identify the members of the respective classes—as long as the trait suffices for the identification of members of a class and also for distinguishing them from members of neighboring classes. Just how different the traits used for identification can be within a single language community may be seen from the joke that Johnny was unable to identify Adam as Adam and Eve as Eve in an old painting because they were not wearing any clothes.

What are the consequences for the theory of meaning as it applies to empirical concepts? I will develop the answer to this question in five steps. First and foremost, it is clear that the learning process does not consist in the transmission of an appropriate explicit definition of the terms in question. What is learned is the correct application of the terms on the basis of certain similarity and dissimilarity relations, and this is not identical with a definition of the term. Alexander was able to distinguish between referents and nonreferents of the three terms for vehicles, but he was certainly unable to provide correct definitions of them in terms of necessary and sufficient conditions. Thus, the meaning of an empirical concept must not be equated with its definition.

Second, one can conclude from the variety of admissible traits for concept application that the particular traits employed by an individual to identify the concept's referents make no *immediate* contribution to the concept's meaning. Otherwise, individuals employing different criteria for the correct application of the same concept would not share the same meaning.

Third, what speakers of the same language community must share is the set of similarity and dissimilarity relations that classify certain objects as belonging to a particular class and not to one of its neighboring classes. Again, how an individual speaker operationalizes these similarity and dissimilarity relations by employing some particular set of criteria is unimportant, as long as his or her set of similarity and dissimilarity relations is the same as that of any other member of the language community. From this it follows that it is the set of similarity and dissimilarity relations that is constitutive of the meaning of empirical terms.

Fourth, empirical terms are not learned in isolation from each other. Alexander learned the terms *bus*, *truck*, and *tram* together; identifying a bus means not mistaking it for a truck or a tram. In other words, in order to use the term *bus* correctly, one must learn the whole set of similarity and dissimilarity relations connecting buses, trams, and trucks. Consequently, the term *bus* does not have a meaning in isolation from the other two terms. Kuhn calls this feature of language its "local holism." The holism is local only to the extent that the meaning of the term *bus*, though connected to the meaning of the terms *truck* and *tram*, is not connected to the meaning of, say, *cat* or *dog*.[8]

8. For references, see ibid., p. 100 n. 175.

Fifth, the acquisition of empirical concepts requires learning a classification of objects. For a given classification, there exist more general concepts, such as "big road vehicle" for bus, truck, and tram, and more specific concepts, like kinds of trucks. Therefore, what is learned during concept acquisition is a part of a taxonomy that is used by the respective language community. In any taxonomy, the terms involved can stand in only one of two relations, either exclusion or inclusion. Partial overlap between contrasting terms is forbidden: something is either a planet or a star, either gold or silver, either a bus or a truck, but never both. Borrowing the term *lexicon* from linguistics, Kuhn called the net of relations in such a taxonomy the "structure of the (respective) lexicon" of concepts.[9]

Now we are ready to sketch what is involved in the knowledge of a term's meaning. One has to know that portion of the lexicon of empirical terms in which the term in question occurs. Knowing the lexicon is knowing something social by individualistic means. To know the structure of the lexicon means to know the set of similarity and dissimilarity relations that hold between the respective objects. This set represents the social aspect of language; every speaker must know it in order to be a member of the language community—or, in other words, to apply the concepts in the same way as the other members of the community. But how one knows the set of similarity and dissimilarity relations may differ from one speaker to the next. Different individuals may differ in how they pick the referents and the nonreferents of a term, but this difference does not usually surface while communicating. Individual differences are thus tacitly embedded in what is socially shared. Meaning is thus something social and is contained in the structure of the respective portion of the lexicon. But meaning may be operationalized in different individuals in different ways.

IV. Some Philosophical Consequences of the New Theory of Meaning

Now I want to discuss two interesting philosophical consequences of Kuhn's theory of meaning for empirical concepts. The first concerns the fact that in the use of empirical concepts in a language community, empirical knowledge about the world can accumulate. Note that I said that *in the use* of these concepts knowledge can accumulate, and not that these concepts can be used to articulate knowledge; the latter is trivial, the former is not. To illustrate what I mean, imagine that we lived in a world in which men and women

9. For references, see ibid., p. 111, n. 222.

could be distinguished by their hairstyle or their clothing (I am aware that this is a highly counterfactual assumption). The portion of the lexicon that contains the terms *man* and *woman* is constituted by the similarities among men, the similarities among women, and their mutual dissimilarities. Now, different speakers of the community will use different criteria to identify men as men and women as women. In the counterfactual world just described, some will use hairstyle, others will use clothing, and still others will switch between criteria. As long as hairstyle and clothing habits are stable, these individual differences in identifying men and women are entirely hidden in everyday communication, and the existence of the differences presents no communicative problem whatsoever. But in the coexistence of these criteria, empirical knowledge about the world is accumulated—namely, that people with a certain hairstyle invariably wear certain clothes, and vice versa. Thus, empirical terms introduced in the manner sketched on the basis of similarity and dissimilarity relations, and not by explicit definitions, have the property that empirical knowledge about the world can reside in their use.

Second, given the way in which empirical concepts are introduced, two modes of language change can be distinguished. In the first mode, the structure of the respective lexicon remains untouched, which means that the relationship among all of the concepts remains the same. However, the criteria used to pick the respective referents may systematically change. For instance, in time measurement the basic periodic motion used in order to pick certain time intervals may change from earth rotations to the motion of pendulums, and then even to some frequency in an atomic process. Or, the primary means of identifying a chemical element may change from a chemical reaction to spectroscopic data. Generally speaking, the better one knows an entity, the more possibilities one has for identifying it. Thus, progress in normal science is often connected with shifts in identification procedures.

The second mode of language change is characterized by change in the structure of the respective lexicon and is, according to Kuhn, characteristic of scientific revolutions. Accordingly, it can be called revolutionary language change. As an example, take the classification of bodies in the Aristotelian tradition and compare it with the classification of bodies at the beginning of the modern period.[10] In the Aristotelian tradition, bodies were either celestial or terrestrial. Celestial bodies were either spheres or—possibly—souls. Among the spheres, stars and planets could be distinguished: the sun and the

10. I owe this illustration to Peter Barker.

moon were planets, the earth of course was not. The basic classification of terrestrial bodies followed from the four elements: there were earthly, watery, fiery, and aery bodies. Now contrast this classification with that of Galileo and Descartes: All celestial bodies belonged either to the class of stars, the sun being one of them; or to the class of planets, which for them included the earth but excluded the sun and the moon; or to the newly created class of satellites, including our moon and the newly discovered moons of Jupiter. Note that these classifications embody some fairly distinct similarity and dissimilarity relations. For instance, in the old taxonomy the sun, the moon, and Mars count as similar: they are all planets; however, they are very different from the stars, and from the earth, which is in a class by itself. In the new taxonomy, the sun is now grouped among the stars and is thus similar to them; the moon is now dissimilar to both the sun and Mars, as it belongs to a entirely new class, namely the satellites; and the earth is now similar to Mars.

Let me now combine these two consequences of Kuhn's theory of meaning. I will do so with a real-life example, although it is a very old one.[11] Some 3,500 years ago, in ancient Egypt, the southern direction could primarily be identified astronomically. In addition, it was possible to use the criterion "upstream," as the Nile happens to flow from south to north in that region. The hieroglyph for "south" was a ship with sails, and that for "north" a ship without sails. This shows that a third criterion for directions could be used. If a moving ship were seen at some distance from the Nile, one could identify directions by noting whether or not sails were set: a ship with sails was moving south or upstream; a ship without sails, north or downstream. The coexistence of these criteria contains empirical knowledge, in this case about a geographical peculiarity of the region and about the use of sails. However, something very strange happened when, on an expedition outside their kingdom, the Egyptians discovered the river Euphrates, which happens to flow from north to south. In this new situation, the different but normally interchangeable criteria used to distinguish north from south no longer functioned properly. The astronomical criterion yielded a different result from the flow criterion and the sail criterion. The reason for this is quite clear: In the new geographical situation, the empirical knowledge on which the interchangeability of the criteria rested was no longer valid. But as this knowledge had become a part of language, its violation became actually apparent as a conceptual confusion.

11. See Paul L. Czonka, "Advanced Effects in Particle Physics. I," *Physical Review* 180 (1969): 1266, 1280 app. A.

This confusion has been recorded on a stele of King Thutmose I from the sixteenth century B.C., where the Euphrates is referred to as "that inverted water which goes downstream in going upstream," or, equivalently, "that inverted water which goes north in going south."[12]

It is this sort of situation that Kuhn had in mind when he spoke about incommensurability. Incommensurability, expressed in terms of the new linguistic framework, results from a difference in structure of the respective lexicons. This structural difference can become manifest in various ways. First, because a network of similarity and dissimilarity relations is constitutive of the structure of the lexicon, incommensurability involves some change of similarities and dissimilarities. Concomitant with this is a restructuring of the taxonomy and a redistribution of some of the objects ordered by the taxonomy. (An example was the transition from the Aristotelian to the early modern taxonomy of celestial bodies.) Second, different lexicons may embody different knowledge claims about the world. As this knowledge has become part of the language, the invalidation of empirical knowledge implicit to the language may show up as conceptual confusion. (The description of the Euphrates in terms of hieroglyphs is an example.) Communication problems among people using different lexicons may be a consequence. Third and last, statements that are articulated by means of one lexicon may not be literally expressed by means of another, incompatible lexicon. The result is that it is impossible to translate incommensurable theories into each other in a literal sense.

From these three features of incommensurability in the new linguistic framework one can see how far Kuhn has come since 1962. Then, incommensurability was an almost inscrutable feature of revolutions connected with sudden gestalt switches. Now, incommensurability becomes a property of a specific language change that may happen gradually for some people and suddenly for others. It is even open to modeling by cognitive scientists. I hope that these features will remove the air of mystery that surrounds incommensurability, and that in the future a little more consensus will emerge about its philosophical significance.

12. James Henry Breasted, *Ancient Records of Egypt: Historical Documents from the Earliest Times to the Persian Conquest. Collected, Edited, and Translated with Commentary* (New York: Russell and Russell, 1906; reprint, 1962), vol. 2, p. 31.