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THE APPLICABILITY OF KUHN'S PARADIGMS TO THE HISTORY OF LINGUISTICS

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This paper examines the applicability to the history of linguistics of Thomas Kuhn's conception of the history of science. It concludes that his notion of *REVOLUTION*, borrowed from the history of the non-sciences, can be applied to the history of linguistics; but the same is not true of his other key notion, the *PARADIGM*. The possession of paradigms, according to Kuhn, is what distinguishes the hard sciences from fields in the humanities and social sciences which have not achieved scientific maturity. Kuhn regards a paradigm as (1) resulting from an outstanding scientific achievement on the part of a single innovator, and (2) commanding uniform assent among all the members of the discipline. If these two requirements are to be everywhere met, the concept cannot be applied either to the history or the present state of linguistics. Serious objections can also be raised to other features of Kuhn's theory, such as the view that shifting allegiance from one paradigm to another is a largely irrational process. The paper recommends, then, that linguists abandon the theory.

This paper examines how Thomas Kuhn's conception of the history of science has been applied to the history of linguistics. Specifically, it poses two questions: (1) Has Kuhn's theory been correctly applied to the history of linguistics? and (2) Is the theory intrinsically applicable to the history of linguistics? For readers who may not be fully acquainted with Kuhn's ideas, I preface my discussion with a summary of them. I close with a few general comments about the adequacy of Kuhn's conception as a tool in intellectual historiography.

When Kuhn's monograph *The structure of scientific revolutions* was first published in 1962, as a volume in the *International encyclopedia of unified science*, it was immediately widely acclaimed and widely criticized. Particularly incisive criticism was leveled, e.g., by Shapere 1964. Kuhn responded to his critics by clarifying and developing his position in subsequent publications. In 1970 he published a revised edition of the book, enlarged by the addition of a 36-page postscript. Some appreciation of the storm of controversy which Kuhn's ideas have aroused among philosophers of science may be gained by perusing the proceedings of the 1965 International Colloquium in the Philosophy of Science (Lakatos & Musgrave 1970)—which was devoted to a discussion of the theory, and at which Kuhn attempted to answer his critics (1970b). The revised version of the theory has been subjected to trenchant criticism by Shapere 1971.

Two theses are basic to Kuhn's theory: one about the nature of science, and the other about how scientific disciplines develop. As for the first, Kuhn holds that science cannot be identified with the particular collection of facts, theories, and methods exhibited in current textbooks. Hence the history of science is not the story of how this fund of knowledge and expertise was acquired. It also follows that out-of-date theories are no less scientific than those current today: all one can say is that the canons of scientific theory and practice vary from period to period. In other words, Kuhn proposes to relativize the notion of science.

As regards the way scientific disciplines develop, Kuhn's second basic thesis is

that they do not develop by gradual accretion of discoveries. Science is not, as has been widely supposed,¹ a cumulative enterprise in which more and more successful generalizations are achieved on the basis of more and more successful measurements and calculations. The historian of science, therefore, cannot hope to characterize the history of a scientific discipline by merely identifying the respective contributions of various practitioners in the past to our present-day stock of knowledge and chronicling the step-by-step process of fresh observation and inductive reasoning based thereon. Kuhn pictures things in a different way: if the progress of a scientific field is plotted on a graph, then the line of development, as he sees it, will show not only smooth upward curves, but also periodic quantum leaps. The quantum leaps are **SCIENTIFIC REVOLUTIONS**, and the smooth portions of the curve are **NORMAL SCIENCE**.

However, Kuhn views a scientific revolution not just as a break in the continuity of a particular scientific tradition, but (more importantly) as an event brought about by the striking achievement of a **SINGLE** scientific genius. His favorite examples of scientific revolutions are the theoretical upheavals associated with the names of Copernicus, Newton, Lavoisier, and Einstein. The role of the lone innovator is essential to Kuhn's conception of a scientific revolution.

As for the periods in between the quantum leaps, Kuhn contends that each period of normal science in the development of a scientific discipline corresponds to one and only one conceptual and methodological framework or **PARADIGM**. In a nutshell, paradigms are 'universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners' (viii).² They are, at one and the same time, concrete scientific practice as well as the law, theory, application, and instrumentation cohering with it. (Kuhn now prefers to call these 'disciplinary matrices', but I shall retain the more familiar term 'paradigm' here.)

In the 1970 version of Kuhn's theory, a paradigm is said to have four components: symbolic generalizations, models, values, and exemplars. **SYMBOLIC GENERALIZATIONS** are expressions which can readily be cast in logical form, such as $f = ma$, Newton's second law of motion. Such formulas are used by adherents of a paradigm in order to apply 'the powerful techniques of logical and mathematical manipulation in their puzzle-solving enterprise' (183). **MODELS** are conceptual analogies which provide the members of a discipline with an ontological framework, e.g. the notion that 'the molecules of a gas behave like tiny elastic billiard balls in random motion' (184). **VALUES** are the criteria used by the adherents of a paradigm to judge between competing theories, identify recalcitrant problems, and justify the particular way they conduct research. Finally, **EXEMPLARS** are 'the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science

¹ On the notion of science as a cumulative enterprise, see Suppe (1974b:6-56). Reichenbach 1951 may be considered a typical representative of this approach. For linguistics viewed in this way, see Bloomfield 1946. This concept has been criticized by Hanson 1958, by Shapere 1966, and by Suppe (1974b:57-118), among others.

² Page references are normally Kuhn 1970a. On the many meanings of the word 'paradigm' in the earlier version of the theory, see Masterman 1970.

texts' (187). They are what Kuhn originally had in mind when he chose the term 'paradigm' to refer to what he now calls 'disciplinary matrices'.³

Paradigms are related to revolutions in the following way: each scientific revolution corresponds to a paradigm, and vice versa. Thus a revolution uniquely determines the character of the paradigm which is adopted in its aftermath. The two concepts are, in fact, defined in terms of each other: 'Scientific revolutions', explains Kuhn, 'are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one' (92). However, there is an important difference between the two: whereas revolutions are precipitated by SINGLE individuals, paradigms are SOCIAL phenomena, namely belief systems shared by all the practitioners of a scientific discipline. In fact, Kuhn requires uniform assent on the part of the members of a scientific community as a necessary defining attribute of any genuine paradigm.

He observes, however, that this uniformity of belief is not achieved overnight. When a paradigm makes its first appearance in a period of revolutionary science, it is invariably resisted by the specialists on whose domain it encroaches. This hostility is understandable, since any new paradigm necessarily involves a profound change in the rules governing the practice of normal science. Kuhn also sees a deeper reason for this conflict: in his view, paradigms commit their supporters to a complete perceptual set together with an associated language, a sort of Whorfian world-view.⁴ From this it follows that, when one paradigm is giving way to another, the members of the scientific community in question are being called upon to undergo a fundamental perceptual and linguistic conversion. As Kuhn graphically expresses it, 'what were ducks in the scientist's world before the revolution are rabbits afterwards' (111). Inevitably, some of the adherents of the older paradigm fail to experience the gestalt switch, and no amount of logical argument will persuade them to change their position. Thus the proponents of rival paradigms speak from incommensurable viewpoints; even the words they use have different meanings. 'How can they even hope to talk together,' says Kuhn (200), 'much less to be persuasive.' It may be noted that this position has aroused much adverse criticism from philosophers of science.⁵

One other important ingredient in Kuhn's system is a notion which I shall call the FIRST PARADIGM. He hypothesizes that every scientific discipline has a chronologically earliest paradigm; in other words, there is a definite point in time when a field achieves SCIENTIFIC MATURITY, to use Kuhn's expression, and it does so by acquiring its first paradigm. Prior to that event, it is a jumble of competing schools of thought; and since the practitioners lack a universally shared framework to

³ See Kuhn (1970a:182, 1974:463). This shift was in part an attempt to remedy the vagueness of the original term, in response to Masterman's criticism. However, Shapere (1971:707) views Kuhn's decision to break down the original paradigm concept into four loosely connected components as a significant retreat from his earlier position. Insofar as Kuhn fails to indicate the relation among these components, I find this criticism fully justified. One has every right to ask, e.g., whether in paradigm change all four components must change—or if not, how many.

⁴ Kuhn is familiar with and obviously approves of what he refers to as 'B. L. Whorf's speculations about the effect of language on world view' (vi).

⁵ See Scheffler's critique (1967:74-89), Kuhn's answer (1970b:267-70), and Scheffler's rejoinder (1972).

determine the direction of research, they waste their time and energy in random data-gathering and fruitless controversy about fundamentals. As examples of fields in the pre-paradigm stage, Kuhn cites 17th-century chemistry and early 19th-century geology (48). As soon as a discipline has acquired its first paradigm, the chaotic disunity of the earlier stage disappears, 'apparently once and for all' (17). Some fields, however, never acquire a first paradigm, and consequently their practitioners remain locked in a chronic state of disagreement 'about the nature of legitimate scientific problems and methods' (viii). Kuhn has the social and behavioral sciences in mind here.⁶

So much for the notion 'first paradigm', and so much also for Kuhn's theory in general. Let us now turn our attention to the way linguists have applied these ideas to the history of linguistics. These applications have taken place in two main directions. On the one hand, a number of theoretical linguists have argued that the recent emergence of transformational generative grammar has conformed to the pattern of a Kuhnian revolution; on the other hand, some historians of linguistics have attempted to depict substantial portions of the Western grammatical tradition as a succession of paradigms.

Regarding generative grammar, it has been proposed that its initial impact in the late fifties and early sixties had all the hallmarks of a scientific revolution as described by Kuhn. One can, so the argument goes, point to an outstanding achievement on the part of a single individual, embodied in such works as Chomsky 1957 and 1965. This achievement attracted an increasingly large group of adherents away from the prevailing post-Bloomfieldian framework.

A number of writers (e.g., Searle 1974, Derwing 1973:12) have focused attention on the bitter exchanges between the partisans of generative grammar and members of the older generation 'who still cling resentfully to the great traditions, regarding Chomsky and his "epigones" as philistines and vulgarians' (Searle, 8). It is usual to conclude that these conflicts were of the kind which Kuhn depicts as occurring when a scientific revolution takes place. The acrimony and fruitlessness of such debates can be considered an indication that the opposing positions were semantically and experientially incommensurable—and that here, as elsewhere, the older generation failed to undergo the necessary conversion experience. The conclusion seems inescapable: as Matthews has expressed it (1974:216), linguistics in the late fifties and sixties 'underwent a Chomskyan revolution, and ever since we are supposed to be working within the new paradigm of generative grammar. If not, we are simply clinging to old ways.'

But the mere fact that linguistics has recently undergone a revolution does not prove that the triumphant theory is a Kuhnian paradigm, since Kuhn's position is that revolutions are shared by both scientific and non-scientific fields, but that paradigms are peculiar to the sciences.⁷ The decisive issue in the case of generative

⁶ Although Kuhn has to some extent relativized the concept of science, it is noteworthy that he holds some remarkably firm convictions about the difference between fields which have attained scientific maturity and those which have not. (For his view of the social sciences, see Kuhn 1970a: vii–viii.)

⁷ Kuhn admits (208) having borrowed the notion of science as a series of tradition-bound periods, interspersed with revolutionary breaks, from the history of the non-scientific fields:

grammar is not the manner of its initial impact, but rather the question whether it in fact possesses the defining attributes of a paradigm.

Turning our attention first to the INTERNAL components of generative grammar, we note the following: The rules of the base and transformational components of a generative grammar, being expressed in logical form, qualify as symbolic generalizations in Kuhn's sense of the term. An ontological model is provided by the notion that a natural language may be regarded as the set of strings generated by a grammar with certain well-defined mathematical properties (cf. Wall 1972:166 and Chap. 9.) Chomsky and his associates have a clearly defined value system: it places a premium on simplicity and generality, and de-emphasizes factual accuracy, comprehensiveness of coverage, and correct methods of approach. Finally, textbooks such as Langacker 1972 provide problem-solutions of the kind referred to by Kuhn as exemplars. So far, so good.

What causes trouble, however, is the sociological dimension of paradigms. Generative grammar does not command uniform assent among linguists all over the world: it is not a conceptual framework shared by all the members of the profession.⁸ There are two main problems in this regard. On one hand, many linguists continue to subscribe to theories other than generative grammar, e.g. stratificational grammar, systemic grammar, string analysis, various brands of European structuralism etc. (It may be noted in passing that some of these theories have appeared SINCE the advent of generative grammar; hence their supporters cannot be categorized as 'holdouts', to use one of Kuhn's terms.) On the other hand, the adherents of generative grammar themselves have split into a number of competing schools of thought over the past decade.⁹ Searle has suggested (p. 8) that a new generation of Young Turks is now offering the same kind of challenge to the transformational establishment as Chomsky did to the post-Bloomfieldians in 1957. But it is far from clear that Chomsky's critics have established a new paradigm: they do not constitute a unified school, and it is too early to predict whether their point of view is destined to be accepted by the whole profession.

From a Kuhnian standpoint, therefore, linguistics is in an ambiguous position at the present time: it is either a field which has so far failed to achieve scientific maturity and is still locked in the pre-paradigm stage of interschool rivalry, or else a field which (though scientific) intrinsically eludes analysis in terms of paradigms. It

'Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools.' The originality of Kuhn's approach, in his own eyes, consists in his having been able to show that these concepts also apply to the sciences, 'fields which had been widely thought to develop in a different way', namely in a purely cumulative fashion. But obviously the fact that, e.g., artistic styles undergo revolutionary change from time to time does not confer on them the status of scientific paradigms.

⁸ The same can be said of the earlier post-Bloomfieldian framework (cf. Hymes 1974b:10).

⁹ An indication of the theoretical fragmentation prevailing at the present time is provided by the following remark made recently by Darden (1974, last page), with regard to the proponents of natural phonology: 'In this, as in other aspects of natural phonology, the multitude of views can be taken as evidence that we have reached that happy state when no one can be sure that he knows anything—except that everyone else is wrong.'

follows that linguists who have tried to portray generative grammar as a Kuhnian paradigm have not understood the theory well enough to realize that it does not apply to contemporary linguistics—or that it applies at best vacuously, in that it relegates linguistics to the same non-scientific status as the social sciences and humanities.

Having concluded that applications of Kuhn's theory to present-day linguistics have been less than successful, I turn to the more distant past of linguistics. The standard view of the history of linguistics, as presented in Pedersen 1931, would seem relatively easy to restate in Kuhn's terminology. One might posit a pre-paradigm stage extending from classical antiquity until the end of the 18th century, characterized by non-scientific speculation and prescriptive grammar; thereafter a 'first paradigm' inaugurated at the beginning of the 19th century by the comparative philologists (Bopp, Rask, Grimm et al.); and finally a revolution and new paradigm dating from the mid-1870's, initiated by the Neogrammarians. Thus a paradigmatic analysis of 19th-century linguistics appears to be unproblematic. This would all seem to corroborate Kuhn's confident assertion: 'Despite occasional ambiguities, the paradigms of a mature scientific community can be determined with relative ease' (43).

However, a recently published anthology (Hymes 1974a), containing papers presented at two conferences on the history of linguistics, reveals more than an occasional ambiguity. The basic crux is this: historians of linguistics have for the first time become aware of the fact that the men who are depicted as revolutionizing the field at various times in the past had antecedents. Diderichsen, in a penetrating study of the background of Rask's theories, concludes (1974:301) that he merely 'applied the theories and methods of eighteenth-century philosophical etymology and grammar to all the main languages of Europe, Indo-European as well as others', and that 'the philosophical grammar of the eighteenth century, by integration and extension of some ancient ideas, worked out a "paradigm" of research, in Kuhn's terms, such that continuous scientific progress could be made by "problem-solving".' To put the matter in blunt Kuhnian language, Rask did no more than mop up after the encyclopédiste Turgot.¹⁰ Malkiel comes to a similar conclusion regarding Diez's contribution to early Romance linguistics, stating firmly (1974:322) that 'there failed to appear in Diez's *oeuvre* any clear-cut break with the earlier tradition.'

Thus the comforting discontinuities of the orthodox picture are gone forever; and the result is a perspective difficult, if not impossible, to analyze in Kuhnian terms.¹¹ Indeed, we constantly face a problem for which Kuhn's theory provides no solution whatever, namely how to distinguish a new paradigm from a variant articulation

¹⁰ Turgot contributed the entry entitled 'Étymologie' for the *Encyclopédie* edited by Diderot and d'Alembert. The volume in which it appeared was first published in 1756.

¹¹ Here we should also consider the problem, essential to any genuine Kuhnian analysis, of identifying the two lone innovators responsible for early 19th-century comparative linguistics and Neogrammarian doctrine, respectively. One might feel inclined to castigate historians of linguistics for not having faced the issue in their discussion of Kuhn's theory, if it were not for the fact that the question is unanswerable. E.g., several eminent scholars were instrumental in establishing comparative linguistics at the beginning of the 19th century; it is futile to ask which of them was the lone innovator.

of an old one.¹² In the absence of a generally applicable yardstick, deciding whether Rask created his own paradigm or elaborated an earlier one is largely a matter of subjective judgment. For example, Greene (1974:498) rejects Diderichsen's view on the grounds that 'since Turgot did not demonstrate the power of the method in actual research, he cannot be said to have founded the new discipline.' The same question has been raised with regard to the Neogrammarians. Diderichsen (302) does not see their work as leading to 'anything that could be called a revolution.' On the other hand, Kiparsky 1974 posits a fundamental opposition between the approach of Bopp's generation and that of the linguists who came to the fore in the 1860's and 1870's.¹³ Much ingenuity could likewise be displayed in arguing whether the linguists of the Prague School adhered to the same paradigm as Saussure, or whether Bloomfield and Sapir represented the same or different paradigms. But it would be to no avail; such questions have no answers.

If it is easy, as Kuhn asserts, to identify the paradigms of a mature scientific community, then we must indeed conclude that, from its inception, modern linguistics has not been a science—or more precisely, that modern linguistics has not been one of Kuhn's 'mature sciences'. For since philosophers are not in agreement as to what constitutes a science,¹⁴ we are under no obligation to adopt Kuhn's solution to that problem. It should offer linguists some consolation to know that a historian of the biological sciences (Greene 1971:22–3) has denied that Kuhn's theory 'can be made to fit the development of natural history from John Ray to Charles Darwin', and argues that 'nothing approaching a "Darwinian" paradigm became established until the 1930's, and even that paradigm was Darwinian only in a very loose sense.' If linguistics falls outside the purview of Kuhn's theory, so do some fields which are commonly regarded as legitimately scientific.

As regards the adequacy of Kuhn's conception for the fields it was designed to illuminate (celestial mechanics, microphysics etc.), we may leave that issue to be decided by historians of the fields in question; historians of linguistics would be well advised to give it a wide berth. As Kuhn himself has emphasized (209), his

¹² This issue has been raised by Shapere (1964:386–7).

¹³ Kiparsky, however, departs from the customary view in suggesting (340) that the revolution was initiated a good decade before the Neogrammarians arrived on the scene, and that the new framework was accepted both by the Neogrammarians and by their opponents. It may be pointed out in addition that Kiparsky's conception of the development of linguistics also deviates from the Kuhnian approach in that he views the contrast between Bopp's generation and the linguists of the latter part of the 19th century as 'a reflection of the two basic reasons why man has always found the study of language so fascinating: on the one hand the elusive relationship between form and meaning, and on the other hand the intricate but perhaps slightly less elusive intrinsic patterning of linguistic forms' (343). In a similar way, Chomsky has viewed the history of linguistics as alternating between two basic approaches, one rationalist and the other empiricist (1966:72–3). But there is no provision in Kuhn's framework for perennial oppositions: each new paradigm is genuinely novel. There is, of course, nothing inherently implausible in Kiparsky's and Chomsky's notion: theoretical components not uncommonly make repeated appearances in the same or in a different discipline. (In this connection, see Holton 1973.) At the same time it is perilously easy to categorize as perennial an opposition which happens to be crucial to one's own immediate theoretical interests. Clearly, one must exercise caution and sophistication in identifying such themes, and guard against the temptation to assume 'that earlier work is to be assessed as answers to one's own questions', as Hymes has put it (1974b:14).

¹⁴ Cf. Gallie 1957, Harrah 1959, and Westland 1972, for example.

book was intended to prove that 'though scientific development may resemble that in other fields more closely than has often been supposed, it is also strikingly different'. It is precisely the possession of genuine paradigms and 'the relative scarcity of competing schools' which distinguish the sciences from the non-sciences, according to Kuhn. To the extent that this distinction is a valid one, the adequacy of the theory depends on the appropriateness of this characterization to the fields he singles out for attention.

It is perhaps ironic to state the issue in the form of a paradox, but Kuhn's theory can apply to linguistics only if it is untrue, i.e. only if his basic distinction between the sciences and the non-sciences is invalid. On the other hand, even if this distinction is invalid—and we may have good reasons for believing that such is the case—it does not follow that the theory applies to linguistics. For as we have seen, it fares badly, no matter where in the history of linguistics we attempt to apply it. This is not to say, of course, that nothing of Kuhn's message is worth salvaging. The notion of scientific disciplines proceeding in an exclusively cumulative fashion is by now discredited, and Kuhn's monograph has made a significant contribution to the debate.¹⁵ But having accepted the idea that all disciplines, both scientific and non-scientific, are subject to periodic revolutions, we do not have to believe in the existence of Kuhn's paradigms. On the contrary, investigation of the past of linguistics will only benefit if the search for paradigms is abandoned.

Practicing linguists will also be better off if they do not regard their own activities from a Kuhnian vantage point. Since linguistics has never been characterized by the uniform assent which Kuhn sees as the distinctive attribute of the hard sciences, an unhealthy situation might arise if linguists began to look upon all theoretical disagreements within their profession as conflicts between rival paradigms, i.e. incommensurable viewpoints, and used this as an excuse not to observe the ground rules of rational discussion. Moreover, since (according to Kuhn) any genuine paradigm is destined inevitably to be accepted by the entire profession, some linguists might feel impelled to give premature assent to any novel theory which they observed gaining wide support, for fear of ending up as isolated adherents of a discarded paradigm. Uncritical acceptance of Kuhn's theory of scientific revolutions could thus lead to a lowering, rather than a raising, of scientific standards within linguistics.

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¹⁵ Other books have also played an important role, e.g. Hanson 1958 and Toulmin 1961. For a general discussion, see Suppe 1974b.

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