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The Problem of Conceptual Change in the Philosophy and History of Science

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INTRODUCTION

The problem of conceptual change has been widely discussed in the philosophy of science since the early 1960s, when Thomas Kuhn and Paul Feyerabend, among others, launched a powerful critique against logical positivism, a critique based on a close reading of the history of the physical sciences. One of their most far-reaching theses was that scientific concepts are historical entities that evolve over time and are replaced by altogether different ones. In their view, the older concepts and their descendants refer to completely different entities. The very subject matter of scientific investigation shifts along with conceptual change. Furthermore, because of such ontological shifts, the possibility of giving an account of theory change as a rational process is undermined. In post-Kuhnian philosophy of science, there has been considerable effort to come to terms with those ontological and epistemological implications of conceptual change.

In this chapter, we give an overview of the problem of conceptual change in 20th century philosophy of science. We start with the logical positivist analysis of scientific concepts. Then, we discuss the "historicist" view of concepts and conceptual change, as expounded in the early writings of Kuhn and Feyerabend. The following sections examine the reception of historicist views by the philosophical community, focusing on the writings of D. Shapere, I. Scheffler, D. Davidson, and H. Putnam. Then, we analyze Kuhn's more recent work, in which he attempted to address some of the difficulties faced by his original account of conceptual change, by articulating further his key philosophical notion of incommensurability. We conclude by discussing a post-Kuhnian approach to conceptual change, which aims at a rapprochement of history and philosophy of science, on the one hand, and cognitive science, on the other.

THE LOGICAL POSITIVIST ANALYSIS OF CONCEPTS

The logical positivists in their manifesto entitled "Wissenschaftliche Weltauffassung: Die Wiener Kreis" ("The Scientific Conception of the World: The Vienna Circle"; Hahn, Carnap, & Neurath, 1996), published for the first time in 1929, specify the goal of their scientific outlook: "unified science".

> The endeavor is to link and harmonize the achievements of individual investigators in their various fields of science. From this aim follows the emphasis on *collective efforts*, and also the emphasis on what can be grasped intersubjectively; from this springs the search for a neutral system of formulae, for a symbolism freed from the slag of historical languages; and also the search for a total system of concepts. (Hahn et al., 1996, p. 328)

What is interesting in this passage for our purposes is the reference to "a neutral system of formulae" and "a total system of concepts". Logical positivists aimed at reaching the goal of unified science[1], by ordering all concepts into a reductive system, what they called a 'constitutive system'.[2] The idea was that any concept, from any branch of science, had to be "statable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the given" (ibid., p. 331). On this lowest level there were supposed to lie concepts of "the experience and qualities of the individual psyche", on the next, physical objects, then, other minds and, lastly, the objects of social science (ibid.). The philosophers belonging to the movement of logical positivism never succeeded in executing this very ambitious project; they knew it was difficult but they were optimistic that the advance of science would offer the means to carry it out. The method logical positivists used to move in and deal with this hierarchical system of concepts was logical analysis, undertaken with the help of the, then modern, symbolic logic, i.e., the formal logic developed by Frege and Russell at the turn of the 20th century. This tool allowed logical positivists to formulate statements in a first-order formal language, that of propositional or predicate logic, which gave them the rigor, clarity and precision they required. The aim was to move from statement to statement by tautological transformations representing, thus, thought and inference as a mechanical, automatically controlled process (ibid., pp. 330, 331). The reason behind all this was the determination to keep metaphysics out of the scientific world-conception (ibid., p. 329). They wanted to cancel out the "metaphysical aberration" (ibid.), to remove "the metaphysical and theological debris of millennia" (ibid., p. 339), to free concepts from "metaphysical admixtures which had clung to them from ancient time" (ibid., p. 334). They were suspicious of metaphysics in general but they were mostly concerned with the metaphysical philosophy of their day as it was expressed in the work of Heidegger, Bergson, Fichte, Hegel, and Schelling (Carnap, 1959, p. 80). So, the project, which had an ultimate political objective of liberation and enlightenment, consisted in manipulating an ordered system of concepts, arranged in statements, by means of a language that contained only structural formulae (Hahn et al., 1996, pp. 331–332). The emphasis on mere structure, mere form, and not content was thought to guarantee intersubjectivity and unity.[3] Let us see in more detail how they understood concepts and how they dealt with them.

Carnap in his essay "Logical Foundations of the Unity of Science" (1981), first published in the *International Encyclopedia of Unified Science* (1938), assigns to the theory of science the study, in the abstract,[4] of the linguistic expressions of science (p. 113). These expressions form statements, which form, in turn, ordered systems, the theories. The task of the philosopher is to study the relations between statements. For instance, when philosophers discuss the problem of confirmation — how scientific theories are confirmed by evidence — they consider the relations between observation statements and statements which express scientific hypotheses. When they discuss explanation — how scientists explain, for example, individual phenomena — they consider the relations between the *explanans* and the *explanandum*, that is, the relations between statements which express general laws and initial conditions on the one hand (the explanans), and a statement expressing the particular event to be explained on the other.

Each statement has as components terms which may express concepts. Carnap says that he prefers 'terms' to 'concepts' because he fears that 'concepts' may be understood psychologically:

> Instead of the word 'term' the word 'concept' could be taken, which is more frequently used by logicians. But the word 'term' is more clear, since it shows that we mean signs, e.g., words, expressions consisting of words, artificial symbols, etc., of course with the meaning they have in the language in question. We do not mean 'concept' in its psychological sense, i.e., images or thoughts somehow connected with a word; that would not belong to logic. (ibid., p. 118)

We see again in this passage the emphasis on the structural formulae of syntax (words, signs, and artificial symbols) and the rejection of any psychological accompaniments of language as not belonging to logic.

Frege, whose lectures Carnap had attended at Jena, was himself concerned that concepts may be understood psychologically[5] and urged the distinction between the psychological and the logical.[6] In his introduction to *The Foundations of Arithmetic* (1980b, p. x), he states it as the first of his fundamental principles: "always to separate sharply the psychological from the logical, the subjective from the objective". He developed a notation to capture what is logical in concepts — their conceptual content — that is, what is relevant to logical inference. This notation is Frege's *Begriffsschrift* (concept-script),[7] a "formula language of pure thought", modeled upon that of arithmetic, in which signs stand for concepts with sharp boundaries.[8] The psychological trappings, the clothing of thought, which may differ from language to language, were left out (Frege, 1979d, p. 142)[9] and the vagueness and ambiguity of natural languages were avoided.[10] With this tool, Frege undertook to provide a detailed analysis of the concepts of arithmetic and a foundation for its theorems (1980a, p. 8), aiming ultimately to show that mathematics can be reduced to logic. Frege hoped to extend the domain of this formal language to the fields of geometry and physics, both of which were considered to place value on the validity of proofs. He saw his notation as realizing Leibniz's "gigantic idea" of a *lingua characterica* (Frege, 1979a, pp. 9, 10, 13; also 1980a, p. 6),[11] and hoped that it would expand to comprise not only all existing symbolic languages (such as those of arithmetic, geometry and chemistry) but also new ones to be developed.

According to Frege, the meaning of terms cannot be sought in the ideas or images (*Vorstellungen*) that may be formed in the speakers' minds. These are private and subjective whereas meanings are objective in the sense of being independent and common to the speakers of language. A painter, a horseman, and a zoologist may connect different ideas with the term '*Bucephalus*'; yet, so far as they communicate, they all share the sign's sense which belongs to mankind's "common store of thoughts which is transmitted from one generation to another" (Frege, 1997a, p. 154). Frege, in his "Über Sinn und Bedeutung" (1997a), first published in 1892, distinguished between sense (*Sinn*) and meaning[12] (*Bedeutung*) and placed sense in a Platonic realm of abstract, timeless, unvarying entities, a third realm, which is different from the realm of physical things in the external world and the realm of mental objects of consciousness in the inner world (Frege, 1997c, pp. 336–337; 1979d, p. 148). Meaning or reference was sought in the world of objects. In general, one might say that the sense of a linguistic expression is a description of conditions that have to be met in order for the expression's meaning or reference to be determined. Sense, that is, helps to pick out objects in the world by describing them in a certain way. For example, the sense of the name 'Bucephalus' is "The horse of Alexander the Great" while its meaning or referent is the actual horse that belonged to Alexander.

Carnap compared the Fregean distinction between sense and meaning (sense and nominatum in his translation) to his distinction between the intension and extension of concepts (Carnap, 1988, pp. 118–133). Following Church's reading of Frege,[13] he thought that the two pairs coincide.[14] The sense or intension of a concept-word or predicate is a concept described by a conjunction of properties, while the meaning or extension of the concept-word is the class of objects that fall under it. For example, the intension of the concept-word 'horse' is "a four legged, solid-hoofed animal with a flowing mane and tail" and its referent is the concept's extension, i.e., the set of horses in the world.[15] Carnap claimed that the determination of intensions (as well as of extensions) is an empirical matter carried out by science, which achieves increasing precision and clarity in the description of properties some of which are selected as essential (Carnap, 1988, p. 241). Carnap's ultimate aim, as described at least in the *Aufbau* (1969, pp. v–viii), and here again it can be said that he follows Frege, was to achieve a rational reconstruction of the concepts of all fields of knowledge with the help of the formalism and logic that had been developed by Frege, but also Russell and Whitehead.[16] This task, which was shared, as we have seen, by other members of the Vienna Circle, involved the explication of concepts, i.e., their clarification in the direction of greater exactness through their reduction, originally to basic concepts of sensory experience (for instance, sense data of the form "a red of a certain type at a certain visual field place at a certain time"), and later, in order to ensure a greater inter-subjective agreement, through reduction to a physical basis, which contained as basic concepts observable properties and relations of physical things. It should be noted, however, that Carnap's logical reconstruction was not at all concerned with the actual concepts and the empirical work of the sciences. Carnap, just like the rest of the logical positivists, concentrated on logical form and structure to ensure inter-subjective objectivity (Friedman, 1999, pp. 95–101). His concepts are mere knots in a system of structural relations and have only formal, structural properties.

The different executions of this project formed the so-called received (Putnam 1975a), orthodox (Feigl, 1970) or standard view (Hempel, 1970) of scientific theories conceived as an axiomatic, hypothetico-deductive, empirically uninterpreted calculus, which was then interpreted observationally by means of bridge principles or correspondence rules[17]. The idea was that there is a "theoretical scenario" involving laws, theoretical concepts and entities, which is brought to apply, by bridge principles, to the empirical phenomena it is supposed to explain. For example, the kinetic theory of gases with its assumptions about gas molecules and the laws that govern them serves as the theoretical scenario to explain phenomena such as temperature and pressure using bridge principles which state, for instance, that the temperature of a gas (empirical concept) is proportional to the mean kinetic energy of its molecules (theoretical concept) (Hempel, 1970). Feigl (1970, p. 6) graphically illustrates this model (see Figure 13.1).

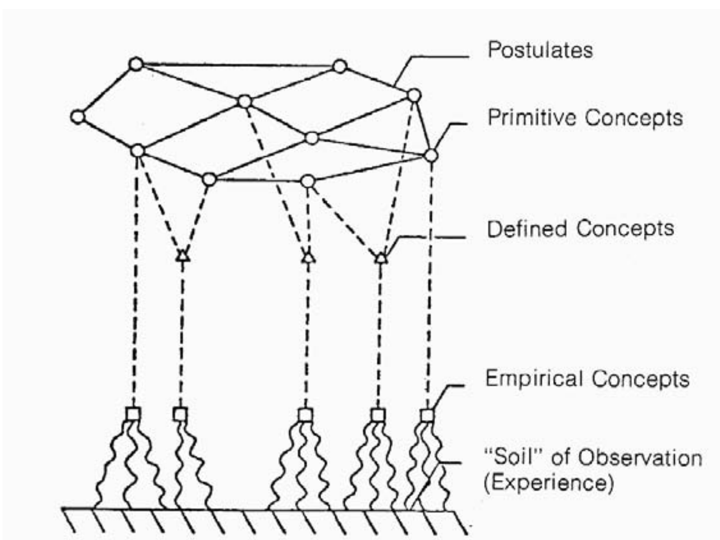

FIGURE 13.1 Feigl's diagram of theories.

In Figure 13.1, the uninterpreted theoretical scenario is a system of abstract postulates, which, while hovering over empirical facts, is linked to experience by means of correspondence rules. The primitive concepts in the theoretical scenario are implicitly defined[18] by the set of postulates that contain them and are used to explicitly define other concepts. As Feigl pointedly observes (1970, p. 5), "[c]oncepts thus defined are devoid of empirical content. One may well hesitate to speak of 'concepts' here, since strictly speaking, even 'logical' meaning as understood by Frege and Russell is absent. Any postulate system if taken (as erstwhile) *empirically uninterpreted* merely establishes a network of symbols. The symbols are to be manipulated according to preassigned formation and transformation rules and their 'meanings' are, if one can speak of meanings here at all, purely formal." The idea is that the validity of inferences could be checked automatically, by electronic computers (Feigl, 1970, p. 4).

We see, then, that in the context of logical positivism, whose reign lasted for more than 30 years and shaped what is still known as philosophy of science, there is no substantive account of concepts. Carnap even goes as far as stating that 'object' understood in its widest sense and 'concept' are one and the same in the metaphysically[19] neutral language that he employs (Carnap, 1969, p. 10). What mattered to Carnap and the logical positivists was to be able to manipulate mechanically signs in the effort to bring to the study of science, and, thus, to philosophy, the rigor and clarity of mathematics and logic. Their concerns were purely logical (i.e., the logical relations among statements and terms); the actual theories and concepts of science were only the occasion to exercise their logical insights. They had no ambition to influence the practice of scientists. "It should be stressed and not merely bashfully admitted", says Feigl, "that the rational reconstruction of theories is a highly artificial hindsight operation which has little to do with the work of the creative scientist" (Feigl, 1970, p. 13). And Carl Hempel (1970, p. 148) notes that "the standard construal was never claimed to provide a descriptive account of the actual formulation and use of theories by scientists in the on going process of scientific inquiry".

Conceptual change was far from their priorities, if non-existent as a matter of concern. The standard account of theories "could at best represent a theory quick frozen, as it were, at one momentary stage of what is in fact a continually developing system of ideas" (ibid., p. 148). Responding to the criticism by Feyerabend and Kuhn, which we will consider later on in this chapter, Hempel (1970, pp. 153–155), as late as the 1970s, considers the question of change in the meaning of terms. He challenges the operationist idea that different methods of measurement indicate different concepts and inquires whether in theory change what we have is change in the meaning of terms or just a revision of the laws in which the terms appear.[20] Do we have new concepts in the new theory or are the concepts of the old used to express new laws which prove the previously held laws false? "[A] satisfactory resolution of the issue would require a more adequate theory of the notion of sameness of meaning than seems yet to be at hand" (ibid., pp. 155–156).

THE "HISTORICIST" VIEW OF CONCEPTS AND CONCEPTUAL CHANGE

The issue of conceptual change, which was understood as change in the meaning of theoretical terms in the sciences, became the focus of attention in the early 1960s with the work most notably of Paul Feyerabend and Thomas Kuhn. The novel idea which these two philosophers introduced was that theory change in the sciences may involve change in the meaning of terms, in which case there is radical discontinuity in the development of scientific knowledge. The logical positivists, and those who were working in the tradition of their philosophy, did not deny that in the course of scientific development theories evolve, are modified or abandoned. They did not deny that new theoretical terms and new concepts are introduced with the advent of a new theory. But they did deny, at least implicitly, that these developments effect radical discontinuity and interfere with the meanings of the terms that are retained. Carnap, for instance, in his important paper "Testability and Meaning" (1936–37, pp. 441–453) explains what is to be done if a new term is to be introduced in the language of science[21]: it has either to be defined in terms of antecedently available vocabulary, if we want to fix its meaning once and for all, or, if we want to fix its meaning now for certain cases and wait for fuller determination in the future, its meaning has to be determined by pairs of reduction sentences, i.e., sentences which describe experimental conditions that have to be fulfilled in order for the new term to apply. Nowhere in both alternatives does Carnap ever consider the possibility of discontinuity which would have implied that it may not be possible to determine the meaning of the new term by reducing it to concepts already available. The reason this possibility was not even imaginable may be sought in the confidence that logical positivists had that the observational and physicalist language which they had placed at the bottom of their formulation of theories could guarantee continuity and intersubjective validity.[22] They had an entity idea of meaning and thought that there is a steady influx of neutral empirical content from the observation level to the abstract level of theory and into the empty syntactical shells of the theoretical terms of science. For instance, the abstract, theoretical predicate Q acquires meaning by being connected to certain observation predicates, which belong to sentences that are easily decided as to their truth or falsity with the help of some observations.[23] Some of these sentences, which may be added to or modified, constitute the necessary and sufficient conditions that have to obtain in order for the predicate to apply and the corresponding concept to be circumscribed.[24]

Both Kuhn and Feyerabend had a different idea of meaning drawing upon Wittgenstein's later philosophy. Feyerabend, in his seminal paper "Explanation, Reduction and Empiricism" (1981a, p. 24), which was first published in 1962, explicitly invoked Wittgenstein when he spoke of his contextual theory of meaning while Kuhn in his landmark book *The Structure of Scientific Revolutions* (1970, p. 45), which was also published for the first time in 1962, referred to Wittgenstein in connection with the notion of family resemblance.

Wittgenstein spoke of meaning as use rejecting an entity idea of meaning (the entity being a mental image, a referent in the world of objects or some kind of an abstract Platonic form). The meaning of a word is its use in language with its rules and grammar. To know the meaning of a word or, equivalently, to have the corresponding concept, is to be able to use it appropriately. But the appropriate use is not given by some definition, which must be antecedently available, comprising a set of necessary and sufficient conditions which specify when and how the word ought to be employed, but rather the appropriate use is learned in practice when the users of language are exposed to concrete examples of application. The uses of a particular word on different occasions and in different contexts, the different "facets" of the use (PG §36), bear similarities to each other, "overlapping and criss-crossing", and form a family of resemblances (PI §§66–67). Yet, no common thread runs through them all, and, so, no set of conditions can be used to fully define and bound a concept.[25] As a result, concepts for Wittgenstein are normally vague (RFM VII §70), fluid (PG §65), hazy (PG §76), fluctuating (PG §36), with blurred edges (PG §§71).[26] This does not mean, however, that they cannot be given sharp boundaries for specific purposes (PI §68–69) or that this rather loose understanding of concepts renders them useless or precludes the possibility of correct use (cf. PG §76, RFM I §116). For one, as Wittgenstein says, there is no single idea of exactness (PI §88); what counts as exact or inexact depends on what we are trying to do.[27] Also, it is not always an advantage to replace an indistinct picture by a sharp one.[28] Second, the fact that our concepts are not bounded by sharp definitional contours, that "a transition can be made from anything to anything" (PG §35), does not mean that the application of concepts is a matter of arbitrary decision, and it does not mean that there is no point in talking about correct use. It is true that Wittgenstein, in several places in his work, says that it is a matter of decision or stipulation whether a particular employment of a concept-word falls under the concept.[29] But nowhere does he say that it is a matter of *arbitrary* decision. Our concepts may not be bounded for us "by an arbitrary definition"[30] but are bounded by "natural limits" which correspond to whatever pertains to the role our concepts have in our life (RFM I § 116). Taking one or the other decision in border line cases has practical consequences which may get us into conflict with society or other priorities that we may have (ibid.). If, for instance, we extend the concept 'number', or the concept 'red', in unanticipated ways we need to give reasons and we need to consider the ramifications this move may have in a whole lot of activities. Wittgenstein's point seems to be that using language is not a matter of theoretically contemplating definitions which fix from without the determinate sense of words and dictate the correct course of action but a matter of practice which requires, among other things, to take responsibility for the judgments we make, which means in its turn to give reasons for what we do. This implies that what is correct to say or do is not determined by some standards set independently of the practices of human beings but rather by what these practices' priorities and goals are.[31]

Feyerabend was well acquainted with Wittgenstein's philosophy[32] and used it in attacking the formal theories of explanation and reduction in the sciences. Ernest Nagel (1979) had maintained that "the distinctive aim of the scientific enterprise is to provide systematic and responsibly supported explanations" (1979, p. 15)[33] and Carl Hempel, together with Paul Oppenheim (1948), provided the best known formulation of them: the deductive-nomological model. According to this model, an explanation consists of two parts: the explanandum (what is to be explained) and the explanans (what is used to do the explaining). The explanandum is a sentence describing either the occurrence of some individual event or the possession of some property by an object or some general regularity or law while the explanans is a set of two groups of sentences: one group contains sentences which represent universal laws and the other contains sentences which state antecedent conditions, i.e., singular statements which assert that certain events have occurred at indicated times and places or that given objects have certain properties (Nagel, 1979, p. 31).[34] Explanations have a deductive structure which means that the explanandum is a logical consequence of the explanans.

The reduction of individual theories to more inclusive ones is, according to Nagel, "an undeniable and recurrent feature of the history of modern science" in view of realizing "the ideal of a comprehensive theory which will integrate all domains of natural science" (Nagel, 1979, pp. 336–337). Reductions, just like explanations, have also a deductive structure. The laws and theories to be reduced (secondary science), for instance Newtonian mechanics, must be a logical consequence of the theory to which the reduction is made (primary science), for instance, Einstein's special and general theories of relativity. An obvious and indispensable requirement, says Nagel, is that the terms appearing in the statements which represent axioms, hypotheses or laws "[must] have meanings unambiguously fixed by codified rules of usage or by established procedures appropriate to each discipline" (ibid., p. 345). Otherwise the reduction cannot go through. Nagel acknowledges though, that, by this condition, it is possible that certain terms in the secondary science may be absent from the theoretical assumptions of the primary science. So, he states his condition of connectability, which requires that, in order for a reduction to proceed, some postulates need to be introduced to establish relations between the terms and expressions of the two sciences. These linkages may be logical connections, conventions created by fiat or factual hypotheses which need to be supported by evidence (ibid., pp. 353–355). With all this in place one theory can be derived from the other. The models of both explanation and reduction provide also a model for the progress of science. Science develops and knowledge is increased by constantly producing all the more comprehensive theories with enlarged and improved explanatory power.

In this context, conceptual change is just the continuous accumulation of better means (concepts, hypotheses, laws, theories) to reach the truth about the world.

Feyerabend (1981a) challenged both the deductive structure of explanation and reduction (on historical and logical grounds)[35] and the assumption it implies of meaning invariance, i.e., the assumption that meanings are invariant with respect to the processes of explanation and reduction. He maintained that the meaning of a term is given contextually,[36] i.e., it "is dependent upon the way in which the term has been incorporated into a theory" (ibid., p. 74) and claimed that elements of many pairs of theories (concepts, principles, laws, etc) are "incommensurable and therefore incapable of mutual explanation and reduction" (ibid., p. 77). The reason is that concepts of an earlier theory cannot be defined on the basis of primitive observational terms of the theory to which a reduction is attempted nor can there be found "correct empirical statements" to correlate corresponding terms and concepts (ibid., p. 76). Observation, and this is yet another influence of Wittgenstein's philosophy, was thought by Feyerabend, but also Hanson[37] and Kuhn, to be theory-laden, that is influenced and shaped by the categories and concepts of each theory. Feyerabend believed that the progress of science requires radical steps forward so, if the meanings of terms are preserved as science develops, the much desired revolutions in the interest of knowledge will not occur. Thus, meaning invariance, according to Feyerabend, is not only incompatible with actual scientific practice but also undesirable (1981a, p. 82). Finally, in an article that was first published in 1965 (1981b, p. 98) Feyerabend maintains that there is a change of meaning "either if a new theory entails that all concepts of the preceding theory have zero extension or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself". This is a view that, as we will see, features prominently in Kuhn's later philosophy.

At least the initial phase of Feyerabend's criticism may be considered internal to the philosophical tradition he was combating in the sense that it focused mostly on the philosophical shortcomings and inadequacies of the models that were put forward at the time. Toulmin (1961, 1972) and Kuhn (1970), more so than Feyerabend, attempted and effected a change of perspective. Toulmin questioned whether there is anything in common to the different explanatory sciences, comparing them to Wittgenstein's games (1961, p. 22), and urged for a more relevant philosophy of science, more relevant, that is, to its actual practice and history. He thought that what he called "Frege's method", which concentrated on idealized logical structures to escape from "the twin heresies of 'psychologism' and the 'genetic fallacy'" (1972, p. 58), "distracts us not only from the process of conceptual change, but also of questions about the external application of conceptual systems, as put to practical use" (ibid., p. 61).[38] Yet, although Toulmin was among the first to call attention to the evolution of science and its concepts, although he spoke of paradigms (borrowed from Wittgenstein's philosophy) and of profound change involving no common theoretical terms and problems (1961),[39] he was very much reluctant to endorse radical discontinuity in science. He contended that there is conceptual change without revolutions and that "intellectual discontinuities on the theoretical level of science conceal underlying continuities at a deeper, methodological level" (1972, pp. 105–106).

The figure most responsible for the change of perspective and the so-called historicist turn in philosophy of science in the 1960s was Thomas Kuhn. He laid emphasis on revolutions and radical conceptual change in science and acknowledged the implications of these claims. Kuhn held a PhD in physics from Harvard University but began his professional career as a historian of science teaching a General Education in Science course designed by the then president of Harvard, James Bryant Conant. His close reading of historical texts in this connection alerted him to the irreducible variability of scientific concepts and led him to question the idealized image of science which assimilated all possible theories to a standard model. In the first page of his celebrated book, *The Structure of Scientific Revolutions* (1970), he states his aim of sketching "[a] quite different concept of science that can emerge from the historical record of the research activity itself". By studying the details of particular historical cases Kuhn came to appreciate the significance of scientific education. He realized that what binds scientists together in a specific tradition, what gives their practice its character and coherence, is not a neutral, ubiquitous, formal description of theories which is based on some inter-subjectively avowed observation sentences, but exposure of students to concrete problem solutions, the paradigms or exemplars,[40] which may vary and which function as models for further research.[41] The process of initiation and training involves doing, rather dogmatically, "finger exercises", that is, learning, usually through textbooks, and then imitating, particular applications of concepts, particular ways of dealing with problems, particular techniques of using instruments and doing research. The consensus and the effectiveness of science are not earned theoretically but practically. Scientists do not need to concern themselves with abstract, explicit definitions, comprising necessary and sufficient conditions, in order to know how to apply a term. Nor do they need to reduce their practice to a set of abstract rules which capture what is essential in their field. Scientific education provides scientists with the ability, rather than the abstract knowledge of rules, to do successful research (1970, pp.43–51; cf. Kuhn, 1977).

Kuhn cites Wittgenstein and his idea of family resemblance to explain how different research problems and techniques are held together in a single tradition. They do not need to share a set of characteristics but are related between them in a network of resemblances overlapping and criss-crossing. In the case of concepts, such as, *game*, *chair*, *leaf*, *planet*, *mass*, *motion*, etc., again, the idea is that one does not need to know a set of attributes, the necessary and sufficient conditions, in order to apply the corresponding term. Now, this understanding of concepts — as abilities to use terms, which is a long way from the entity account involving a seepage of meaning from an observational basis through correspondence rules to an uninterpreted calculus — implies that when there is a change in the exemplars used in teaching there is going to be a change of concepts or, what amounts to the same thing, a change in the meaning of terms.

The change of exemplars is not forced upon scientists by the world as such nor is it brought about de novo. Kuhn emphasizes the indispensability of commitment to the tradition built around previously upheld theories as a condition of innovation and change. This is what he calls the "essential tension" implicit in scientific research (1977, pp. 227, 236). There is, on the one hand, firm, or even dogmatic, adherence to deeply ingrained patterns of research, and, on the other, a constant pursuit of novel ideas and discoveries. Scientists are pulled in both directions: they are traditionalists and iconoclasts at the same time (ibid., p. 227). According to Kuhn, only if scientists are well acquainted with the problems to be tackled and the techniques appropriate for use, can they be able to spot and evaluate anomalies which may arise in the course of their research. "[N]ovelty ordinarily emerges only for the man who, knowing with precision what he should expect, is able to recognize that something has gone wrong" (1970, p. 65). Tradition-bound research is called by Kuhn normal science. In this mode, scientists undertake to solve puzzles, that is, problems very similar to the textbook paradigms, which have solutions that are anticipated,[42] trying more to prove their own ingenuity rather than shatter the tradition and start a new one. Yet, their practice, conservative as it may be, paves the way to change, both on a regular basis — in the course of normal science — but also in revolutionary moments. When anomalies persist, when they become central to the investigation and when they preclude applications which are of practical importance, scientists need to reconsider their strong commitments and inherited beliefs. They proceed, then, to reconstruct their field in an effort to assimilate new solutions and new theories. This is not always achieved in a smooth way and what usually emerges is not a cumulative result. "Contrary to a prevalent impression, most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge. To assimilate them the scientist must usually rearrange the intellectual and manipulative equipment he has previously relied upon, discarding some elements of his prior belief and practice while finding new significances in and new relationships between many others. Because the old must be revalued and reordered when assimilating the new, discovery and invention in the sciences are usually intrinsically revolutionary" (Kuhn, 1977, pp. 226–227).[43] Revolutionary shifts, which Kuhn sees as displacements of the conceptual networks through which scientists view the world (1970, p. 102), are rather rare but, he also notes, that "the historian constantly encounters many far smaller but structurally similar revolutionary episodes [which] are central to scientific advance" (1977, p. 226). So, scientific practice is a dynamic, developmental process punctuated occasionally by radical changes which produce theories that are incommensurable with the previous ones. This means that the new theories cannot be mapped onto the old, new relations are established between concepts and laws and new exemplars occupy the knots in the new framework. The two systems, old and new, lack a common core or a common measure.[44] Concepts in the new context, even when they continue to be named by the terms used in the previous theories, or even when there is quantitative agreement in calculations that involve them, still are viewed by Kuhn to be markedly different. "[T]the physical referents of ... the Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einstenian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.)" (Kuhn, 1970, p. 102). The difference in the concepts of two successive theories lies in the different applications they have and the different connections they enter. There is no discussion of tracing a common meaning in some common observational content because terms in Kuhn's framework do not acquire meaning by being anchored to experience but by being taught in practice in specific applications.

EARLY RECEPTION OF THE HISTORICIST ACCOUNT OF CONCEPTUAL CHANGE

Feyerabend's and Kuhn's theses on conceptual change and incommensurability were received rather unfavorably by philosophers of science.[45] The main thrust of the criticism concerned the unpalatable consequences of incommensurability. First, it was claimed that if the incommensurability thesis "were true, no theory could contradict another" (Achinstein, 1968, p. 92). This can be shown by means of a historical example. The quantum theory of the atom, proposed by Niels Bohr in 1913, was supposed to contradict classical electromagnetic theory. According to the former theory, electrons orbiting around the nucleus (i.e., undergoing accelerated motion) did not radiate, in violation of the laws of the latter theory. If the term "electron" meant different things in the context of the two theories, however, there would be no contradiction between the quantum and the classical descriptions. A second, related difficulty is that if conceptual change were as radical as portrayed by Feyerabend and Kuhn, then two incommensurable theories would not even have a common subject matter. For instance, if the concepts of Newtonian mechanics and the concepts of relativity theory referred to different entities, they could not be alternative accounts of the same domain. Third, if the principles of a theory constituted the meanings of its terms, then those principles would have to be analytic statements. That is, they would have to be devoid of empirical content. A fourth difficulty concerned the process of learning a scientific theory. If the meaning of theoretical terms were theory-dependent, then "a person could not learn a theory by having it explained to him using any words whose meanings he understands before he learns the theory" (ibid., p. 7).

The root of these difficulties seemed to be Feyerabend's and Kuhn's claim that scientific concepts (or, equivalently, the meanings of scientific terms) were determined by the theoretical framework in which they were embedded. Several critics stressed the obscurity of this claim and pointed out that it was not supported by a full-fledged theory of meaning. Dudley Shapere was among the sharpest early critics of Kuhn and Feyerabend. In his critical review of *The Structure of Scientific Revolutions*, Shapere argued that "Kuhn has offered us no clear analysis of 'meaning' or, more specifically, no criterion of change of meaning" (Shapere, 1964, p. 390). He made a similar point against Feyerabend: "We are given no way of deciding either what counts as a part of the 'meaning' of a term or what counts as a 'change of meaning' of a term" (Shapere, 1981, pp. 41–42). Furthermore, the theoretical context of a term, which supposedly determined its meaning, was not fully specified. It was unclear, for example, whether the metaphysical beliefs of the creator of a theory play a role in determining the meaning of its terms (ibid., p. 42).

A similar problem was pointed out by Hilary Putnam. If the meaning of a term depends on its theoretical context, then are there parts of the context that may change without affecting the term's meaning (Putnam, 1975e, pp. 124–125)? Conversely, would every kind of theory change affect the meaning of its terms? Feyerabend acknowledged that certain minor variations of a theory would not impinge on the meanings of its terms. For example, the meaning of "force" would not change if we moved from classical mechanics to a similar theory that differs from classical mechanics only with respect to "the strength of the gravitation potential" (Feyerabend, 1981, p. 97).

As we saw in the previous section, Feyerabend attempted to respond to this difficulty by linking meaning with classification:

> a diagnosis of *stability of meaning* involves two elements. First, reference is made to rules according to which objects or events are collected into classes. We may say that such rules determine concepts or kinds of objects. Secondly, it is found that the changes brought about by a new point of view occur *within* the extension of these classes and, therefore, leave the concepts unchanged. Conversely, we shall diagnose a *change of meaning* either if a new theory entails that all concepts of the preceding theory have extension zero or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself. (ibid., p. 28)

However, this proposal also faced problems. First, for a new theory to entail "that all concepts of the preceding theory have extension zero", a common core of meaning must be shared by the two theories (Achinstein, 1968, p. 95; Shapere, 1981, p. 52). Second, Feyerabend's construal of meaning change as the outcome of classification change presupposes the absolute character of rules of classification. However, the existence of such rules in science is questionable. Scientific classifications often have a pragmatic character and they need not reflect the intrinsic properties of the entities classified. One may choose classificatory rules so as to make the existing system of classification immune to theory change (see Shapere, 1981, pp. 51–52).

A different line of attack against incommensurability focused on its incompatibility with the success of translation practices. Among the most prominent critics who followed this line were Donald Davidson and Hilary Putnam.[46] Davidson criticized incommensurability in a celebrated article where he denied the possibility of radically different systems of concepts or "conceptual schemes" (Davidson, 1984). He associated conceptual schemes with languages and claimed that two conceptual schemes could differ only if the languages that bear them were not intertranslatable. The difference of two conceptual schemes must, thus, have a linguistic manifestation: "two people have different conceptual schemes if they speak languages that fail of intertranslatability" (Davidson, 1984, p. 185). The impossibility of translation would preclude communication between two such speakers.

Furthermore, if there were texts written in a language incommensurable to our own, they would be impossible to translate and ipso facto to understand. The existence of such texts, however, is belied by the successful interpretive practice of historians, such as Kuhn himself. Kuhn has been able to decipher and convey the content of purportedly incommensurable concepts, found in past scientific texts, using the resources of contemporary language. The existence of incommensurability is called into question by that very interpretive success: "Kuhn is brilliant at saying what things were like before the revolution using — what else? — our post-revolutionary idiom" (ibid., p. 184). Thus, "Instead of living in different worlds, Kuhn's scientists may … be only words apart" (ibid., p. 189).

Along the same lines, Putnam maintained that if the incommensurability thesis "were really true then we could not translate other languages — or even past stages of our own language — at all. And if we cannot interpret organisms' noises at all, then we have no grounds for regarding them as *thinkers*, *speakers*, or even *persons*" (Putnam, 1981, p. 114). Furthermore, incommensurability is at odds with historical analysis: "To tell us that Galileo had 'incommensurable' notions *and then to go on to describe them at length* is really incoherent" (ibid., p. 115).[47]

THE FIRST RESPONSE TO INCOMMENSURABILITY: A FLIGHT TO REFERENCE

The first attempt to come to terms with the difficulties encountered by incommensurability by developing an alternative analysis of conceptual change was made by Israel Scheffler. Scheffler accepted the holistic account of meaning, which was shared by logical positivists and their historicist critics (Scheffler, 1982, pp. 45–46). He pointed out, however, an ambiguity of the word "meaning". On the one hand, the meaning of a word is "a matter of the concept or idea expressed", that is, it concerns "the connotation, intension, attribute, or sense associated with the word". On the other hand, the meaning of a word is "rather a matter of the thing *referred* to", that is, it concerns "the denotation, extension, application, or reference of the word" (ibid., pp. 54–55).

Scheffler argued "that, for the purposes of mathematics and science, it is sameness of reference that is of interest rather than synonymy [sameness of meaning], in accordance with the general principle that a truth about any object is equally true of it no matter how the object is designated." (ibid., p. 57). That is, a concept associated with a word may change without affecting the truth values of the statements containing the word, provided that its referent remains invariant. Furthermore, the stability of a term's referent makes possible a genuine disagreement between two users of the term who, nevertheless, associate different concepts with it. Finally, because of referential stability, conceptual change does not undermine the validity of scientific deductions. Thus, pace Feyerabend, Hempel's deductive-nomological model of explanation and Nagel's account of reduction qua deductive explanation remain applicable to actual cases of scientific explanation and scientific change (ibid., pp. 61–62).

Here it is important to point out that Scheffler did not rule out the possibility of referential variance. Rather his point was that conceptual change did not *necessarily* imply instability of reference. He did not spell out, however, his proposal to disentangle meaning and reference in terms of a developed theory of meaning. Such a theory, which would be developed by Hilary Putnam, would have to show how the reference of a term can be fixed without invoking the full concept associated with it.

MEETING THE HISTORICIST CHALLENGE: THE CAUSAL THEORY OF MEANING

The account of meaning that Putnam developed in the early 1970s was meant to be an alternative to both the logical positivist and the historicist views of meaning. In 1962 he had criticized the logical positivist distinction between observational and theoretical terms, arguing that it could not be explicated in terms of the distinction between observable and unobservable entities. Theoretical terms (e.g., "satellite") may refer to observable objects and observational terms (e.g., "particles too little to see") may refer to unobservable entities (Putnam, 1962, p. 218). Putnam's critique indicated that meaning should not be tied "too closely to the observable" (Ben-Menahem, 2005, p. 8). Furthermore, the historicist, contextual account of meaning was also problematic.[48] Historicists insisted on the theory-dependence of meaning. Furthermore, they stressed the ubiquitous presence of theory change in the historical development of the sciences. These two theses, along with the Fregean assumption that meaning determines reference, implied that the ontology of science has been in flux. For a scientific realist, as Putnam was at the time, that was an unacceptable consequence.

Putnam acknowledged "that meaning change and theory change cannot be sharply separated" (Putnam, 1975h, p. 255).[49] Furthermore, theories may not describe the world correctly and, therefore, "Meanings may not fit the world; and meaning change can be forced by empirical discoveries" (ibid., p. 256). Thus, the problem situation faced by Putnam was to come up with an account of meaning that would allow for meaning change, while neutralizing its *prima facie* relativist and anti-realist implications.

Putnam explicated the notion of meaning in terms of the notions of communication and teaching. "It is a fact ... that the use of words can be taught. If someone does not know the meaning of 'lemon', I can somehow convey it to him. … in this simple phenomenon lies the problem, and hence the *raison d'être*, of 'semantic theory'" (ibid., p. 147). The meaning of natural kind terms, including those put forward in scientific theories, is not specified by a set of necessary and sufficient conditions that govern its application. Thus, it cannot be conveyed by "an analytic definition (i.e., an analytically necessary and sufficient condition). ... *Theoretical terms* in science have no analytic definitions" (ibid., p. 146). Furthermore, the meaning of a natural kind word, say "tiger", does not involve "the totality of accepted scientific theory about tigers, or even the totality of what I believe about tigers" (ibid., p. 147). If that were the case, then it would be impossible to teach anyone the meaning of a natural kind term he or she does not know. Rather, to get to know the meaning of such a term one needs to learn certain "core facts" about a "normal member of the kind" (ibid., p. 148). This is not sufficient, however, for learning the meaning of the term in question. One needs, in addition, to become acquainted with the reference of the term, that is, with the actual entities denoted by the term.

Putnam conceded that the meaning of scientific terms is, partly, theory-dependent. He suggested, however, that the reference of those terms is fixed not by theoretical beliefs, but through our causal interaction with the world.[50] For example,

> No matter how much our theory of electrical charge may change, there is one element in the meaning of the term 'electrical charge' that has not changed in the last two hundred years, according to a realist, and that is the reference. 'Electrical charge' *refers to the same magnitude* even if our theory of that magnitude has changed drastically. And we can identify that magnitude in a way that is independent of all but the most violent theory change by, for example, singling it out as the magnitude which is causally responsible for certain effects. (Putnam 1975b, p. ix)

Thus, our causal interaction with electrical charges plays a double role: First, it fixes the reference of "electrical charge"; and, second, it renders the reference in question immune to any revision of our theoretical beliefs about electrical charges. In general, theory change does not affect the reference of natural kind terms, which are used "to refer to a thing which belongs to a natural kind which does *not* fit the 'theory' associated with the natural kind term, but which was believed to fit that theory ... when the theory had not yet been falsified" (Putnam, 1975f, p. 143).

Putnam allowed for the possibility "that a concept may contain elements which are not correct" (Putnam, 1975g, p. 196). However, if the progress of science leads to the rejection of certain elements of a concept, its extension need not be affected: "concepts which are not strictly speaking true of anything may yet refer to something; and concepts in different theories may refer to the same thing" (ibid., p. 197). Different speakers need not associate the same concept with a term, say "electricity", in order to refer to the same entity. What they should share is " that each of them is connected by a certain kind of causal chain to a situation in which a *description* of electricity is given, and generally a *causal* description – that is, one which singles out electricity as *the* physical magnitude *responsible* for certain effects in a certain way" (ibid., p. 200).

But what exactly is a concept? Putnam evaded the question and focused instead on what it means to *have a* concept. Following Wittgenstein, he suggested that possessing a concept amounts to having certain perceptual and linguistic abilities: "an organism possesses a *minimal concept* of a chair if it can recognize a chair when it sees one, and … it possesses a *full-blown concept* of a chair if it can employ the usual sentences containing the word *chair* in some natural language" (Putnam, 1975c, p. 3). Furthermore, "two people have the *same* concept … [when they have] the same set of linguistic and nonlinguistic abilities in a certain respect" (ibid., p. 8). Given that concept possession is largely a matter of linguistic skills, it follows "that a great deal of philosophy should be *reconstrued* as about language … In particular, all of the traditional philosophy about 'ideas', 'concepts', etc." (ibid., p. 9). It should, therefore, occasion no surprise that, in philosophy of science the problem of conceptual change has been approached, for the most part, through linguistic categories, such as meaning and reference. Putnam, for instance, explicitly identified meaning with "what it is to have a concept of something" (ibid., p. 16).

Nevertheless, according to Putnam, there is an important difference between concepts and meanings. The former are possessed by individuals, whereas the latter have a social character—they are possessed by a linguistic group (cf. Putnam, 1983, p. 75). Only certain people in a linguistic community, the relevant experts, have a mastery of the concepts associated with certain words. Only metallurgists, for instance, grasp fully the concept of gold. The meaning of 'gold', on the other hand, is possessed collectively by a whole community:

> everyone to whom gold is important for any reason has to *acquire* the word 'gold'; but he does not have to acquire the *method of recognizing* whether something is or is not gold. He can rely on a special subclass of speakers. The features that are generally thought to be present in connection with a general name — necessary and sufficient conditions for membership in the extension, etc. — are all present n the linguistic community *considered as a collective body*; but that collective body divides the "labor" of knowing and employing these various parts of the "meaning" of 'gold'. (Putnam, 1973, p. 705)

We think, however, that one still has the option to identify meanings with the concepts possessed by the relevant experts. This would imply that most of us have an inadequate mastery of the meaning of many of the words we use. But this is hardly a problem. There is no reason to pretend that a layman knows the meaning of a natural-kind term, when there are many situations in which he or she could not use this term correctly.

Moreover, Putnam argued strenuously that the concept we associate with a natural-kind term does not determine its reference. We have already seen that two speakers who associate different concepts with the same term may still refer to the same entity. Furthermore, two speakers may share the same concept and, nevertheless, refer to different things. Putnam exhibited this possibility by means of a thought experiment involving "twin earth", a fictitious planet that is identical with our earth in every respect save for the microscopic constitution of water. A speaker on earth and his counterpart on "twin earth" share the same concept of water (transparent, odorless, thirst-quenching liquid). They nevertheless refer to different things when they use the term "water". The speaker on earth refers to H_2O, whereas his counterpart refers to XYZ (ibid., pp. 701ff).

Natural-kind concepts do not determine the reference of the corresponding natural-kind terms because these terms have an "indexical" character. That is, they resemble words such as "I" or "this" whose reference depends on the spatial and temporal context in which they are used. The term "water", for instance, refers to "stuff that bears a certain similarity relation to the water *around here*" (ibid., p. 710).

Putnam developed a full-fledged account of meaning and reference in a paper entitled the "Meaning of 'Meaning'", where he articulated his insights about the indexical and social character of meaning and, especially, reference (Putnam, 1975h; cf. Floyd, 2005). To explicate the notion of meaning he introduced the notion of a stereotype associated with a natural kind, namely

> a standardized description of features of the kind that are typical, or 'normal', or at any rate stereotypical. The central features of the stereotype generally are *criteria* — features which in normal situations constitute ways of recognizing if a thing belongs to the kind or, at least, necessary conditions (or probabilistic necessary conditions) for membership in the kind. (Putnam, 1975h, p. 230)

Thus, possessing the main characteristics of the stereotype associated with a term is necessary for being able to use that term correctly. In actual scientific practice, however, those characteristics are not used as necessary and sufficient conditions. Rather they are employed as "*approximately* correct characterizations of some world of theory-independent entities" (ibid., p. 237). Furthermore, stereotypes are associated with "conventional ideas, which may be inaccurate. I am suggesting that just such a conventional idea is associated with 'tiger', with 'gold', etc., and, moreover, that this is the sole element of truth in the 'concept' theory" (ibid., p. 250). Recalling Putnam's discussion of concept possession, we may identify stereotypes with concepts. In his more recent work, Putnam suggested a similar view:

> I myself would regard possession of the *stereotype* — not the *theory* — that electrons are charged particles ("little balls" with trajectories and unit negative charge) as part of our concept of the electron. On my view, stereotypes are far more stable than theories, and contribute to the identity of our natural kind concepts *without* providing necessary and sufficient conditions for their correct applications. They cannot do the last because, in cases like this one, they are known to be "oversimplified". (Putnam, 1992, p. 445)

Thus, the deliberate simplification (or, we would add, idealization) of the stereotypic features is one more reason why these features cannot function as necessary and sufficient conditions.

Putnam made a further step away from the conception of meaning as a set of necessary and sufficient conditions. He disentangled meaning from analyticity. In traditional semantic theory the meaning of a natural kind term is specified by a set of features, whose attribution to the kind in question is a matter of analytic stipulation. The statement that members of the kind have these features is an analytic truth. Putnam, on the other hand, following Quine's well-known critique of the analytic-synthetic distinction (Quine, 1951), rejected the traditional view. Consider, for instance, the attribution of stripes to tigers:

> there is a perfectly good sense in which being striped is part of the meaning of 'tiger'. But it does not follow … that 'tigers are striped' is analytic. If a mutation occurred, all tigers might be albinos. Communication presupposes that I have a stereotype of tigers which includes stripes, and that you have a stereotype of tigers which includes stripes … But it does not presuppose that any particular stereotype be *correct*, or that the majority of our stereotypes remain correct forever. Linguistic obligatoriness is not supposed to be an index of unrevisability or even of truth; thus we can hold that 'tigers are striped' is part of the meaning of 'tiger' without being trapped in the problems of analyticity. (Putnam, 1975h, p. 256.

Thus, our meaning-constitutive (and *ipso facto* concept-constitutive) beliefs about natural kinds can be revised without change of subject matter. Our revised beliefs may still be about the "same things".[51]

If analytic definitions (or necessary and sufficient conditions) do not provide the means to understand meaning, then a novel account of meaning is needed. "The meaning of 'meaning'" provides such an account, according to which the meaning of a word is represented by a four-dimensional "vector" with the following components: "(1) the syntactic markers that apply to the word, e.g. 'noun'; (2) the semantic markers that apply to the word, e.g. 'animal', 'period of time'; (3) a description of the additional features of the stereotype, if any; (4) a description of the extension" (ibid., p. 269). The semantic markers consist of those features of the stereotype that "attach with enormous centrality to the [corresponding] words … form part of a widely used and important *system of classification*", and are "*qualitatively* harder to revise" than the rest (ibid., p. 267). In Putnam's view, "[t]he centrality guarantees that items classified under these headings virtually never have to be *re*classified" (ibid., pp. 267–268). Furthermore, the final component of a term's meaning is its extension, which is identified by means of a (fallible) description. Since the meaning of a term includes its extension, it follows that meanings should be distinguished from concepts (cf. Floyd, 2005, p. 23).

Putnam's account of meaning allows for conceptual change. The stereotype (the concept) we associate with a term may change under empirical pressure. However, conceptual change need not be accompanied by ontological shifts. Two successive scientific concepts may differ and, nevertheless, refer to the same thing(s). Thus, Putnam has offered us a way to take on board some of the historicist insights about the development of the sciences without, however, succumbing to their more radical relativist and anti-realist inclinations.

Putnam's promising approach to meaning and conceptual change has also encountered difficulties. Discussing these difficulties in detail would lead us too far astray. We will just sketch one of the most important.[52] The difficulty in question derives from the realist presuppositions of Putnam's account of meaning. As we have seen, the users of a natural kind term belong to a linguistic community which has "contact with the natural kind" (Putnam, 1975g, p. 205). There are two problems here. First, when the term in question denotes unobservable entities, such as the electron, it is unclear whether the required "contact" is available. Second, it has often been the case that words referring to putative natural kinds, such as "phlogiston" or "ether", turned out to be empty. In those cases the presumed "contact" was clearly missing. Thus, it would follow from Putnam's account of meaning that the users of those terms were not linguistically competent! Putnam realized that "it may seem counterintuitive that a natural kind word such as 'horse' is sharply distinguished from a term for a fictitious or non-existent natural kind such as 'unicorn', and that a physical magnitude term such as 'electricity' is sharply distinguished from a term for a fictitious or nonexistent physical magnitude or substance such as 'phlogiston'" (ibid., p. 206).

However, he did not seem to take into account that those who introduced and used those terms did so for similar reasons, namely to make sense of various observed phenomena. And that for some time theories based on 'electricity' and 'phlogiston' were equally viable accounts of their respective domains. It is unclear why our different (retrospective) judgments concerning the (non-)existence of phlogiston and electricity should lead us to different semantic stances towards the corresponding terms.

Despite its problems, Putnam's account of meaning has been one of the most articulate attempts to face the challenge that Feyerabend's and Kuhn's historicist accounts of conceptual change posed for the philosophy of science. Another such attempt was by Kuhn himself, who tried to come to terms with the difficulties faced by his early formulation of incommensurability. Kuhn's more recent work will be the subject of the next section.

KUHN'S EXPLICATION OF INCOMMENSURABILITY

Some of Kuhn's philosophical insights were an outgrowth of his experience as an interpreter of past scientific texts. One of those insights was incommensurability, which Kuhn regarded as the key notion of his philosophy of scientific development. During the 1980s and 1990s, he defended this notion against the criticisms that had been raised against it, and he attempted to explicate and develop it further (Kuhn, 2000).

As we have seen, two of those criticisms were particularly forceful. First, Kuhn's critics claimed that incommensurability implies incomparability and, therefore, renders rational theory-choice impossible. Second, incommensurability was construed as untranslatability and was declared to be at odds with the interpretive practices of historians of science. The purported incommensurability between past scientific theories and their contemporary descendants would imply that a translation of those theories to a modern scientific idiom is impossible. However, historians of science, such as Kuhn himself, have been able to translate past scientific texts, belonging to a discourse purportedly incommensurable to our own, to equivalents that are accessible to a modern audience. Thus, the incommensurability thesis is undermined by the very success of the historiography which it inspired.

Kuhn denied both of these critiques. First, he maintained that incommensurability does not imply incomparability. The term "incommensurability" was appropriated from geometry, where a comparison of incommensurable magnitudes is possible. The same is true of incommensurable theories. The fact that there is no common language in which the assertions of two incommensurable theories can be expressed does not preclude the comparison of the theories in question. Comparative theory-evaluation does not require the existence of such a common language.

Second, Kuhn resisted the identification of the interpretive practice of historians with a translation process. The aim of the historiographical enterprise is to understand, as opposed to translate, alien scientific texts. It is true that texts written in a language incommensurable to our own are impossible to translate. However, they are not impossible to understand, by acquiring from scratch the language in which they were written.

Having responded to criticism, Kuhn proceeded to develop a fuller account of incommensurability as a manifestation of a deeper mismatch between two linguistic structures. The language in which a scientific theory is formulated incorporates a taxonomic structure of natural kinds. These structures are subject to the so-called no-overlap condition. That is, no entity can belong to more than one natural kind. In Kuhn's words, "[t]here are no dogs that are also cats" (Kuhn, 2000, p. 92). The only case where this condition is not fulfilled is when one natural kind is part of another, more inclusive one. For example, cats are also mammals. Incommensurability is now conceived as the outcome of a violation of the no-overlap condition. Two incommensurable taxonomic structures contain overlapping natural kinds — kinds which have some members in common. The overlap has to be partial — the one kind must have members that do not belong to the other and vice versa. If, on the one hand, the overlap were complete then the two taxonomies would obviously coincide. If, on the other hand, there were no overlap at all, it would be possible to construct a more inclusive taxonomy incorporating each of the taxonomic structures in question. The natural-kind terms of this extended taxonomy could be used to express any assertion that can be formulated within each of the languages corresponding to the two more restricted taxonomies. In that case incommensurability would not arise.

When, however, two taxonomies partially overlap, it is not possible to subsume both of them under a wider taxonomic framework. Any framework that would subsume one of the taxonomies in question could not accommodate the other and vice versa. Such a framework would have to include overlapping natural kinds, which are ruled out by the no-overlap condition. In the absence of such a framework no language can be found that would provide a common ground between the taxonomies in question; hence incommensurability.

Taxonomic incommensurability can now be used to re-conceptualize scientific revolutions. These are episodes in the history of science where a whole taxonomic structure is replaced by its incommensurable successor, whose natural kinds partially overlap with some of the natural kinds that were hitherto in place. As a result of this overlap, the new taxonomy cannot subsume the old one. As an example, consider the transition from Ptolemaic to Copernican astronomy in the 16th century. In the Ptolemaic taxonomy of the heavens, planets were identified as those heavenly bodies which moved with respect to the fixed stars. They were seven: Moon, Mercury, Venus, Sun, Mars, Jupiter, and Saturn. In the Copernican taxonomic structure, on the other hand, planets were identified as those heavenly bodies which moved around the sun. They were six: Mercury, Venus, Earth, Mars, Jupiter, and Saturn. Obviously, there is a partial overlap between the Ptolemaic and the Copernican classificatory schemes. Because of this overlap some of the assertions of Ptolemaic astronomy regarding "planets" cannot be expressed in Copernican terms; hence the incommensurability between the Ptolemaic and the Copernican conceptual schemes.[53]

The reformulation of scientific revolutions in Kuhn's later writings goes as far as dimming the light on discontinuous change. Kuhn now speaks of two lexicons "used at two widely separated times" (Kuhn, 2000, p. 87), leaving aside and in the shadow the processes that made these lexicons possible and through which they came about. The philosopher studies two distant in time, frozen, taxonomic structures trying to detect congruence, compatibility and overlap, whereas the historian may try to uncover the micro-processes by which change is effected and which take place in the in-between transitional periods.

The emphasis given in Kuhn's later writings to some kind of continuity rather than abrupt change is also brought out in his analogy between revolutions in science and speciation in biological evolution (ibid., p. 98; see also Kuhn, 1970, pp. 171–173). In both cases, a slow, steady development driven from behind is occasionally marked by episodes which yield new specialties in science and new species in nature that branch off from the trunk of knowledge or the biological tree respectively. Breakdowns of communication in science are compared to reproductive isolation of populations in nature and are taken to be signals of crises (Kuhn, 2000, p. 100). Discourse, however, among scientists across the divide of incommensurable taxonomies still goes on, Kuhn admits, "however imperfectly" (ibid., p. 88), by using, for instance, metaphor and other linguistic tropes.

Focus on evolution and continuity rather than revolution and discontinuity in Kuhn's later writings may not have eliminated the idea of incommensurability, which has become local and has been understood as incongruity of clusters of partially overlapping conceptual frameworks, but has given rise to concerns regarding the Kuhnian project itself. Kuhn, supposedly, modified his philosophy to make more plausible the history of science which he saw as advancing through revolutions. If now evolution is substituted for revolution, "what point could there be in talking about Kuhnian revolutions at all?" (Machamer, 2007, p. 43). Kuhn's model was introduced as revisionist, upsetting the standard cumulative account of scientific development but, according to this critic, through its transformations, ended up looking very much like the one it displaced.

A second problem concerns the notion of local incommensurability. As we saw, in order to have local incommensurability between frameworks there needs to exist partial overlap between them. Two things can be said here: first, one may maintain that partial overlap between theories is likely to hold in most cases since theories develop out of earlier ones or, in case they are contemporaneous, they develop against each other (Fine, 1975, p. 30).[54] If this is true, then the thesis of incommensurability becomes trivialized. The second thing that can be said in relation to overlap is that instead of viewing it as a condition of incommensurability, one may view it as a means of eliminating it. Assuming certain common things, those falling under the area of overlap, may be of help when we try to trace and unpack the mismatching incommensurable clusters (ibid.). With some effort, based on this common ground, critics say, one may access the problematic areas and achieve communication and translation. Kuhn would have conceded, we think, that a partial overlap between incommensurable taxonomic structures may provide the common ground necessary for communication, albeit not for translation.

Another area of concern is taxonomic structure itself. Are the classificatory systems that we find in conceptual frameworks arbitrary, conventional, up to the scientific communities, or do scientists "carve nature at its joints"? What kind of similarities do objects which fall under a category share? Are these similarities detected or imposed? All these issues are related to the problem of realism. If classification is conventional, then the fear is that we lapse into idealism and constructivism. If similarities between objects are to be read off from actual instances, then abstract concepts with no observable extensions available need to be dealt with differently.[55]

Even if we grant that incommensurability amounts to a disparity between local areas of classificatory systems, still, the question remains whether there might be conceptual change that does not involve reorganization of lexical structure. Is all conceptual change a matter of taxonomic change? Kuhn's analysis of incommensurability is applicable to concepts that pick out observable objects, which can be identified by ostension. On the other hand, his analysis of the meaning of theoretical concepts (such as force, mass, charge, and field) has not been sufficiently developed. Those concepts refer to entities, properties, and processes that are not directly accessible and their meaning is learned in the context of the application of scientific laws. When those laws are revised, the meanings of the concepts they contain should change accordingly. However, the source of incommensurability in such cases is rather unclear (see Andersen & Nersessian, 2000; Nersessian, 2002d).

A different set of criticisms concentrates on the contribution of individuals to conceptual change. Kuhn has stressed the role of scientific communities in bringing about conceptual change, but his account, especially in its late formulation where he considers whether developed taxonomic structures map onto each other, does not examine how the restructuring of categories is motivated and how it takes place (Nersessian, 1998, 2002d). This criticism is reinforced by the literature on conceptual change in educational contexts, which has also stressed "the active role of the individual in understanding or constructing new knowledge" (Vosniadou, 2007, p. 3). And this brings us to our final section.

UNDERSTANDING THE FINE STRUCTURE OF CONCEPTUAL CHANGE IN SCIENCE

Kuhn's later work on incommensurability has deepened considerably our understanding of the structure of conceptual revolutions in science. Its main drawback, as we see it, is that it focuses exclusively on the beginning and the final stages of radical conceptual change. Thus, it gives no account of the fine-grained processes, the nitty-gritty details of the transition between two incommensurable conceptual frameworks. This shortcoming is characteristic of most philosophical theories of conceptual change, with the exception of those offered by Dudley Shapere and Nancy Nersessian.

Shapere rejects both the necessary and sufficient conditions view of concepts and the idea that concepts are fully theory-dependent. Rather he considers scientific concepts as trans-theoretical, as providing a common ground between successive scientific theories. He accepts that concepts evolve, but he stresses the "chain-of-reasoning" connections between the successive versions of a concept. These connections guarantee the continuity of conceptual change:

> that continuity, or, more importantly, the chain of reasons which produced that continuity/… alone justifies our speaking of "the concept" or "the meaning" of … [a] term, and our speaking of the term having "the same reference". (Shapere, 1984, p. xxxiv)

Thus, for Shapere, it is important to focus on the specific reasons that motivate conceptual change in order to understand its continuity.

Nersessian has developed Shapere's insight into a full-blown account of conceptual change in science (see, e.g., Nersessian, 1984, 1987, 1992, 1995). She has pointed out that the problem of conceptual change has two salient aspects (Nersessian, 2001). The first is about finding a representation of concepts that could capture both their synchronic characteristics and their diachronic development. Nersessian rejects the necessary and sufficient conditions view of concepts, arguing that it cannot do justice to the fact that concepts are evolving entities. If the meaning of a concept were given by a set of necessary and sufficient conditions, then the concept in question could not evolve; it could only be replaced by an altogether different one. Thus, the necessary and sufficient conditions view obscures the significant continuity that characterizes the evolution of scientific concepts. To highlight the continuity of conceptual change, Nersessian has proposed a representation of concepts in terms of a "meaning-schema":

> The meaning of a scientific concept is a two-dimensional array which is constructed on the basis of its descriptive/explanatory function as it develops over time. I will call this array a "meaning schema". A "meaning schema" for a particular concept, would contain, width-wise, a summary of the features of each instance and, length-wise, a summary of the changes over time. (Nersessian 1984, p. 156)

The features that comprise the meaning of a particular instance of a concept concern "'stuff', 'function', "structure", and 'causal power' ... Here, "stuff" includes what it is (with ontological status and reference); 'function' includes what it does; 'structure' includes mathematical structure; and 'causal power' includes its effects" (ibid., p. 157). This matrix-like representation of scientific concepts portrays their complex structure and their dynamic, evolving character. Furthermore, it reveals the significant continuity between successive versions of a concept. Tracing the career of scientific concepts by means of "meaning-schemata", it becomes evident that they change in a continuous, albeit non-cumulative, fashion (see, e.g., Nersessian, 1987, p. 163; 1992, p. 36).

The second and most neglected part of the problem of conceptual change is to account for the mechanisms of concept formation and to understand how new concepts develop out of older conceptual frameworks. Recently, many historians and philosophers of science have shifted the focus of their analyses from the products of scientific activity (codified in published papers and textbooks) to the cognitive and material practices of scientists (science in action). Nersessian's approach to conceptual change is in tune with this turn to practice. She insists that conceptual change cannot be adequately understood by focusing exclusively on the products of scientific theorizing, that is, on fully articulated conceptual structures. Rather, it "is to be understood in terms of the people who create and change their representations of nature and the practices they employ to do so" (Nersessian, 1992, p. 9). These practices consist in problem solving. Developing an insight of Karl Popper, who viewed the history of science as a history of problem situations, Nersessian has argued that concept formation has to be understood in the context of evolving problem situations:

> Historical investigations establish conceptual change to be a problem-solving process that is extended in time, dynamic in nature, and embedded in social contexts. New concepts do not emerge fully grown from the heads of scientists but are constructed in response to specific problems by systematic reasoning. (Nersessian & Andersen, 1997, p. 113)

The resources and the constraints for the construction of novel concepts are provided by the scientific and socio-cultural context in which problem-solving activity is embedded. This activity encompasses a rich repertoire of reasoning strategies, going well beyond induction and deduction, which have been the main preoccupation of philosophers of science. Those heuristic strategies include drawing analogies, constructing visual representations, forming idealizations and abstractions, and inventing thought experiments (see Nersessian 1988, 1992). Nersessian has argued that these strategies can be viewed as instances of model-based reasoning, which has received extensive attention in cognitive science (Nersessian 1999, 2002a, 2002b, 2002c).[56] On the plausible assumption that the cognitive capacities of scientists do not differ substantially from those of "ordinary" people, it becomes possible to draw on the resources provided by cognitive science to analyze the forms of reasoning employed in creative scientific work. The resulting rapprochement between history and philosophy of science and cognitive science promises to be beneficial to both parties.

Let us close with indicating briefly how Nersessian's analysis of conceptual change may resolve one of the thorny philosophical problems that were brought to the fore by historicist philosophers of science, namely the problem of scientific rationality. Historicists and their critics thought that conceptual change undermined the rationality of scientific development. If, say, Newtonian "mass" and relativistic "mass" are not the same concepts, then how can one rationally compare Newtonian mechanics and relativity theory? This difficulty, however, is an artifact of the way the problem of conceptual change was framed, that is, as a problem concerning the relationship between the beginning and the final stages of a long process. If, on the other hand, one examines the fine structure of that process, the problem dissolves. As a matter of fact, scientists never faced a situation where they had to compare Newtonian mechanics and relativity theory. The transition from Newtonian "mass" to relativistic "mass" passed through various developments in electromagnetic theory, most notably through H. A. Lorentz's theory of electrons, all of which were rational in the sense that they were adequate responses to particular problem situations. The conceptual transition from Newtonian to relativistic physics was a gradual and reasoned process. Thus, conceptual change in science can be fully compatible with an account of science as a rational enterprise (see Nersessian, 1987, p. 163).

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NOTES

1. This goal consisted in unifying the language and the laws of the various branches of science by deriving, for instance, the laws of psychology and social science from the laws of physics and biology (see Carnap, 1981).
2. Carnap in his *Aufbau* (1969) called it "constructional system of concepts".
3. Schlick, for instance, claimed that content is, by definition (1981, p. 142), inexpressible and incommunicable (ibid., p. 137), whereas structure can be shared. This can be illustrated with the example of colour. Different people may have different images and impressions of particular colours but the system of colours can be represented in a publicly visible structure which can be shared by all. This structure was the three–dimensional colour solid in the shape of a double cone on which the whole system of colours was exhibited. Colour samples of hues were arranged on it according to hue, saturation and brightness, giving a spatial and synoptic presentation of colour complexity. With this device even a blind person familiar with the structure of space, "which to him is a certain order of tactual and kinaesthetic sensations" (ibid., p. 138), can acquire and have the system of colours. Visual or tactual content (as a matter of individual experience) is not important for understanding and communication. What is important is structure.
4. Carnap does not include in the theory of science the study of the actual scientific activity, its historical development, the individual conditions of scientists, the society in which science is practiced. He thinks that these issues have a place in the *Encyclopedia* (1938), the project the logical positivists had conceived to advance the understanding of science, but he assigns them to sociology, psychology, or history of science.
5. "The word 'concept' is used in various ways; its sense is sometimes psychological, sometimes logical, and sometimes perhaps a confused mixture of both" (Frege, 1979b, p. 88).
6. Frege also insisted on distinguishing the psychological laws of thought from the laws of logic (1979d, p. 149).
7. 'Begriffsschrift' has also been translated as 'ideography'. See Van Heijenoort (1980).
8. For a concept to have sharp boundaries means that "every object must fall under it or not, tertium non datur" (Frege, 1997b, p. 298). If a concept does not satisfy this requirement, it is meaningless (Frege, 1979c, p.122).
9. Frege compares thought to the kernel that has to be separated from the verbal husk (1979d, p. 142).
10. Frege (1980a, p. 7) spoke of "break[ing] the domination of the word over the human spirit by laying bare the misconceptions that through the use of language often almost unavoidably arise concerning the relations between concepts and by freeing thought from that with which only the means of expression of ordinary language constituted as they are, saddle it".
11. Leibniz called it "the alphabet of thought" (Leibniz, 1989, p. 6). It was supposed to aid reasoning and communication by calculations upon signs of concepts.
12. Frege's technical term 'Bedeutung' has also been translated as 'reference', 'denotation' or 'nominatum'. We are following here the most recent consensus to standardize the translation as 'meaning'. Two are the major reasons that are given: (1) This is the most natural English equivalent and captures, normally, Frege's early, non-technical, use of the term; (2) any other translation (for instance, 'reference') would mean that the translator proceeds to give a particular interpretation of the development of Frege's thought. For a thorough discussion of the issue see Beaney (1997, pp. 36–46).

    Frege made the distinction between sense and meaning to account for the informativeness of identity statements. While 'a=a' is a tautology and carries no information, 'a=b' is a statement that purportedly extends knowledge. If 'a=b' is true it might appear that it is no different from 'a=a'. By distinguishing between Sinn and Bedeutung, Frege could account for the difference in the cognitive value of the two equalities: the two names, a and b, have the same meaning, or designate the same Bedeutung, but they have a different sense.

13. For the exegetical and philosophical controversies regarding Sinn and Bedeutung in Frege's philosophy see Beaney's Introduction to *The Frege Reader* (1997).
14. According to Carnap the two pairs coincide in "ordinary (extensional) contexts" (Carnap, 1988, p. 124), i.e., contexts like sentences, whose truth value does not change if we substitute an expression which occurs in them with another. For instance, the sentence "The Morning Star is the planet Venus" continues to be true if we substitute the name 'The Evening Star' for the name 'The Morning Star', whereas substituting the 'The Evening Star' for the 'The Morning Star' in the sentence "John believes that the Morning Star is the planet Venus" does not mean that the sentence will have the same truth value (intensional context). Carnap also finds a correlation among his distinction between 'extension' and 'intension', the distinction between 'extension' and 'comprehension' in the Port-Royal Logic and John Stuart Mill's distinction between 'denotation' and 'connotation' (ibid., p. 126).
15. It should be noted that according to Frege concept words, just like any other words, have meaning only in the context of a proposition.
16. Carnap (1969, p. 8), just as Frege, traces his project back to Leibniz's idea of *ars combinatoria* and *characteristica universalis*.
17. The statements which connect theoretical concepts and postulates to experience have been called "coordinative definitions" by Reichenbach, "correspondence rules" and "semantic rules" by Carnap, "bridge principles" by Hempel, "dictionary" by Campbell and Ramsey, "coordinative definitions", "operational definitions", "epistemic correlations", "interpretative principles" (see Feigl, 1970, Hempel, 1970).
18. The logical positivists were indebted to Hilbert for the idea of implicit definitions (Friedman, 1999, p. 100).
19. Carnap says that the language of objects lends itself to realism while the language of concepts to idealism (Carnap, 1969, p. 10).
20. In a similar vein, Sellars (1973) considers the issue of change of belief vs. change of meaning and discusses the difference between conflicting beliefs involving the same concepts and conflicting beliefs involving different but similar concepts.
21. Notice that Carnap speaks of one unifying language of science. Toulmin (1972, p. 62) calls attention to what Carl Hempel means by 'the language of science': "the lower functional calculus with individual constants … universal quantifiers for individual variables, and the connective symbols of denial, conjunction, alternation and implication". Once again, notes Toulmin, "the philosopher's version of 'the language of science' turns out to be, not a mode of discourse ever employed in the actual work of professional scientists, but that very symbolism of 20th-century formal logic whose relevance needs to be demonstrated" (ibid.).
22. Bas van Fraassen (2002, p.117) makes a similar point: "Modern empiricism's notion of experience harbored a major historical tactic for denial of genuine conceptual change. If experience speaks with the voice of an angel, then we have a constant bedrock on which to found both discourse and rational belief".
23. Carnap admits that the distinction between observable and non-observable is not a sharp one (1936–37, p. 455).
24. Hempel (1990, pp. 225–226) notes however the difficulty of giving such definitions.
25. One of Wittgenstein's examples is the notorious concept of 'game' (PI §§ 66–71).
26. The references given are only indicative. Similar remarks can be found in many other places in Wittgenstein's work. One may compare here Wittgenstein's vague and fluid concepts to Waismann's open terms (1978, pp. 119–124). Waismann contends that most empirical concepts have an "open texture", i.e., "are not delimited in all possible directions" (ibid., p. 120) — the "essential incompleteness of an empirical description (ibid., p. 121), which means that their definitions are always corrigible. Waismann distinguishes open texture from vagueness, — "Vagueness can be remedied by giving more accurate rules, open texture cannot" — but understands vagueness rather differently from Wittgenstein.

Waismann calls a word vague "if it is used in a fluctuating way (such as 'heap' or 'pink')" (ibid., p. 120) while Wittgenstein thinks that normally all concepts are vague because they cannot be given precise definitions. So, even the word 'gold', which Waismann takes to be of an open texture but not vague in its actual use (ibid.), would be considered vague by Wittgenstein, in non scientific contexts, in the sense that its application cannot be delimited.

27. "Am I inexact when I do not give our distance from the sun to the nearest foot, or tell a joiner the width of a table to the nearest thousandth of an inch?" (PI §88).
28. Wittgenstein (PI §71) contrasts his own understanding of concepts to Frege's: "Frege compares a concept to an area and says that an area with vague boundaries cannot be called an area at all. This presumably means that we cannot do anything with it. –But is it senseless to say: 'Stand roughly there'? Suppose that I were standing with someone in a city square and said that. As I say it I do not draw any kind of boundary, but perhaps point with my hand – as if I were indicating a particular *spot.* And this is just how one might explain to someone what a game is. One gives examples and intends them to be taken in a particular way."
29. "I reserve the right to decide in every new case whether I will count something as a game or not" (PG §73; cf. PG §71).
30. Notice that here Wittgenstein, contrary to how we normally see things, attributes arbitrariness to the adoption of a definition.
31. A more thorough discussion of these issues would certainly have to take into account the complexities of the rule-following literature, exegetical and generally philosophical. A sample of the debates can be found in (Miller & Wright 2002).
32. Feyerabend had a brief encounter with Wittgenstein in Vienna but his plans to study with him at Cambridge were foiled by Wittgenstein's death in 1951. In 1952, he wrote a critical review of the *Philosophical Investigations*, more of a pastiche of paraphrase and comments, which was translated from the German by G.E.M. Anscombe herself (a leading philosopher and one of Wittgenstein's literary executors) and published in *Philosophical Review* Vol. 64, No 3 in 1955, 449–483.
33. Hempel (1948, p. 135) also thought that explanation is "one of the foremost objectives of all rational inquiry".
34. However, in case the explanandum is a general regularity, no singular antecedent conditions are needed in the explanans.
35. Feyerabend (1981a) argues that the history of actual scientific practice does not satisfy the requirements of the deductive-nomological model of explanation and that these requirements cannot be satisfied and should not be satisfied from the perspective of a "disinfected" or "sound" empiricism (ibid., pp. 47, 57, 76). His basic idea is that empiricism will admit and even require theories that are "factually adequate and yet mutually inconsistent" (ibid., p. 73) in order both to avoid dogmatism and maximize empirical content by encouraging the testing of theories against each other and not solely against their own empirical consequences. According to Feyerabend, the formal models of explanation and reduction do not take this version of the underdetermination thesis into account and require that theories be uniquely determined by facts and reduced to a single, comprehensive, theory.
36. Feyerabend explicitly connects contextual theory of meaning to Wittgenstein's philosophy but he wrongly criticizes Wittgenstein for allegedly replacing Platonism of concepts by Platonism of language-games (1981a, p. 74n68).
37. Hanson (1958, chapter 1) invokes Wittgenstein's philosophy constantly in his discussion of the theory-ladenness of observation. Feyerabend in his autobiography (1996, p. 140) cites three books that had influenced him in this respect: Bruno Snell's *The Discovery of the Mind*, New York: Dover (1982), first published in 1953, Heinrich Schäfer's *Principles of Egyptian Art*, Oxford: Griffith Institute (1987), first published in German in 1963, and Vasco Ronchi's *Optics, The Science of Vision*, New York: Dover Publications (1991) first published in 1957. In *Against Method* (1978, p. 133), he also cites Wittgenstein.
38. Toulmin feared that if we ignore the continuous evolution of science, "as philosophers, we may end, by replacing the living science which is our object of study by a formal and frozen abstraction, forget-

ting to show how the results of these formal enquiries bear on the intellectual and practical business in which working scientists are engaged" (1961, p. 109). And in his later work (1972, p. 59), he underscored the same point: "By analyzing our standards of rational judgement in abstract terms, we avoid (it is true) the immediate problem of historical *relativism*; but we do so only at the price of replacing it by a problem of historical relevance."

39. "Men who accept different ideals and paradigms have really no common theoretical terms in which to discuss their problems fruitfully. They will not even have the same problem: events which are 'phenomena' in one's man's eyes will be passed over by the other as 'perfectly natural'" (Toulmin 1961, p. 57).
40. Kuhn in *The Structure of Scientific Revolution* (1970) used the term paradigm in at least two different senses: one wide and one narrow. In the narrow sense a paradigm is a concrete exemplar or model. In the wide sense a paradigm is the tradition built around one or several exemplars. In his *Postscript to the Structure* (1970, pp. 174–210), he used two different terms to signify the difference in meaning: disciplinary matrix for the wide sense and exemplar for the narrow.
41. "[A] paradigm is what you use when the theory isn't there" (Kuhn, 2000a, p. 300).
42. "[T]he characteristic problems are almost always repetitions, with minor modifications, of problems that have been undertaken and partially resolved before" (Kuhn, 1977, p. 233).
43. In (1970, p. 85), Kuhn, citing the historian Herbert Butterfield, compares a science's reorientation after a paradigm change, to "handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework".
44. Kuhn acknowledges, however, that "[d]uring the transition period there will be a large but never complete overlap between the problems that can be solved by the old and the new paradigm. But there will also be a decisive difference in the mode of solution. When the transition is complete, the profession will have changed its view of the field, its methods, and its goals" (1970, p. 85). As we will see, in his later writings, Kuhn stressed more strongly that incommensurability is only local.
45. For a good synopsis of the early criticisms of Kuhn's and Feyerabend's views of meaning, see Suppe 1977, pp. 200–208.
46. Cf. also Scheffler 1982.
47. Note that in his more recent work Putnam has distanced himself from Davidson's denial of conceptual schemes. See Putnam, 2004, p. 50.
48. It is worth noting that, at some point, Putnam had also subscribed to a contextual view of meaning. See Putnam, 1975d, pp. 40–41.
49. Interestingly enough, Putnam gives credit to Quine for the "realization" of the inter-dependence of theory and meaning.
50. Here Putnam's ideas overlapped with those of Saul Kripke (1980). Note, however, that Putnam's attitude towards the philosophically charged notion of causality has changed considerably since the 1970s. See Ben-Menahem 2005, p. 16.
51. By the way, this takes the bite out of some of the criticisms that were raised against Feyerabend's and Kuhn's views of meaning (see above, p. 354).
52. For some other difficulties faced by Putnam's theory of meaning we refer the interested reader to Enç, 1976, Nola, 1980, Kroon, 1985, Psillos, 1999, Stanford & Kitcher, 2000, Arabatzis, 2007.
53. Here we should point out that Hanne Andersen, Peter Barker, and Xiang Chen have recently developed an illuminating account of Kuhn's taxonomic approach to incommensurability, in terms of Lawrence W. Barsalou's frame representation for concepts (see Andersen, Barker, & Chen 2006).
54. The exceptions would be cases of complete overlap, where a theory is fully absorbed, as a limiting case, by its successor.
55. For a thorough discussion of these issues see Kuukkanen, 2006.
56. Space limitations prevent us from expanding on this point. We refer the interested reader to Nersessian's contribution to this volume, where she gives a detailed account of the role of model-based reasoning in conceptual change.

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