

# The problem of “meaning change” in Friedman’s notion of constitutive *a priori* principle

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## 1. Introduction

The last decade has seen a renewed interest in the original Kantian notion of constitutive *a priori* principle that Michael Friedman deserves the merit of having brought back to the forefront of philosophical debate since the Nineties (Friedman 1997, 1999, 2001).

As is well known, it was the tradition of logical empiricism that pointed out the twofold meaning of *a priori*, which was already clear and distinct within the original Kantian conception: “necessary and un-revisable, true for all time” on the one side, and “constitutive of the object of [scientific] knowledge”, on the other (Reichenbach 1920/1965). Such a distinction was fundamental not only to Reichenbach (1920), but even to Carnap (1934), who both inspired Friedman’s idea of a “relativized *a priori*”.

In his 1920 *Theory of Relativity and A Priori Knowledge*, Reichenbach formulated a revised concept of the synthetic *a priori* in order to reconcile Einstein’s general relativity with the Kantian transcendental system. It is in such a framework that the *a priori* was presented in its constitutive (and coordinating) function between a concept and its object, and it was also regarded as relativized by means of the so-called *procedure of the continuous expansion*, which makes it “technically possible to discover inductively new coordinating principles that represent a successive approximation of the principles used until now” (Reichenbach 1920/1965, pp. 68-9). Moreover, Friedman himself quoted a passage from section 82 of Carnap’s *Logical Syntax of Language*, in which he asserts that:

[A]ny sentence of the language of mathematical physics, including L-rules or analytic sentences, may be revised in light of a ‘recalcitrant’ protocol-

sentence... Nevertheless, the L-rules, in sharp contrast with the P-rules, *define* what it means for a protocol-sentence to stand in logical relations to a synthetic sentence in the first place (Friedman 2001, p. 72).

It is clear that both Reichenbach and Carnap sustained the revisable and constitutive nature of the *a priori* while rejecting its absolute necessity and unrevisability. What both the two Neo-empiricists and Friedman argued is basically that such principles are *a priori* in as much as they are prior to experience: they set the necessary conditions for establishing empirical knowledge. Nevertheless, Friedman himself recognized that it would be comprehensible to doubt the maintenance of this “constitutive relationship” in face of the scientific development.

In response to such doubts, Friedman devoted great part of his *Dynamics of Reason* to explaining in what sense constitutive principles are necessary conditions of the possibility of properly empirical laws. Of course, Friedman was aware of the puzzling idea of calling “*a priori*” principles that change in response (not only) to empirical findings, above all in a “post-Quinean philosophical environment” (Friedman 2001, p. 71).

What I want to point out is the “meaning change”<sup>1</sup> that Friedman ascribes to terms and principles, which he calls *a priori*, in the transition from the old framework to the new.

This captures the sense, in particular, in which there has indeed been a ”meaning change” in the transition from the old framework to the new: even if the *same terms and principles* reappear in the new framework they do not have the same meaning they had in the old (Friedman 2001, p. 99, ft. 37, emphasis added).

Following Friedman, we should admit that the same words possess different meanings in different frameworks. In fact, terms and principles that are empirical in an old framework may shift to constitutive status in the new framework, and vice-versa. If Friedman’s account seemed to entrust the prospective rationality of science to constitutive *a priori* principle, the notion of meaning change suggests instead that Friedman’s argument upholds the Kuhnian account of incommensurability, while we were expecting he aimed to mitigate it. As he clearly states in the passage to follow:

The later framework is not translatable into the earlier framework, of course, simply because the concepts used in formulating the later framework have not yet come into existence (Friedman 2001, pp. 98-9).

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<sup>1</sup> I am particularly indebted to discussions at Uppsala University with Prof. Lars Göran Johansson, who led me to reflect on the notion of meaning and, in particular, the problem of meaning change. Here we had the occasion to compare the analytical and the, broadly speaking, “continental” views on meaning.

Friedman’s account purports to emphasize that a transition from empirical laws to principles, and vice-versa, should imply a conceptual shift. But is “meaning change” the appropriate expression that may describe such a conceptual shift? What does the word “concept” mean for Friedman? How are concepts related to the meaning of terms? What does the “meaning” mean in Friedman’s view? What is the relation between terms and theories? Does theory change entail a meaning change of scientific terms, that is, do terms determine theory change (i.e. difference in theories is *ipso facto* difference in terms)?

Recently, some specialists (Tsou 2010, for instance) have stressed Friedman’s notion of constitutive *a priori* principle in light of Putnam’s positive account of apriority. Friedman and Putnam’s notions of relativized *a priori* are presented as similar insofar as they both affirm the existence of principles in science, which are revisable and relativized to a particular body of knowledge. However, the similarities do not take into account that Friedman ascribed a meaning change to coordinating principles that are constitutive of the new framework. Could Putnam subscribe such a meaning change?

This paper aims to analyse Friedman’s notion of constitutive *a priori* principle in relation to the problem of meaning change by taking into account Putnam’s theory of meaning and his notion of framework principle.

## **2. The twofold meaning of constitutive *a priori* principles**

According to Friedman, each scientific theory consists of three asymmetrically functioning parts: a mathematical part, a mechanical part, and a physical or empirical part. The mathematical part includes basic mathematical theories that are employed to describe the spatio-temporal framework in question, viz. infinite Euclidean space, four-dimensional Minkowski space-time, semi-Riemannian space-time manifolds. The physical part uses the theories in the mathematical part to formulate empirical laws describing concrete empirical phenomena, viz. the law of universal gravitation, Maxwell’s equations for the electro-magnetic field, and Einstein’s equations for the gravitational field. The third component (coordinating principles) comprising the mechanical part, functions to set up a correspondence between the mathematical part of the programme and concrete empirical phenomena. For example, the Newtonian laws of motion, the light principle, the principle of equivalence, the quantum of action would show such a coordinating function<sup>2</sup>.

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<sup>2</sup> The light principle (or the law of constancy of the velocity of light) coordinates concrete

It is thanks to coordinating principles that precise laws of nature formulated by means of the mathematical part of the programme or of the theory have empirical meaning. As Friedman put it:

Given such a tripartite structure, the laws of nature comprising the (properly) physical part can then be empirically tested: for example, by Newton's description of the solar system (including planetary perturbations) in *Principia*, Book III, or Einstein's calculation of the advance of the perihelion of Mercury (Friedman 2001, p. 80).

However, Friedman remarked, the function of coordination between the mathematical part and empirical phenomena has not to be regarded as a straightforward empirical test of these two components. For the mathematical part of the theories is in no way tested by such a procedure:

"what is empirically tested – Friedman adds – is rather the particular coordination or correspondence in virtue of which some or another mathematical structure is used to formulate precise empirical laws about some or another empirical phenomena" (Friedman 2001, p. 80).

We may say that the aforementioned "procedure" of coordination helps us to work out one of the ambiguities we encounter confronting Friedman's notion of constitutive *a priori* principle: its formulation as necessary condition for possession of a truth-value of an empirical law. As we also observed above, constitutive principles are expressed in terms of necessary conditions of the possibility of properly empirical laws, that does not mean that A is a necessary condition of B if B implies A, rather, as Friedman exemplifies, that A is necessary for B's meaningfulness, or, in other words, that A is a presupposition of B. However, the idea of presupposition is too weak to grasp the meaning of constitutive function of such principles. Rather, what really captures the sense is the reference to that particular correspondence function in virtue of which a mathematical structure can formulate certain empirical laws about some or

physical phenomena with the Lorentzian (or infinitesimally Minkowskian) character of the new four dimensional space-time metric. Einstein introduced the principle of equivalence after rejecting the notion of absolute simultaneity in the special relativity theory. On such a basis, it follows that the classical Newtonian theory of gravitation is also untenable. Therefore, Einstein formulated a new theory of gravitation compatible with the new relativistic space-time structure. As far as the quantum of action is concerned, the elevation of the quantum of action to *a priori* principle has to be connected with Bohr's work since 1922, when he recognized that within the *idea of a correspondence* between process of transition and components of motion lay a logical relation capable of establishing a dependence between spectrum and motion similar in all respects to that whereby in classical theory the intensity of the radiation emitted by a particle in the course of its harmonic oscillation depends upon its amplitude. But a specific study of this last point is required.

another empirical phenomena. It is the case of Einstein’s principle of equivalence in general relativity theory, in absence of which Einstein’s field equations would remain a purely mathematical description of a class of abstract (semi-) Riemannian manifolds with no empirical meaning.

Friedman emphasized the increasing abstractness of modern mathematical physics, from the sixteenth centuries onwards, in relation to pre-modern physics, whose theoretical concepts of space, time, and motion, were immediately suitable for the world they represented. Thus, the more the mathematical representations become abstract the more their coordination with experience becomes cogent. Conversely, it would be a purely logical view of inferential relationships that would lead into Quinean holism.

To summarize, Friedman seems to introduce a twofold function of constitutive principles: i) coordinating principles as presuppositions of empirical laws; ii) coordinating principles as “mediators” between abstract mathematical structure and empirical phenomena.

The laws of motion, in the context of Newtonian physics, therefore function as what Reichenbach...aptly calls coordinating principles (axioms of coordination). They serve as general rules for setting up a coordination or correspondence between the abstract mathematical representations lying at the basis of Newtonian physics (infinite Euclidean space, uniformly traversed straight lines in this space, abstract temporal intervals during which such states of uniform motion traverse equal spatial intervals) and concrete empirical phenomena to which these representations are intended to apply (the observable relative motion in the solar system, for example), (Friedman 2001, pp. 76-7).

The distinction is subtle, as Friedman himself pointed it out in the quotation to follow:

The Newtonian laws of motion are thus presuppositions of the properly empirical laws of Newtonian physics (such as the law of gravitation) in the sense considered earlier, *but they are also presuppositions of a very special sort*. Their peculiar function is precisely to mediate between abstract mathematical representations and the concrete empirical phenomena these abstract mathematical representations are intended to describe (Friedman 2001, p. 77, emphasis added).

The difference between coordinating principles as presuppositions and coordinating principles in their peculiar function of mediating between mathematical structures and concrete empirical phenomena lies on the formal notion of “function”, as it was articulated by Ernst Cassirer in a paper of 1907, “Kant und die moderne Mathematik”, and more extensively in *Substance and Function* of 1910 (Padovani 2011). Cassirer began to elaborate the idea of function or of a continuous series to see

“how such idea can be *a priori* generated step-by-step” (Friedman 2001). There exists coordination among the elements of the series, which are constituted by the relations each element bears to the other members of the same series. It is such a connecting relation that is constitutive of the object of scientific knowledge. Moreover, Cassirer’s idea of function entails a dynamical process of the abstract structures, which are ordered by mathematical relations (Friedman 2001):

It is the functional form itself that changes into another; but this transition never means that the fundamental form absolutely disappears, and another absolutely new form arises in its place. The new form must contain the answer to questions, proposed within the older form; this one feature establishes a logical connection between them, and points to a common forum of judgment, to which both are subjected (Cassirer 1910/1923, pp. 268-69; cf. Padovani 2011).

Cassirer’s reading of the historical development of mathematics lies at the basis of his “genetic” conception of knowledge that inspired both Reichenbach and Friedman’s notion of relativized *a priori* principle. As it was recently noted, Reichenbach’s concept of probability, which emerges from his doctoral dissertation of 1916, is largely indebted to Cassirer’s general approach presented in *Substance and Function* (Padovani 2011). As we observed above, Cassirer articulated a concept of function as constitutive of the object of scientific knowledge, which encompasses the notion of coordinating principle, and gives account of the conceptual shift of certain theoretic components in different scientific frameworks. In particular, Reichenbach referred precisely to Cassirer’s dynamical role of functions to explaining the conceptual development of the constants in nature:

[E]very constant is presented as a function; the natural constant which is simply given for certain laws and whose measurement several experiments are dedicated is brought into connection with completely different quantities, so that it appears as a function whose specific value in the previous laws is only attained under special circumstances. [...] This is the general approach of physics: to resolve constants into functions, to find more general laws that contain the previous laws as a special case. No end of this process is in sight (Reichenbach 1916/2008, p. 115, Cassirer 1910, pp. 351 ff.).

Cassirer’s notion of function might help to clarify the twofold meaning Friedman attributes to coordinating principles, as “mediators” between abstract mathematical representations and sensory phenomena, on the one hand, and as presuppositions of empirical laws, on the other. Nevertheless, Cassirer’s genetic conception of knowledge does not suffice to explain Friedman’s account of ascribing the “elevation” of an empirical law to the status of a convention. As in the case of special relativity theory, when Einstein used his light principle “empirically to define

a fundamentally new notion of simultaneity and, as a consequence, fundamentally new metrical structures for both space and time”.

It is in precisely this way, as writers under the influence of Poincaré are fond of putting it, that Einstein has “elevated” an empirical law to the status of a convention – or, as I myself would prefer to put it, to the status of a coordinating or constitutive principle. It is precisely here that an essentially non-empirical element of “decision” must intervene, for what is at issue, above all, is giving a radically new space-time structure a determinate empirical meaning – without which it is not even empirically false but simply undefined (Friedman 2001, p. 88).

An alternative explanation for giving account of the notion of coordinative definition to which Friedman appeals might be offered by Poincaré’s philosophical reflection, although his main insight was not to designate certain empirical principles as having coordinative status. He simply regarded certain principles as non-empirical. Moreover, as it will be shown in the paragraph to follow, Friedman used the same example of Einstein’s light principle to point out the empirical motivations for choosing a new coordinating principle.

Therefore, Friedman’s puzzling reference to conventionalism still remains: a similar element of decision does not seem to have any role, for instance, in formulating the principle of free mobility because it simply fails to make an empirical claim. One could argue, in more general terms, that concepts to which a certain principle refers to are not definable independently of the principle itself.

### **3. How can constitutive *a priori* principles grant continuity to different frameworks?**

One of the main teachings of Prof. Friedman is to present philosophy as it played a decisive role in making paradigm clashes possible. In his view, philosophical reflection plays a special and characteristic function in transitions between radically different conceptual frameworks during scientific revolutions. In particular, it is thanks to philosophical reflection if empirical laws would be elevated to the status of a coordinating principle. Friedman deserves the merit of having shed light on the role of philosophy in science, not only as a general heuristic tool, in the sense suggested by Gerald Holton, but as an objective tool of scientific research. As it was noted, he has taken up some insights of Kant and logical positivists, as that of considering the aims and methods of the philosophy of science, but, above all, he stressed the role that philosophy plays in the evolution of science focusing on the conceptual shifts that physics has passed through in the last three centuries (Di Salle 2002). He aimed to

point out how new scientific concepts can emerge from a critical reflection on established beliefs, whose resulting philosophical insight would not be a mere motivation for the new theory, rather an essential part of the theory itself. Coherently with this interpretation of scientific progression, Friedman's reconstruction describes a dialectical evolution of science, and seems to prevent himself from Kuhn's criticisms against the so-called cumulative conceptions of science. Ironically, the enquiry on the role of philosophy in science becomes the watershed between two radically different conceptions of scientific enterprise.

Indeed, Kuhn regarded philosophical views as merely subjective, in the sense that philosophical reflection can influence scientists when no strictly rational decision is possible: when scientists fail to solve problems by applying the methods sanctioned by the leading paradigm, they have to turn to debate about the fundamentals that is characteristic of philosophy and of the sciences in their immature, pre-paradigm phase (Kuhn 1970, p. 6).

Friedman's intention was to mitigate Kuhn's view by stressing the role of philosophy as a source of new ideas as being part of scientific discourse, "that is not itself scientific in the same sense" (Friedman 1999, p. 19). The main difference between the two approaches lies on the special status of such fundamental concepts: while scientific practice takes them for granted, philosophical reflection can provide new concepts by criticising the old ones. However, this activity is not confined to periods of revolution or inter-paradigmatic change, as Kuhn affirmed, but it is a significant part of the so-called "normal science". For instance, Einstein was able to appeal to practitioners of the preceding paradigm in classical mathematical physics "by placing his articulation of fundamentally new coordinating principles within the long tradition of reflection on the question of absolute versus relative motion going back to the seventeenth century" (Friedman 2001, p. 105). But this tradition of reflection, as Friedman claimed, is largely philosophical. One of the main characteristics Friedman ascribed to the distinction between philosophical and scientific reflection is that the former fails to reach the communicatively rational consensus achieved by the latter. So one has to ask oneself how philosophical reflection could help in mediating such rational agreement during scientific revolutions. According to Friedman, the answer to this difficulty is threefold:

First, the consensus we require in the case of a radically new scientific paradigm is...relatively weak: we require only that the new constitutive framework becomes a reasonable and responsible live option. Second, although we do not...attain a stable consensus on the results of distinctively philosophical debate, we do, nonetheless, achieve a relatively stable consensus on what are the important contributions to the debate and, accordingly, on what moves and arguments must be taken seriously.

Third, characteristically philosophical reflection interacts with properly scientific reflection in such a way that controversial and conceptually problematic philosophical themes become productively intertwined with relatively uncontroversial and unproblematic scientific accomplishments; as a result, philosophical reflection can facilitate interaction between different (relatively uncontroversial and unproblematic) areas of scientific reflection, so as, in particular, to facilitate the introduction and communication of a new scientific paradigm at the same time (Friedman 2001, p. 107).

Einstein’s so-called light principle that light has the same velocity in all inertial frames, for instance, could be seen as a coordinating principle for the definition of simultaneity: it represented a line of demarcation between Einstein’s thinking and the nineteenth century physics, but its roots are deeply entrenched in nineteenth century philosophy.

It is worth remarking that Kuhn ignored that the process of concept redefinition is a self-conscious and rational act, with a combination of scientific and philosophical motivations, that is itself constitutive of a new conceptual framework (Di Salle 2002, p. 194). Such motivations, in Friedman’s view, are responsible for the elevation of an empirical law to the status of a convention. However, it is here that Friedman lays himself open to criticism: to claim that an empirical law has been elevated to the status of constitutive principle raises some perplexities because of the different functioning part of constitutive principle with respect to the “original” empirical law.

Robert Di Salle pointed out that it would be more appropriate to say that it is the interpretive extension of an empirical law to become a constitutive principle, and in addition to that he specified that it would be also a mistake to regard certain principles as empirical, simply because it is not we who grant the new status, but constitutive principles do possess such status inherently. That the speed of light is the same in all inertial frames is a constitutive principle in the sense that it is part of the definition of an inertial frame – a concept that is not clearly defined independently of this principle.

The principle appears to be a straightforward empirical claim, at least if the notion of inertial frame is taken for granted, as indeed it appears to be in Einstein’s proposal that “the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good” (1905, 1952, p. 37). To assume this, however, is to assume that we can begin with an inertial frame and determine, as a matter of empirical fact, the velocity of light in that frame. Clearly the Michelson-Morley experiments assumed this much, as did the entire Maxwell-Lorentz theory. The remarkable difference between the Lorentzian perspective and Einstein’s, then, is not Einstein’s reinterpretations of the results of such

measurements, but his recognition that such a measurement is, in the contemporary situation, impossible. The velocity of light cannot be measured relative to an inertial frame, because we can no longer assume that we have an independent way of constructing an inertial frame in advance (Di Salle 2002, p. 197).

To summarize Di Salle's view, Einstein came to the "light principle" not by taking a decision of elevating an empirical fact to its constitutive status, but through a conceptual analysis that allowed him to unfold the constitutive principle inherent in a given body of theory. However, such an alternative view of considering the "constitutive nature" of certain empirical laws seems to confine the role of philosophy to an undefined conceptual analysis, whose unproblematic scientific component might prevail over the problematic philosophical one. In such a way it would be arduous to show how problematic philosophical reflection becomes intertwined with properly scientific reflection. Nonetheless, stressing the element of decision, according to Friedman's account, does not mean to introduce a subjective element and hence to restrict the role of philosophy to an extra-scientific territory, as what he wants to point out is that a non-empirical element of decision intervenes as a consequence of the philosophical debate on the foundations of the discipline itself. In addition, Friedman admitted the existence of empirical motivations for preferring a new coordination to the former one, as in the case of Einstein's special relativity:

The new empirical discovery in question – undetectability of differences in inertial motion in electrodynamics – provides us with strong empirical motivation, not only for entertaining a new coordination, but also (as Einstein was apparently also the first to see) for doubting the adequacy of the classical coordination. For, if there were in fact an empirical counterpart to the classical notion of absolute simultaneity, then there would be (in the context of electrodynamics) an empirical counterpart to absolute velocity as well. But the new empirical discovery strongly suggests that there is no such empirical counterpart (otherwise differences in inertial motion would be empirically detectable after all), (Friedman 2001, pp. 88-9).

But he also reminded that the history of science provides us with cases of non-empirical under-determination, "where two empirically equivalent hypotheses face off against the background of a common constitutive framework, and methodological principles such as simplicity or conservativeness are then invoked to settle the question" (Friedman 2001, p. 89). Conversely, one could object that so-called methodological motivations as conservativeness or simplicity could be encompassed by Kuhn's notion of paradigm shift and lay themselves open once again to criticisms of subjectivism and relativism. On this point, it is clear that Friedman has a hard job to convince his critics how constitutive principles change and develop from empirical to *a priori*: whether in response to empirical findings or due to cognitive values or

presuppositions at the basis of the scientist’s decision. In the former case, it would be hard to show what is the role for philosophy in science, in the latter case Friedman should clarify why his presuppositions are more objective than the Kuhnian ones.

#### **4. Putnam’s notion of relativized *a priori***

Putnam’s early works on the *analytic and the synthetic* distinction is usually a common reference point while confronting the notion of relativized *a priori*. What sounds really surprising is that Friedman never quoted Putnam while explaining the notion of constitutive *a priori* principle. Among the specialists in the field of history and philosophy of science the idea is widespread that the two scholars’ different cultural perspectives could explain their, so-called, incommunicability. Moreover, as it was recently noted, there is a “curious difference” between Putnam and Friedman’s accounts: Putnam articulated his notion of relativized *a priori* through Quinean insights, whereas Friedman did not hide his opposition to Quine’s holistic view (Tsou 2010). However, whether Quine’s analytic-linguistic approach to philosophy of science could be viewed as the watershed between two ways of conceiving the reflection on scientific development, I wonder why both Friedman and Putnam never jointed their efforts against what should be considered as their natural opponent, the Kuhnian perspective of incommensurability and untranslatability between different paradigms.

Since his celebrated article “It ain’t necessarily so”, Putnam dealt with the momentous distinction between *statements necessary relative* to a body of knowledge and *statements contingent relative* to that body of knowledge. Indeed, Putnam was remarking that the notion of necessity must be erased even from relative statements, whose necessity is “merely psychological”. As Putnam wrote: “[T]he traditional philosophical distinction between statements necessary in some eternal sense and statements contingent in some eternal sense is not workable” (Putnam 1962a, p. 670). Elsewhere, Putnam attacked the traditional notion of *a priori* regarded as necessary truth “relative to the context of ‘all context’” that Quine’s analysis had already undermined.

In “Two dogmas revisited”, Putnam pointed out that Quine used one of the notions of analyticity as apriority. As is well known, Quine distinguished a linguistic notion of analyticity: “a sentence is analytic if it can be obtained from a truth of logic by putting synonyms for synonyms”, from “a notion of analytic truth as one that is *confirmed no matter what*” (Putnam 1976).

Putnam remarked that the latter is the traditional notion of apriority, or rather, one of its traditional notions. Thus, it is the notion of unrevisability that – given Putnam’s understanding of Quine – should be rejected. But Putnam envisaged another account of analyticity yet since “It ain’t necessarily so” and “The analytic and the synthetic” from 1962 by stressing the importance of analytic statements in science. For Putnam, there are certain particular laws acting as principles within a specific framework. They are analytic, so to speak, to a particular body of knowledge, serving as necessary presupposition for making empirical knowledge possible:

The principle ‘ $e = 1/2 mv^2$ ’ may have been introduced...by stipulation; the Newtonian law of gravity may have been introduced on the basis of induction from the behaviour of the known satellite system and the solar system (as Newton claimed); but in subsequent developments these two famous formulas were to figure on a par. Both were used in innumerable physical experiments until they were challenged by Einstein, without ever being regarded as themselves subject to test in the particular experiment. If a physicist makes a calculation and gets an empirically wrong answer, he does not suspect that the mathematical principles used in the calculation may have been wrong...nor does he suspect that the law ‘ $f = ma$ ’ may be wrong. Similarly, he did not frequently suspect before Einstein that the law ‘ $e = 1/2 mv^2$ ’ might be wrong or that the Newtonian gravitational law might be wrong.... These statements, then, have a kind of preferred status. They can be overthrown only if someone incorporates principles incompatible with those statements in a successful conceptual system (Putnam 1962b, pp. 45–6).

Moreover, Putnam brought the example of the axioms of Euclidean geometry as principles close to analytic statements before the work of Riemann, Lobachevskij, and others in the nineteenth century. However, the same axioms of Euclidean geometry lost their character of presuppositions they held in the pre-relativistic frameworks, in as much as they had to be revised after the development of general relativity theory. In Putnam’s terms, we shall call such principles “framework principles”:

‘[F]ramework principles’ ...have the characteristic of being so central that they are employed as auxiliaries to make predictions in an overwhelming number of experiments, without themselves being jeopardized by any possible experimental results. This is the classical role of the laws of logic; but it is equally the role of certain physical principles, e.g., ‘ $f = ma$ ’...the laws of Euclidean geometry, and the law ‘ $e = 1/2 mv^2$ ’, at the time when those laws were still accepted (Putnam 1962b, pp. 48–9).

It is worth noting that Putnam described framework principles also as “contextually a priori”, “necessary to a particular body of knowledge”, and “quasi-necessary relative to contextual scheme” given the abnormality of calling potentially

false statements “necessary” or “*a priori*”, whether these statements are contextualized or not (Tsou 2010). Following Putnam’s reasoning, framework principles are therefore immune to revision insofar it is not possible to disconfirm them on the basis of the theoretical background of which they are constitutive.

Putnam and Friedman’s accounts of relativized *a priori* are only apparently similar. Beyond doubt, they agree on the fundamental function of relativized *a priori* principles in science: they both affirm that these principles are also revisable and necessary relative to a particular framework and so on. However, I am quite sure that Putnam would not subscribe Friedman’s notion of meaning change delineated at the outset of this paper.

There is another point of divergence between Putnam and Friedman I wish to point out: Putnam saw such coordinating principles also as auxiliaries, a definition that Friedman would not accept.

For the sake of clarity, I wish to point out that it is my aim nor to propose the application of a lexical notion of meaning, in the wake of Putnam’s theory, to Michael Friedman’s notion of constitutive *a priori* principle, neither to detect eventual weak points in his conception of scientific development. Rather, I want to shed some light on Friedman’s notion of meaning change utilized to describe conceptual shifts throughout scientific theories. Furthermore, I would like to ascertain whether Friedman’s notion of conceptual shift either is or is not grist to Kuhn’s mill.

## **5. Why to talk about meaning?**

Putnam reminds us that the theory of meaning depends upon the idea that a natural language has rules. He also defined the “meaning” of a word as a function of the rules governing its employment. These rules determine which locutions are synonymous, which locutions have more than one meaning, which sentences are analytic on which readings, etc. What happens when someone asks for the meaning of a word? For example, what happens when one asks a typical native speaker of English for the meaning of the word “gold”?

As Putnam argues, the English native speaker would probably provide us with a mass of “empirical information” about gold: that it is precious, normally yellow, incorruptible, etc. Perhaps, he would add also the “essential linguistic information” that gold is the name of a metal. However, one should notice, if gold became as “cheap as dirt”, or began to rust, the meaning of the word “gold” would not change. Coherently with his realistic conception of meaning, Putnam put forward that the

meaning of the word “gold” would change only if we gave up using “gold” as the name of a metal.

The example of gold was used by Putnam to object to Feyerabendian doctrine of meaning, as Putnam ironically called it. On purpose, it is worth recalling briefly Putnam’s paper (Putnam 1965) against Feyerabend’s view of meaning presented in his 1962’s work.

Feyerabend identified the meaning of a term with a certain accepted theory containing the term. This is not to say that he claimed that the meaning of a term is a theory; rather, he slid from the term “meaning” to some such locution as “accepted usage” and then cited empirical beliefs containing the term as examples of the accepted usage. Feyerabend claims: “if the same term occurs in two different theories  $T_1$  and  $T_2$ , it cannot be supposed to have the same meaning” (Feyerabend 1962). Moreover, he puts it also that “meanings are not invariant with respect to the process of explanation”, i.e. the fact explained contains terms, which change their meaning when the statement in question is deduced from a theory (Feyerabend 1962). This position cannot be sustainable from a realistic point of view. Let’s take up Feyerabend’s example of the word “temperature”. Following Putnam’s “hard realistic” view of 1965, we could say that even though we revised our beliefs about the exact laws obeyed by the physical magnitude of temperature, we would continue to use the word “temperature” to refer to the same physical magnitude. The use of the term “temperature” lies on the empirical fact that there exists a single physical magnitude, which is responsible for differences in “felt warmth”. However, one theory is essential to the meaning of the word temperature:

[T]hat the magnitude we identify as “temperature”, and quantify by means of thermometers, or however, is the magnitude whose greater and lower intensities are measured by the human sensorium as *warmer* and *colder* respectively. This does not mean that the human sensorium never fooled, but that when it is not fooled, when the differences in felt warmth are accounted for by a difference is some property of the object rather than of the subject, it is generally a difference in “temperature” that is responsible (Putnam 1965, p. 128).

It is evident that the term “temperature” is theory loaded, but it is also true that a meaning change of that term does not occur just because of a change of beliefs. Following Feyerabend, and Putnam’s understanding of his doctrine, what Galileo meant by the synonymous Italian word for “temperature” was something different from what we mean today by the word “temperature”. Feyerabend explained this as a consequence of the fact that we have given up the proposition that “the temperature shown by a thermometer is not dependent upon the chemical composition of the fluid

used”, which Feyerabend took to be constitutive of the Galilean concept. As Putnam showed, this is not possible, because if this statement were actually constitutive of Galileo’s concept of temperature, then Galileo would not be in the conditions to grasp the denial of the aforementioned proposition.

What Galileo meant was that intrinsic property of the body which the thermometer measures, and not the result of the measurement. And Galileo could *understand* the statement that measured temperature does not exactly correspond to true temperature, and that measured temperature depends to some extent on the fluid used, just as well as you or I can, independently of our degree of physical sophistication (Putnam 1965, p. 122).

However, Feyerabend did not uphold the radical view that any change in theory is a meaning change of terms. What he wanted to show is that false theories are presupposed by ordinary language. Even in science, if we do not draw a line of demarcation between “ordinary language” and “common sense” (the everyday beliefs of most speakers), it is plausible that most people, and scientists among them, may believe “many false things”. Feyerabend’s view of meaning cannot be regarded as a theory of meaning, as his claim was that the rules of language in connections with some specific terms presuppose false theories.

If we followed Carnap’s approach we would be entitled to say that questions of verifiability enter into questions of meaningfulness, but not into questions of sameness or difference of meaning, which are rather questions concerning the semantical rules of the language. Therefore, it is the role of terms in empirical theories that renders them meaningful.

As we have observed above, Feyerabend rejected such an approach, as he claimed that the meaning of a term depends on a whole theory containing the term. Therefore, it is not surprising if Friedman has used Carnap’s theory of linguistic frameworks, where a change in status from analytic to synthetic would involve a change of meaning, as an analogy for meaning change of terms and principles in inter-paradigmatic transitions. What is not clear is why Friedman did not embark himself in the elaboration of a radically new theory of meaning for replying to possible realistic objections. Whereas it was not Feyerabend’s intention to propose a theory of meaning, Friedman’s work seems still halfway, instead.

Let me allow adding that through the analysis of Friedman’s account one can note that he speaks of *principles* and *terms*. Therefore, one needs to point out that meaning change in terms is not trivially meaning change in principles that should be dealt with in different ways and methods. Indeed, if a meaning change occurs in principles, what terms should change meaning which they are part of in order we are allowed to talk about meaning change of that principle?

Moreover, as far as the notion of meaningfulness is concerned, according to Friedman, we should regard as testable the particular coordinating function in virtue of which the mathematical structure of a theory is used to formulate empirical phenomena. Whatever terms and principles they will be, it is their role in the formulation of empirical theories what renders them meaningful. Even in this case questions of verifiability enter indirectly into questions of meaningfulness, but not into questions of difference of meaning. The problem of meaning change should rise from the inter-paradigmatic transition of either a term or a principle from empirical to *a priori*. Thus, we should ask ourselves what terms and principles are constitutive of a theory. Perhaps, Friedman used indifferently the expressions terms and principles because in some cases you can have a single constancy acting as a principle (the constancy of the velocity of light or the quantum of action, for instance), some others you can have laws (Newton's three laws of motion, for instance). But to be consistent he should have drawn a distinction between the notions "constitutive *a priori* principles" and "constitutive *a priori* terms", if any. The point still remains: how and why should they change meaning?

## 6. Meanings and Concepts

When for the first time I confronted with Friedman's thought, I was impressed by the proposition in which he states that change of theories entails meaning change of terms. According to Friedman, and to Kuhn as well, the emergence of new concepts in a new framework would render the earlier framework untranslatable in the new, if we took for granted the quotation cited in the introduction of this paper, Friedman 2001, pp. 98-9. To the extent that it seems that change of concepts entails meaning change of terms, which those concepts refer to, in different frameworks. As it is well known, such a conclusion would be untenable from a realistic point of view. The notion of concept Friedman deals with seems to refer to abstract entities, which can be grasped through an individual psychological act. According to this notion, theory of meaning would come to rest on two assumptions, which, as Putnam argued, are not satisfied by any notion of meaning:

- (I) That knowing the meaning of a term is just a matter of being in a certain psychological state [...].
- (II) That the meaning of a term (in the sense of intension) determines its extension (in the sense that sameness of intention entails sameness of extension), (Putnam 1975a, p. 219).

What Putnam has showed is that it is possible for two speakers to be in the same psychological state, although the extension of two equal terms used by the two speakers in their respective idiolects is different. In fact, extension is not determined by psychological state, therefore meanings are not concepts. Considering that meaning cannot be identified with extension either, as Putnam argued, it seems preferable to identify meaning with an ordered pair of entities, one of which is the extension. It comes out that meaning determines extension (i.e. difference in extension is *ipso facto* difference in meaning).

We have seen that the extension of a term “is not fixed by a concept that the individual speaker has in his head” (Putnam 1975a, p. 245), because extension is determined both socially and, in part, indexically. Putnam emphasized that social determination of extension is due to the division of linguistic labour.

Returning to the dichotomy “meaning change” or “change of theory”, it is worth noticing that Friedman’s account, given my understanding of his thesis, resembles Quine’s that meaning change and theory change cannot be sharply separated. But I do not think this conclusion was exactly one of Friedman’s aims!

A way out of this riddle can be found in Putnam’s revision of Quine’s notion of meaning change, but only admitting that meaning change can be forced by empirical discoveries, and at certain conditions.

If we discovered that we live in a non-Euclidean world it would change the meaning of “straight line”, although, as Putnam argued, “it would not be only a change of meaning”. In particular, “it would not be a change of extension: thus it would not be right to say that the parallels postulate was true in the former sense of the words” (Putnam 1975a, p. 256).

If a term has changed its meaning, it means that whatever of the ordered pair of entities (syntactic markers, semantic markers, stereotype, extension), which define, for Putnam, the meaning of a word as a finite sequence, or “vector”, has changed.

In my view, Putnam’s proposal helps to clarify many aspects of Friedman’s explicit conviction that there is a meaning change of terms in the transition from empirical laws to constitutive *a priori* principles. Friedman’s philosophical view hides the anti-realistic conception that the extension of a term is not tied to the notion of truth, but to its intra-theoretic notion. According to realism, the extension of a term is just what the term is true of. A realistic scientist would use scientific terms as if they were “approximately correct characterizations of some world of theory-independent entities” (Putnam 1975a, p. 237). In such a view, later theories would be better descriptions of the same entities that earlier theories referred to. It is the realistic hypothesis that this is *right* that will account for the communicability of scientific results. However, according to Friedman, communicability in science is

granted by the role philosophy plays in facilitating the introduction and communication of new scientific paradigms. What role should we assign to philosophy in a “so-to-speak” realistic environment? Putnam’s theory of meaning can pin down the role of philosophy in science. Philosophical debate, in fact, may play a special role in the process of social determination of extension that is forced by empirical discoveries. In particular, the “syntactical” role of philosophy in determining the meaning of a term may be identified in the process of stereotype’s formation.

As far as coordinating principles are concerned, no one will deny their centrality in a given body of knowledge from an analytical perspective, either. In inter-paradigmatic transition, they could be forced by empirical discoveries to changing not only the meaning, but also the truth-value.

## Conclusion

It was this paper’s aim to analyse Friedman’s notion of constitutive *a priori* principles in light of Putnam’s notion of framework principles that some specialists have already confronted without stressing the condition of meaning change Friedman assigns to coordinating principles in their evolution from empirical laws. This notion of meaning change appears to hide Friedman’s anti-realistic conception that meanings are tied to concepts, although analytical philosophy of language had already showed the un-tenability of this conclusion. However, a realistic theory of meaning does not entail the rejection of the notion of coordinating principle: it allows not falling into contradictions when discussing the problem of meaning change related to the change of “status” of such principles in their inter-paradigmatic transition. To be more precise, one could hold that following a new scientific discovery, and the advent of a new scientific framework, there could be a change of function of some terms and principles and, in Friedman’s view, this change of function *might* be followed by a meaning change of such terms. According to Putnam, this would not be a *mere* meaning change, due to neither a change of extension nor a concept change, but in light of later and more sophisticated theories, we could get better descriptions of the same entities that earlier theories referred to. Indeed, what was expressed in the former sense of these terms and principles could not be *true* in the new paradigm. What could happen if a principle influenced the stereotype of a word? This would cause a meaning change of that word.

As it could be the case in the “somewhat unlikely” event of the word “straightness”, whose meaning would change if the parallels postulate determined its

stereotype (of straightness) in the transition from Euclidean geometry to a non-Euclidean geometry.

As I see it, it is here that we can set the role of philosophy in science, in the stereotype’s formations of such words. It is precisely the ideal place where philosophical reflection can facilitate interaction between different (relatively uncontroversial and unproblematic) areas of scientific reflection Friedman was referring to. But a further discussion of it would take me away from my topic that aimed to compare Putnam and Friedman’s accounts of coordinating principles in respect to the notion of meaning.

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