UNIVERSITÀ DEGLI STUDI DI PALERMO
Dottorato di ricerca in Scienze Filosofiche – indirizzo Filosofia del Linguaggio, della Mente e dei Processi Formativi
Dipartimento di Scienze Umanistiche
Settore Scientifico Disciplinare M-FIL/05

Scientific Lexicons
Meaning, Reference, Truth

Tesi di
Marco Marletta

Referente di indirizzo:
Prof.ssa Francesca Piazza

Tutor:
Prof. Francesco La Mantia

CICLO XXVI – Anno di conseguimento titolo 2016

Index

Introduction

Medieval scientists used to believe that, when metal was heated, it lost his metal-like properties (brilliance, luster, cohesiveness and so on), due to the loss of a principle dubbed “quicksilver”; the process through which a metal loses its quicksilver was called “calcination” and the result (the metal without quicksilver) “calx”. At the same time, scientists held that the process of combustion consisted in the loss of another principle: “sulfur”, which makes substances such as wood and charcoal solid and combustible. Many years later, in the 18th century, George Ernst Stahl discovered that there is only one principle involved in calcination and combustion: he observed that a calcinated metal can be recovered by heating the calx in presence of charcoal and baptized this principle (possessed by both metals and charcoal) “phlogiston”, which was supposed to be emitted both by bodies undergoing combustion and by heated metals. Several phenomena were thought to be related to the presence of the absence of phlogiston. For example, from the fact that the flame of a candle in a closed container dies out before the candle is consumed, it was inferred that air can absorb a limited amount of phlogiston and from that point on, the burning body cannot emit phlogiston. Moreover, scientists observed that, if one puts an animal in the container after it is saturated with phlogiston, it dies because it cannot breathe; and what was inferred from this experiment is that respiration consists in removing phlogiston from the lungs and that phlogiston does not support life.

In order to explain observed phenomena, scientists deploy terms that are expected to individuate entities that cannot be directly observed, in fact or in principle. For example, that the maximum “kinetic energy” of a pendulum occurs at the lowest point of its swing, or that the “entropy” of a closed system decreases if we put work into it, or that “phlogiston” does not support life. Over the course of the history of science, the sets of beliefs, theoretical descriptions and intended applications associated with such terms widely changed. In some cases, for example “mass” or “time”, we continue using the same terms, even if the theories by which they had been introduced have been modified or even rejected. In other cases, like “phlogiston”, the term is no longer in use. Philosophers of science have been interested in many question concerning the status of those terms: for example, they ask to what they refer (if anything), what is their extension, what we are talking about when we use sentences in which they occur, whether they have a meaning (and what determines it) or whether they have intension. Moreover, the meaning and the reference of scientific terms have often been expected to be relevant to the truth of the sentences in which they are used: referential success (the fact that the entities singled out by theoretical terms exist) is often considered a basic *desideratum* for any true scientific sentence. It is hard to imagine how a sentence such as "phlogiston does not support life" can be true, once we have acknowledged that it does not exist. But, on the other hand, as the No Miracle Argument claims, it is quite intuitive that scientific theories owe their empirical success to the fact that they individuate some essential features of the world with good approximation; otherwise, the success of their empirical predictions would be a "miracle". And, since the phlogiston theory does explain many phenomena in the chemical field, perhaps we should believe that it is true, in some sense, even if the term "phlogiston" (or "dephlogisticated air") is non-referential. Most realists would accept at least some of the following claims:

- mature and successful scientific theories are approximately true;
- the main terms of mature and successful scientific theories are genuinely referential;
- the (approximate) truth of a theory is an adequate explanation of its empirical success;
- the (approximate) truth of a theory is the only explanation of its empirical success;
- a scientific theory may be approximately true even though its terms are referentially empty;
- the history of mature and successful theories shows improving approximation to the truth;
- the theoretical claims of scientific theories should be literally construed, as true-or-false;
- scientific theories make genuine existential claims;
- the empirical success of a theory constitutes evidence for the referential success of its terms;
- the aim of science is a literally true account of its intended domain of phenomena.

(Leplin 1984: 1-2)

All these claims take for granted an overall realist perspective that I am not going to put in question throughout my work. Specifically I am taking for granted that there is a strict relation between empirical success and approximate truth, even if it is far more complex than the No Miracle Argument. Rather, in my work, I am going to defend a theory about the relation between meaning, reference and truth of scientific terms and my starting point is Thomas Kuhn's theory of scientific lexicons or lexical structures. A lexical structure is a set of inter-defined terms, introduced together by means of the same theoretical laws and examples of application. For example, the terms “mass”, “force”, “acceleration”, “weight” constitute a lexical structure, introduced by the laws of classical mechanics (the laws of motion, the law of gravitation, Hooke's law) and a set of exemplary (paradigmatic) applications (inclined plane, simple pendulum, the planetary system and so on). So, a theory is constituted by a mathematical core and a domain of intended applications, which is not intensionally or extensionally given, but only by a list of exemplary applications (accepted by the relevant scientific community). By means of this concept, I will approach the questions that I have asked in the above list, but my work is not intended as a historical reconstruction of Kuhn's ideas. Establishing whether Kuhn was realist or anti-realist, relativist or anti-relativist or whatever is out of the bounds of this work. Moreover, as regards the main question of my work, i.e. whether the lexical structures theory entails the rejection of the correspondence theory of truth, my answer totally diverges from Kuhn's one. In the end, I will use (and sometimes modify) his theory of meaning and reference of scientific term to face problems that are actually discussed by the philosophers of science, especially concerning the concepts of selective confirmation and truth.

In fact, even though Kuhn has been the first to arise the most important of these problems (referential change, pessimistic induction and so on), in the meanwhile the debate has evolved and several intriguing concepts have emerged; in particular, I am talking about the concepts of selective (or preservative) confirmation and deflationary truth. The former points out that not only the truth, but also the approximate truth of a theory can be a sufficient explanation of its empirical success. To that extent, we should look for the parts of scientific theories that are responsible for their empirical success and they are usually supposed to be those parts that are preserved over the course of the history of science. The latter affirms that the truth is a property without nature or essence and all we can say about it is exhausted by the Equivalence Schema: “p” is true if and only if p”; therefore it has no explanatory power. Thus, we cannot use the truth to explain the empirical success of science and to state that the truth of a theory should entail its empirical success. The relation between selective confirmation and deflationism goes as follows. Selective confirmation replies to pessimistic induction, which aims to undermine the inference from empirical success to truth resting on the fact that many successful scientific theories were proven to be false (like the phlogiston theory); on the contrary, selective realism is more modest, since it says that only some parts of the theories were responsible for their empirical success and that their preservation is sufficient to save the inference from empirical success to truth (for example, the laws of motion are preserved as limiting cases in Einstein's theory, even though Newton's theory is false, strictly speaking). On the other hand, deflationism attacks the other side of the inference (from truth to success), since it claims that truth is not a sufficient explanation of any phenomena, because it is not an effective property. The main problem is to justify the inference from empirical success to truth and the other way round from a “partial” perspective, using a concept of truth which mirrors that of selective confirmation: an approximate truth not committed to a one-to-one correspondence between theoretical entities and “real entities”; and therefore consistent with the fact that reference is not a direct or neutral relation between name and object.

The main theses that I will defend in my work are that: 1) the reference of scientific terms is a function of the lexical structure they belong to (it is not language-independent or theory-independent); 2) the meaning of scientific terms is structurally fixed (through the mutual relations between the nodes of the structure) in a contextualist way (through the applications of the theory by which they are introduced); 3) *pace* Kuhn, this does not imply that the correspondence theory of truth it is not applicable to scientific knowledge (at least in the weak variants that do not assume an isomorphic relation between propositions and facts). For this purpose, my work is dived in three parts. Part 1 deals with the relation between meaning and reference, following and improving Kuhn's perspective; Part 2 analyzes the reasons (related to the theory of scientific lexicons) which urged Kuhn to reject the correspondence theory; Part 3 is devoted to the defense of a weak correspondence theory which is consistent with the perspective outlined in the previous parts.

In Part 1 I will claim that the relation between scientific terms and objects is not a one-to-one relation, but, rather, it involves the structural relations between different terms, applications and problem situations. Their meaning is determined by this relation, while reference is a function of the structure. At first, I will analyze how objects are singled out by paradigms, especially referring to the constitutive role played by the combination of physical laws and concrete applications (interpreted mathematical structures). Then, I will focus on the theories of meaning, adopting a perspective characterized by meaning eliminativism, structuralism and contextualism, which can account for meaning shift and the distinction between co-referential changes (which do not affect interpreted structures) and substantial changes.

The main point of Part 2 is that, contrary to what Kuhn himself believed, the theory of meaning and reference defended in Part 1 does not imply the rejection of the correspondence theory of truth; and, in particular, it focuses on the role of exemplary applications in scientific lexicons. Firstly, I will further analyze the relation between mathematical core and concrete applications, in order to individuate those parts of scientific theories which are essential for their application and empirical success (referring in particular to the process of scientific training). Moreover, I will focus on why this theory of reference is supposed to be an objection against the correspondence theory, by means of the incommensurability thesis and the comparison objection. My argument is that those objections may apply only to strong correspondence theories.

Finally, in Part 3, the claim is that weak correspondence is consistent with the theory of scientific lexicons and that, additionally, the concepts of exemplary application and interpreted structure can constitute evidence for the correspondence theory. By "weak correspondence", I mean that the truth is a property with (minimal) explanatory role, which expresses a relation between linguistic and non-linguistic entities (which make propositions true), but does not postulate a direct relation between propositions and facts, a unitary account of the correspondence relation and treats the facts in a deflationary way. The main problems that weak correspondence solves are: the comparison objection; the lack of a unitary, context-independent correspondence relation; the use of concrete lexical structures rather than models and uninterpreted mathematical structures; the inference from truth to empirical success, from the perspective of the approximate truth of the theories.

Part 1: Meaning and Reference

The main problem that I will face in the first part of my work is the relation between meaning and reference of scientific terms, especially referring to the language-dependent nature of the relation between scientific languages and the world. In order to do this, I will discuss, clarify and improve the perspective presented by Thomas Kuhn in his latest works. To that extent, this is the part of my work interested in historical questions about Kuhn's ideas, for the most part. Sections 1 and 3 are intended as historical reconstructions of his theses, while section 2 aims to put them on the table of the current debate on scientific realism, referential change, pessimistic induction and so on. Finally, section 4 is both historical and theoretical, since it tries to modify some of Kuhn's claims to express that theory of meaning in a clear and satisfactory way. The main claim of this part is that the relation between scientific terms and objects that populate the world is not a one-to-one, neutral or direct correlation, but, rather, it involves the structural relations between different terms, applications and problem situations. Roughly, the meaning of scientific terms is determined by the relations (*lexical structures*), while their reference is language-dependent, since it is a function of the relevant lexical structure. For this purpose, in the first part (sections 1-2) I will analyze how objects (if any) are singled out by scientific theories and paradigms, concluding that there is no language-independent way to achieve this aim. In section 1 I will summarize Kuhn's conception of physical laws, on the basis of Wittgenstein's and Kant's ideas that influenced him, arguing that symbolic generalizations (structurally construed) play a constitutive role in the constitution of the world of science and in the individuation of the entities that we recognize in it. In section 2 I will discuss how the structural access to the experimental processes is related to scientific realism, and especially selective realism, which aims to find out those parts of scientific theories that cannot be rejected without turning into a scientific revolution: from this perspective, interpreted mathematical structures are supposed to suffice. Moreover, in the second part (sections 3-4) I will specifically focus on the relation between meaning and reference, outlining a theory of the meaning of scientific kind terms. At first, in section 3, I will criticize the causal theory of reference by means of Kuhn's arguments against it: I will show that the causal theory cannot account for the relation between mathematical core and applications and, focusing on co-referential terms, it trivializes the concept of reference. Finally, in section 4, I will summarize the theory of meaning I am defending, concluding that it is characterized by meaning eliminativism, contextualism and structuralism. The main advantage of this theory is that it can deal with meaning shifts in the history of science without asserting the existence of all entities postulated by the theory.

Section 1: Analytic/Quasi-Analytic/Syntetic A Priori. Kuhn's Kantianism

1. Introduction

In his analysis of the structure of scientific paradigms, Thomas Kuhn states that we should not consider physical laws "laws", but rather "law sketches". These laws (for example $f = ma$ or $I = V/R$) constitute the mathematical core of scientific theories and allow scientists to deal with them as mathematical structures, justifying the application of logical tools and manipulations. But the expression "law sketches" means that such equations do not specify how scientists are expected to apply them to the empirical context and puzzle-solving. Scientific symbolic generalizations change case by case: $f = ma$ is transformed in $mg = (d^2s / dt^2)$ for the case of the free fall, or in other ways for the simple pendulum. Kuhn means that symbolic generalizations are incomplete in so far as they are separated from the specific context to which they apply; that is to say, from paradigms strictly construed. From this perspective, paradigms are exemplary problem solutions (for example, the simple pendulum or the inclined plane), which specify how physical laws attach to nature, through concrete examples of application. The empirical content of scientific theories consists in the combination of laws and paradigms: this is why, throughout his works, Kuhn refers to physical laws as analytic, quasi-analytic or synthetic a priori. Here I will analyze Kuhn's conception of such laws and I will focus (following the expression "synthetic a priori") on the Kantian and Wittgensteinian legacy that influenced Kuhn. Although Kuhn's Kantianism has already been investigated (Hoyningen-Huene 1993), I think that a more detailed examination of his concept of "synthetic a priori"[1] can clarify some points concerning scientific realism

1 Hoyningen-Huene briefly refers to Kuhn's synthetic a priori in (Hoyningen-Huene 1993: 211); see

and the relationship between paradigms and reality.

In fact, everyone knows that Kuhn claims that "the world changes over the course of scientific revolutions". Against the constructivist and anti-realist interpretations of the "world changes" thesis, Hoyningen-Huene demonstrated that we can identify two meanings of the word "world" (and "nature") (Hoyningen-Huene 1993: 31-36) coexisting in Kuhn's works (especially in *The Structure of Scientific Revolutions*). On the one hand, "world" means something that changes over the course of the history of science, like Kant's "nature in the material sense" or "aggregates of appearances". Roughly, it is the "phenomenal world" and epistemic subjects are co-constitutive of it. On the other hand, the word "world" means also something which remains uninfluenced by radical theory changes. It is similar to Kant's "thing-in-itself", unknowable by direct perception and scientific theories. The world-in-itself is independent from epistemic subjects, while the phenomenal world consists of the interaction of the objective features with the subjective ones.

In the constitution of the phenomenal world, synthetic a priori propositions play a major role. In the following section I will employ Wittgenstein's concept of "grammar" to clarify Kuhn's Kantianism.

2. Analytic, quasi-analytic and synthetic a priori propositions

In the introduction I have sketched Kuhn's attempt to distinguish between symbolic generalizations and exemplary problem solutions. Such a distinction moves the empirical content of scientific laws to exemplary solutions (or paradigmatic applications). This is the basic reason which urges Kuhn to describe such propositions as "analytic", that is to say (as a first approximation) "non-falsifiable"; every state of affairs is consistent with such universal laws. As we will see further below, Kuhn's target is the ultimate structure of Newton's mechanics. The famous astrophysicist Arthur Eddington had already affirmed that the first law of motion can be expressed as follows: "every body continues in its state of rest or uniform motion in a straight line, except in so far as it doesn't" (Eddington 1929: 124). As everyone knows, the correct sentence states "except in so far as it may be compelled to change that state by impressed forces". Eddington provides many examples of possible refutations of the first law of motion,

also Irzik, Grünberg (1998).

which are rejected on the basis of the existence of invisible forces which influence moving bodies (for example frictional resistance, gravitation and so on). Clearly, this does not mean that Newtonian physicists were wrong in defending their theory against alleged falsifications. It means only that, from a logical viewpoint, no experimental process can directly falsify the fundamental laws of motion. Some philosophers (for example the Popperians) might reply that this depends on "normal" scientists' dogmatism and that it is a problem which threatens to impede progress (Lakatos, Musgrave 1970).

On the contrary, I will argue that these propositions are not falsifiable not only for the dogmatic attitude of scientists, but also for the structural role they play in scientific theories. Although Kuhn emphasizes this property of scientific laws in the "Postscript-1969", since *The Structure of Scientific Revolutions* he has noticed that normal science is characterized by some propositions which present a dual nature, both analytic and synthetic. Here he focuses on the analytic nature of symbolic generalizations, i.e. their ability to resist empirical refutations, which makes them look like necessary propositions (Kuhn 1970: 78). Kuhn refers to them as "purely logical statements" or "tautologies" (Kuhn 1970: 78, 131-133). He recalls Newton's second law of motion and says that: "though it took centuries of difficult factual and theoretical research to achieve, [it] behaves for those committed to Newton's theory very much like a purely logical statement that no amount of observation could refute" (Kuhn 1970: 78). Later, Kuhn comes back to this question: he analyzes Dalton's work on the chemical law of fixed proportions and affirms that Dalton's thesis that atoms could only combine one-to-one or in some other simple whole number ratio

> enables him to determine the sizes and weights of elementary particles, but it also made the law of constant proportion a tautology. For Dalton, any reaction in which the ingredients did not enter in fixed proportion was *ipso facto* not a purely chemical process. A law that experiment could not have established before Dalton's work, became, once that work was accepted, a constitutive principle that no single set of chemical measurements could have upset. (Kuhn 1970: 133)

Nevertheless, Kuhn's emphasis on the analytic nature of universal laws does not allow us to figure out another important feature of symbolic generalizations. Indeed, from a different viewpoint, these propositions seem more synthetic than analytic: we cannot consider them a mere product of conventional stipulations and arbitrary definitions, but, rather, the result of both “factual and theoretical research”. This is why in the seventies Kuhn describes universal laws as “quasi-analytic” propositions (Kuhn 1977: 304 n. 14 and Kuhn 2000: 187 fn. 17). The question is explicitly linked to the identification of the hard-core of scientific theories and to the distinction between normal and revolutionary changes in the history of science: “I suspect that, quite generally, scientific revolutions can be distinguished from normal scientific developments in that the former require, as the latter do not, the modification of generalizations which has previously been regarded as quasi-analytic” (Kuhn 1977: 304 fn. 14). Or, to be more precise:

> The problem of distinguishing between a core and an extended core has a close counterpart in my own work: the problem of distinguishing between normal and revolutionary change. I have here and there used the term ‘constitutive’ in discussing that problem too, suggesting that what must be discarded during a revolutionary change is somehow a constitutive, rather than simply a contingent, part or the previous theory. The difficulty, then, is to find ways to unpacking the term ‘constitutive’. My closest approach to solution, still a mere *aperçu*, is the suggestion that constitutive elements are in some sense quasi-analytic, i.e. partially determined by the language in which nature is discussed rather than by nature *tout court*. (Kuhn 2000: 187 fn. 17)

Kuhn’s description of quasi-analytic propositions is supposed to provide a distinction between normal and revolutionary changes. A scientific change is revolutionary (i.e. two paradigms are incommensurable) if and only if the change involves quasi-analytic propositions. Since such propositions define their own terms, a modification in the tautologies results in a meaning change.

In his works of the eighties, Kuhn acknowledges that the expressions “analytic” and “quasi-analytic” were misleading and inappropriate (Kuhn 2000: 212). Symbolic generalizations are, to some extent, necessary (I will face this problem in Part 2 – section 1), but we should make some clarifications. The second law of motion cannot be considered a tautology *tout court*, at first because its terms (‘force’, ‘mass’) are not independently available for use in a definition of the other; moreover, while analytic propositions cannot be tested, the second law can be tested. Anyone can measure forces and masses (according to their Newtonian meanings) and apply his results to the second law form; in this way, one might demonstrate the falsity of this law. Rather, the necessity of symbolic generalizations is language-dependent: a revolutionary change (i.e. a change in quasi-analytic propositions) is a meaning change and no substitute for the second law of motion could be used (without giving up Newton's language).

In order to fix the vagueness of his previous exposition, in his latest works Kuhn uses the Kantian expression “synthetic a priori” to describe the role played by universal laws in scientific practice (Kuhn 2000: 71, 73-74 fn. 19, Kuhn 1990: 306, 317 fn. 17). The description of the analytic-synthetic nature of universal laws manifests the need for a new definition of the language (of science)-world relation:

> Using the Newtonian lexicon, the statement “Newton’s second law and the law of gravity are both false” is itself false. Furthermore it is false by virtue of the meaning of the Newtonian terms ‘force’ and ‘mass’. But it is not – unlike the statement “Some bachelors are married” false by virtue of the *definition* of those terms. The meaning of force and mass are not embodiable in definitions but rather in their relation to the world. The necessity to which I here appeal is not so much analytic as synthetic a priori. (Kuhn 2000: 73-74 fn. 19)

Now, Kuhn provides a new analysis of the Newtonian terms of the second law of motion and their relationships with the empirical content of the law (Kuhn 1989: 58-89 and Kuhn 1990). Kuhn says that physics students are usually introduced to the second law as a description of the behavior of moving bodies; but the law and the term ‘mass’ are acquired together. Thereafter the second law can be used to supply the missing measure of force (which has already been introduced, through the description of the dynamometer), since mass is proportional to acceleration under the influence of a force. Once students have learned the second law and the term ‘mass’, we can introduce the law of gravity as an empirical regularity, showing that the mutual attraction between bodies is proportional to the product of their masses. Now we can also establish some expects of the Newtonian use of ‘weight’, as a relational property dependent on the presence of two bodies. Unlike mass, weight is variable and his variation can be captured by the spring balance[2].

However, Kuhn presents also a second way of introducing the terms ‘mass’ and ‘weight’. It begins with the introduction of the term ‘mass’ (intended as ‘gravitational mass’) and describing gravity as a force of attraction between bodies proportional to their masses. So, once mass has been stipulated, weight is considered a relational property, the force resulting from gravitational attraction. Now we have gained knowledge about the terms ‘mass’ and ‘weight’ and finally we can introduce the missing

2 A spring balance measures weight (it differs from one location to another), while pan balance measures mass (it depends on the body and is constant in every place).

component of the theory (the second law of motion) as empirical law.

> The two routes thus differ in what must be stipulated about nature in order to learn Newtonian terms, what can be left instead for empirical discovery. On the first route the second law enters stipulatively, the law of gravitation empirically. On the second, their epistemic status is reversed. In each case one, but only one, of the laws is, so to speak, built into the lexicon. I do not quite want to call such laws analytic, for experience with nature was essential for their initial formulation. Yet they do have something of the necessity that the label 'analytic' implies. Perhaps 'synthetic a priori' comes closer. (Kuhn 2000: 71)

Scientific laws have both an empirical and a conventional nature[3]. But these two features are inextricably interwoven: the empirical content of symbolic generalization is not absolute (it does not depend only on the empirical basis), but, rather, it is relative to which parts of the theory scientists consider empirically testable. There is no sharp distinction between descriptive and conventional propositions: "empirical content must enter formalized theories from the top as well from the bottom" (Kuhn 1977: 300). So, for example, you may study radio sources (like supernovas, quasars and so on) by means of radio telescopes, assuming that they work in accordance with the laws of electromagnetic radiation; to that extent, the term "radio source" is theoretical referring to electromagnetism. And the same applies to optics, but this does not mean that radio sources are theoretical relative to other fields of scientific investigation.

Irzik and Grünberg analyze Kuhn's Kantianism referring to Whorf's linguistic relativity. They affirm that Whorf and Kuhn agree at least about three basic points: 1) language structures thought and experience; 2) the categorization of the world depends on language; 3) radically different languages give rise to different sciences (Irzik, Grünberg 1998: 215). I understand Irzik and Grünberg's point and sometimes Kuhn considered himself a Whorfian (Kuhn 1999: 34). But I think also that the thesis of linguistic relativity can lead to a misunderstanding about the dual nature of scientific laws that Kuhn investigates. On a Whorfian basis, one may associate Kuhn with the relativist ideas of Feyerabend (1975)[4], since, above all, it draws the attention to the *a priori* nature of scientific lexicons, while we are dealing with their *synthetic a priori* nature. For these reasons, I prefer to use a different linguistic model to analyze the relationship between paradigms and reality: Wittgenstein's grammar.

3 The same remarks apply to the analysis of Ohm's law as well (Kuhn 1977: 303-304).

4 Note that Feyerabend refers to Whorf much more often than Kuhn. Except the above passage, the only references to Whorf in Kuhn's works are Kuhn 1970: vi and Kuhn 1977: 258.

In fact, some scholars have already noticed that the role played by symbolic generalizations in scientific practice according to Kuhn is similar to which played by the standard meter and color-samples in language games according to Wittgenstein (Malone 1993, Baltas 2004). In his investigation of the synthetic a priori nature of the second law of motion, Kuhn himself quotes Wittgenstein; he asks whether Newtonian mechanics could withstand the revision of the second law, of the third law, of Hooke's law, or the law of gravity and he answers: "these are not questions that individually have yes or no answers. Rather, like Wittgenstein's 'Could one play chess without the queen?' they suggest the strains placed on a lexicon by questions that his designer, whether God or cognitive evolution, did not anticipate its being required to answer" (Kuhn 2000: 72).

In fact, like Kuhn's symbolic generalizations are grounded in both scientific language and reality at the same time, Wittgenstein's samples-rules or units of measurement are pieces of extra-linguistic world captured by language and used in language to speak about the world. Roughly, the difference between descriptive propositions and propositions that present samples can be explained as follows: we use to ask if a given body is really a meter long; but it is meaningless to ask if the standard meter is really a meter long. The truth-value of sample-propositions cannot be determined by world experience without falling into an infinite regress. A proposition such as "this is the standard meter" shows a sample and, like tautologies and equations, it is not a description of the world (a candidate for truth or falsehood), but rather the formal matrix of empirical truth-value attributions. After the *Tractatus*, Wittgenstein defines pseudo-propositions as "grammatical rules". Saying that something is a unit of measurement implies showing some grammatical rules.

> The rules of grammar are arbitrary in the same sense as the choice of a unit of measurement. But that means no more than that the choice is independent of the length of the objects to be measured and that the choice of one unit is not 'true' and another 'false' in the way that a statement of length is true or false. Of course that is only a remark on the grammar of the word "unit of length". (Wittgenstein 1974: 185)

Here arbitrary does not mean "merely conventional", or better perhaps, it is a kind of conventionality very similar to which Kuhn attributes to symbolic generalizations; here arbitrary means non-testable through normal experience (Wittgenstein 1974: 186-187). Thus, it points out the same sense of language-related necessity that Kuhn attributes to analytic, quasi-analytic and synthetic a priori propositions: “the only correlate in language to an intrinsic necessity is an arbitrary rule. It is the only thing which one can milk out of this intrinsic necessity into a proposition” (Wittgenstein 1958: 116). Finally, another brief clarification has to be made. We have seen that, for Kuhn, empirical content is not absolute and synthetic a priori propositions can be considered empirical or conventional depending on the accepted interpretation of the theory. In the same way, according to Wittgenstein, there is no ultimate distinction between descriptive and grammatical propositions: his aim is not to propose again the old analytic-synthetic dichotomy. There are not two *kinds* of propositions, but two different *uses*: describing the world and showing a rule. These uses are linked to different language games: a statement is empirical or grammatical only within a language game. These remarks introduce a new conception of the relationship between language and reality (Wittgenstein 1958: 89), to some extent consistent with Kuhn’s conception that I am going to analyze with the help of the other philosophical reference of Kuhn: Kant.

3. The conditions for the possibility of experience

I have recognized some analogies between Kuhn’s theory of symbolic generalizations and Wittgenstein’s theory of grammatical propositions. To sum up, the truth-value of this sort of propositions is related to definitions and meaning, but it is not the kind of conventionality which, like analytic propositions, makes true a proposition in virtue of its internal properties. Moreover, like synthetic propositions, they allow us to tell something about the world, rather than about other words; but, unlike synthetic judgments, their predictions cannot be empirically falsified. So, the first result I have achieved about Kuhn’s Kantianism is that universal laws and symbolic generalizations are not falsifiable because they constitute the grammar of scientific practice.

Nevertheless, in the analysis of Kuhn’s use of the expression “synthetic a priori”, a question arises on the relation between (scientific) language and reality. We have seen that the synthetic a priori nature of symbolic generalizations implies a complex combination of theoretical and experimental research. Now, that is because the “world of science” (i.e. the object of scientific knowledge) is the result of the combined action of reality and paradigm. The way paradigms contribute to determine experience is the Kantian legacy inherited by Kuhn.

Actually, Kuhn does not affirm only that observation is theory-laden. Rather, following Kant, he states that the taxonomic structure of paradigms (Hacking 1993, Chen 1997, Sankey 1998 and Massimi 2015 for a clarification of the taxonomic notion of paradigm) provides a structure for possible experience: "insofar as the structure of the world can be experienced and the experience communicated, it is constrained by the structure of the lexicon of the community which inhabits it" (Kuhn 2000: 101). Kuhn acknowledges that the claim that scientific lexicon is a structure which constraints experience is a Kantian idea, since the lexicon provides us with preconditions of possible experience, just like Kantian categories. Or, in other words:

> My structured lexicon resembles Kant's a priori when the latter is taken in its second, relativized sense. Both are constitutive of *possible experience* of the world, but neither dictates what that experience must be. Rather they are constitutive of the infinite range of possible experiences that might conceivably occur in the actual world to which they give access. Which of these conceivable experiences occurs in that actual world is something that must be learned, both from everyday experience and from the more systematic and refined experience that characterizes scientific practice. (Kuhn 2000: 245)

The most important difference between Kuhn and Kant about the nature of the "a priori" is that, while, for Kant, it justifies the universality and necessity of knowledge, for Kuhn, the "a priori" is supposed to be relativized. The scientific experience of the world is determined by taxonomic schemes, so that the world of science changes as well as the paradigms[5]: there is a plurality of potential worlds of science. Indeed, Kuhn presents the worlds of science just like a plurality of possible worlds in the history of science. Kuhn, like Kripke, considers a possible world a world which is stipulatable in some languages (Kuhn 2000: 64); in other words, a possible world is a world conceptually accessible. This difference is very obvious since, according to Kuhn, paradigms are historical things and many paradigms can accommodate the same set of experimental data. But this entails a second difference as well. Kant's "a priori" is transcendental: it is necessary and universal and grounds the possibility of experience.

5 This is Kuhn's ontological relativism: the thesis that the world changes during scientific revolutions (Kuhn 1970: 111-135). Sankey presents it as follows in his taxonomic interpretation: "The taxonomic structure of the phenomenal world of a theory depends on the categorial scheme employed by the theory. As a result, the phenomenal worlds of scientific theories associated with different categorial schemes contain divergent systems of natural kinds. Thus, the set of natural kinds constitutive of the phenomenal world of a theory depends on the categorial scheme of the theory. Given that such phenomenal worlds vary relative to the categorial scheme of operative theory, the existence of a set of natural kinds which populates the phenomenal world of the scientist is therefore a form of existence which is relative to prior choice of scientific theory." (Sankey 1997b: 316).

Instead, for Kuhn (and for Wittgenstein) paradigms are not transcendental forms: they are immanent to human practices and this rules out the possibility of a metaphysical foundation of scientific knowledge.

The removal of the distinction between "possible world" and "world conceptually accessible" automatically eliminates the "world-in-itself" from the specter of possible worlds. Lexical structures are, to some extent, co-constitutive of possible worlds: being a possible world means being accessible by some languages. In this way the world-in-itself is not included in the set of possible worlds; that is because, by definition, it is conceptually inaccessible to any (scientific) community sharing a language. Kuhn's conception of the "thing-in-itself" is a controversial and ambiguous issue, so I will not specifically focus on it. He often affirms that the objective reality exists and is independent of our knowledge and language. But he also states that such a reality is almost or totally unknowable: his view is Kantian, but without "things-in-themselves" and with changing categories of the mind, which follow the accommodation of language and experience. But, for Kuhn, this view does not make the world less real (Kuhn 2000: 207). The point is that, as it is, the thing-in-itself may be useful only as a commonsensical defense against idealism and solipsism, since we posit its existence to account for our perception of the world and we assume its immutability in so far as we wish to rule out individual and social solipsism. In the end, he specifies that the thing-in-itself is something "ineffable, indescribable, undiscussibile" (Kuhn 2000: 104). The world-in-itself and the phenomenal world should remain separated because scientific knowledge would be pointless without a cleavage between them, but scientific lexicons contribute to determine scientific reality. We have seen that the world of science, in so far as it is partially determined by symbolic generalizations, is language-dependent. Now I can add that, according to the necessarily public nature of grammar (Wittgenstein 1958: 81), the phenomenal world can be considered a language-dependent world, but only from a social viewpoint. This clarification can explain some ambiguities about the existence of the mind-independent reality. The world changes over the course of scientific revolutions as far as paradigms are shared by a (scientific) community. To that extent, the metaphor of a mind-dependent world is misleading as well as the metaphor of a constructed world since "it is groups and group practices that constitute worlds (and are constituted by them). And the practice-in-the-world of some of those groups *is* science. The primary unit through which the sciences develop is thus, as previously stressed, the group, and groups do not have minds." (Kuhn 2000: 103).

Anyway, according to Kuhn, there is a structural relation between scientific language (its taxonomic structure) and its capability of giving us access to certain possible worlds. In other words, taxonomic categories provide a categorization of the "world" by some similarity-dissimilarity relationships. Kuhn uses the expressions "taxonomic structures", "categories", "kind terms" in his latest works. In the seventies the similarity-dissimilarity relationships by which the paradigm attaches to the world were analyzed by Kuhn through Wittgenstein's notion of "family resemblance" (which plays a major role in the process of scientific training: see Andersen 2000a). Basically, scientists have learned to master these family resemblances during their professional education: the structure of scientific taxonomies implies a linguistic way of working in a scientific world. Coming back to the previous example, in order to gain access to the Newtonian world, physics students must understand and use interrelated terms such as 'mass', 'weight', 'force' and the laws in which they occur. I will analyze this point in section 2 – part 2 – but, anyway, once a scientific community shares a lexical structure, it gains access to many possible worlds, which only now are available to observation and experimental verification or falsification.

Nevertheless, Kuhn stresses that different but compatible lexicon can make the same possible world accessible; but incompatible taxonomies determine different possible worlds: scientific communities possessing a lexicon or a structured vocabulary have access to the set of worlds that can be described through the relevant lexicon. But rival lexicons give access to different sets of possible worlds and these worlds are never entirely overlapping (even if they are usually partially overlapping). In fact, Kuhn's theory of the linguistic access to possible worlds does not mean that a possible world is conceptually accessible to any language: only certain languages create the conditions for the access to a possible world. He specifies that only the possible worlds conceptually accessible by the lexicon of a linguistic community are available for it:

> Only the possible worlds stipulatable in that language can be relevant to them. Extending quantification to include worlds accessible only by resort to other languages seems at best functionless, and in some applications it may be a source of error and confusion. […] At least in their application to historical development, the power and utility of possible-worlds argument appears to require their restriction to the worlds accessible with a given lexicon, the worlds that can be stipulated by participants in a given language-community or culture." (Kuhn 2000: 64-65)

Consequently, we have seen that, according to Kuhn, a given possible world can be the object of experimental investigation if and only if it is stipulatable in a given lexicon. That is because, like Kantian categories, scientific lexical structures are constitutive for the possibility of experience: without an appropriate lexical structure for the access to a possible world, the very field of possible experience collapses. As a result, like the access to possible worlds is language-dependent, possible worlds are language-dependent as well. However, these observations do not lead Kuhn to some sort of linguistic idealism. Kuhn does not abandon the idea of a mind-independent world. Rather, his targets are naive realism and truth as correspondence[6]: "If, as standard forms of realism suppose, a statement's being true or false depend simply or whether or not it corresponds to the real world-independent of time, language and culture – then the world itself must be somehow lexicon-dependent" (Kuhn 2000: 77). Again, taxonomic structures do not determine (*actual*) experience, but *possible* experience. A world is a set of possible states of affairs; and so we can consider an experimental fact (such as verification or falsification) the actualization of a possible fact in the world actually experienced by a given scientific community.

The truth-value of an empirical proposition is a question necessarily internal to a lexicon, since it can be conducted only with a lexicon already in place, and its outcome is influenced by the lexicon we are using. In so far as it is constitutive for the possibility of experience, a taxonomic structure is the matrix of any truth-value attribution within the experience field of its lexicon. But, precisely because it is a formal matrix, it is neither true nor false: "lexicons are not, in any case, the sort of things that can be true or false" (Kuhn 2000: 244). Within a lexical structure, propositions can be rationally justified or falsified, but lexical change is a pragmatic matter. In the same sense that we have seen referring to symbolic generalizations, the justification of a taxonomic scheme is, to some extent, something conventional.

It is important to precise again that 'conventional' does not mean 'arbitrary' *tout court*. The essential point is that the relation between scientific language and reality cannot be satisfied by the internal-external dichotomy. Since a paradigm provides the conditions for the conceptual access to the world of science (its grammatical structure),

6 The rejection of the correspondence theory of truth is an important and underrated problem for Kuhn, in particular in his latest works (see especially Kuhn 2000: 90-104). For a complete exposition see Bird 2000: 209-266. I will focus on that problem in Part 2 – sections 3-4.

from a Kuhnian viewpoint, it does not make sense to talk about paradigm and reality as independent of each other. Kuhn has often stated that knowledge of nature and knowledge of language are inextricably linked in the paradigm (Kuhn 1970: 110). As regards meaning changes during scientific revolutions, he explicitly says that these revolutionary changes within the categorial structures are about names or language as well as about nature; and these features cannot be sharply separated. That is because the taxonomic structures of paradigms is internally related to some knowledge of nature: "each of the lexicons […] embodies knowledge of nature" (Kuhn 2000: 74).

In his comment on Richard Boyd's realism (Kuhn 2000: 206-207), Kuhn considers unsatisfactory the traditional idea of an accommodation of language to the world. He thinks that it implicitly assumes the existence of one real world, largely unknown, but toward which science proceeds with increasing verisimilitude. Kuhn expresses his puzzlement as follows:

> What is the world, I ask, if it does not include most of the sort of things to which the *actual* language spoken at a given times refers? Was the earth really a planet in the world of pre-Copernican astronomers who spoke a language in which the features salient to the referent of the term 'planet' excluded is attachment to the earth? Does it obviously make better sense to speak of accommodating language to the world of accommodating the world to language? Or is the way of talking which creates that distinction itself illusory? Is what we refer as 'the world' perhaps a product of mutual accommodation between experience and language? (Kuhn 2000: 206-207)

The last question is clearly a rhetorical question. According to Kuhn, scientific development is a process of mutual accommodation between scientific language and reality: the results of these mutual accommodations are the worlds in which scientific communities live. The connection between language and nature is explained by Kuhn by means of the metaphor of a coinage with two faces: "the criteria relevant to categorization are ipso facto the criteria that attach the names of those categories to the world. Language is a coinage with two faces, one looking outward to the world, the other inward to the world's reflection in the referential structure of the language" (Kuhn 2000: 30). Now, on the basis of the previous observations, we can fully understand this metaphor. For what Kuhn says about synthetic a priori propositions and the conditions for the possibility of experience, scientific language embodies knowledge of nature, which is not falsifiable through experience. So, a paradigm change (a revolutionary modification in the synthetic a priori propositions) can only be holistic, because scientific experience does not exist outside the paradigm. In other words, the implicit knowledge which changes over the course of scientific revolutions consists in the similarity-dissimilarity relations which determine the field of possible experience and thanks to which normal science can take place. Scientists only now can check their empirical statements. Kuhn's theory affirms that two kinds of knowledge are combined and acquired together through scientific training: knowledge of words and knowledge of nature. In fact, physics students learn on the one hand the meaning of scientific terms (their use in experimental practice) and, on the other hand, the sort of things which populate the world and the respective behaviors. Finally, a distinction between two kinds of knowledge – knowledge of words and knowledge of nature – is misleading: "not really two sorts of knowledge at all, but two faces of the single coinage that a language provides" (Kuhn 2000: 31).

4. The world-language connection and the functions of normal science

In the last section I have argued that the concept of synthetic a priori entails a connection between world and language which is not localized in specific propositions (protocol statements, base assertions and so on); rather, the Kantian and Wittgensteinian conception of scientific knowledge determines a network of interwoven pieces of world and pieces of language, so that experience pervades whole scientific theories, both from the top and from the bottom. The referential function of scientific lexicons is not a neutral connection between language and the "external world" or what is "really there". Rather, from the neo-kantian perspective, the notion of reference has to be refined from an internalist viewpoint; i.e., the question "what do the real world consist of?" is meaningful only within a paradigm, or a grammar. This thesis does not refer to the language-dependence of scientific objects (the *referents* of scientific theories), since the existence of objects and their ontological autonomy and individuality deal with the world-in-itself. Rather, it refers to the language-dependence of the connection between paradigms and reality (the *reference* of scientific theories). The referential function of scientific languages depends on the lexical structure of the languages themselves. To individuate an object means to isolate it in experience, describing its spatio-temporal boundaries and distinguishing it from similar and different objects in its background. The emergence of objects comes from a segmentation of reality which allows scientist to discover the objects (in a given spatio-temporal position). This is a preliminary condition for referential success and is linked to the structure of the lexicon we work with. In fact we identify objects by means of coherent sets of properties (such as mass, acceleration, weight and so on in the previous example), that we detect through experimental practices and measurements. The idea underlying Kuhn's theory of lexical structures is that a single property is not sufficient to single an object out and that we should use a structural set of interrelated properties (Kuhn 2000: 58-89 and Kuhn 1990). This thesis is what is to be inferred from the above analysis of the hard-core of Newton's mechanics and is linked to the idea that we can change the structural relationships between the fundamental concepts of classical physics to accommodate the data in different ways and save the laws of motions against empirical falsifications.

Now, it should be clear what Kuhn means when he speaks about the mutual accommodation between language and experience. The possibility of scientific experience depends on the existence of paradigms, which give a structure to the "world of science", the phenomenal world that changes over the course of scientific revolutions. Paradigms influence such a world, since singling scientific objects out requires a set of structural relations provided by the lexical structure. Therefore, to some extent, nature adapts itself to the paradigm, although the existence of objects and their individuality is a metaphysical question, which does not deal with scientific knowledge; rather, paradigms deal with the epistemic conditions of science. On the other hand, paradigms adapt themselves to nature, since their structural nature (they are not composed of isolated concepts, but of clusters of terms that influence each other) enables meaning shifts, which make possible the accommodation of the experimental data. Such an accommodation is mutual because every change in the paradigm entails a feedback mechanism between paradigm and nature and the other way round. This process characterizes both normal and revolutionary science: meaning change is not an exclusive feature of revolutionary science, as I will claim in section 4. In fact, what is supposed to change is the set of synthetic a priori propositions. A revolutionary change is a scientific development which requires a radical change in propositions that have previously been regarded as synthetic a priori: "what must be discarded during a revolutionary change is somehow a constitutive, rather than simply a contingent, part or the previous theory" (Kuhn 2000: 187 fn. 17). Since we have seen that the distinction between synthetic a priori and empirical propositions is not qualitative (it depends on the role played by the proposition in the constitution of the theory), it follows that the distinction between normal science and revolutionary science is not as rigid as it is usually supposed to be. Just like revolutionary science, normal science allows significant changes, but the organization of the theory directs the "arrow of the falsification" toward its extended core (and not toward the core), even though the distinction between core and extended core is not so sharp.

In this context, an important issue in normal science is the function of measurement. Kuhn individuates three fundamental aims of normal science (Kuhn 1970: 25 ff.):

1) The determination of the classes of facts which are particularly revealing of the nature of things (according to the paradigm);
2) The determination of the classes of facts which can easily be directly compared with the predictions of the theory;
3) The empirical work to articulate the paradigm, solving its ambiguities and more and more original problems.

The third aim is the most important and the most representative of the development of normal science. It exactly consists in the mutual accommodation between paradigm and nature that I have outlined in the previous parts. In turn, the third aim can be divided in three classes of puzzle-solving activities:

3.a) the determination of physical constants (for example the empirical determination of Avogadro's number or Joule's coefficient and so on);

3.b) the formulation of quantitative laws (for example Boyle's law or Coulomb's law);

3.c) the application of the paradigm to new areas of interest.

Kuhn notes that, when scientists approach these normal problems, they do not look for the agreement between theory and experiment, but, rather, for the "reasonable agreement" (Kuhn 1977: 184) between paradigm and experimental data. "Reasonable agreement" means that the relationship between (scientific) language and world depends, as I have stated, on the paradigm itself. Scientists usually seem to be struggling with facts or to harmonize the facts with the theory and the theory with the facts.

5. Conclusions

In this chapter I have started introducing some problems of Kuhn's symbolic generalizations and I have recognized some functions of paradigms-synthetic a priori judgments, which constitute the experimental grammar of science and make possible the access to some portions of reality. My conclusion is that, according to the Wittgensteinian-Kantian model, this framework of the relation between language and world is Kantian in two ways: 1) it considers scientific paradigms the boundaries of objective experience (and not the other way round), although in a relativized and non-transcendental way; 2) it entails an "internalist" conception of the referential function of scientific language (so that we can speak of mutual accommodation between paradigm and experience and not of agreement between theory and reality). Finally, the main point is that universal laws of science are not falsifiable not only for the dogmatic attitude of scientists, but also, and above all, for their structure and function in scientific theories. Of course, all the problems I have dealt with in this chapter require important clarifications. The first problem is how they are related to the current debate on scientific realism I will focus on in section 2. Moreover, I shall specify how the internalist view of reference can address the problem of the relation between meaning and reference (of natural kinds) and I will do this in sections 3-4. Additionally, in this chapter I have focused mainly on symbolic generalization, while, in Part 2 – sections 1-2, I will explain how they are related to concrete applications. And finally, the most important problem (that I will face in Part 2 – sections 3-4 and Part 3) is whether Kuhn is right in saying that the ideas I have exposed in this chapter entail the rejection of the correspondence theory of truth.

Section 2: Selective Realism and the Concept of Reference

1. Introduction

In the last chapter I have focused on the relation between scientific paradigms and the world, starting with Kuhn's perspective; but his theses about this topic are actually out of fashion and the contemporary debate about scientific realism diverges from the points Kuhn raised. Therefore, I do not think that it would be helpful to ask whether Kuhn was realist or anti-realist in the terms of the actual debate and this is not the aim of this chapter. Additionally, I do not want to go into the details of scientific realism, since it goes beyond the scope of my work, but it will be fruitful to introduce some basic ideas that are at the heart of the discussion; basically, because they are useful to present some questions that I will discuss in the next chapters (reference, selective confirmation, mathematical structure, physical interpretation, approximate truth, pessimistic induction, causal theory of reference and so on) and they are a good tool to approach the language-dependence of reference (that I will analyze in Part 1 – sections 3-4). So, this chapter is not intended as an official stance on scientific realism. I will conclude that semirealism is a good option as regards the concepts of concrete structure and detection-auxiliary properties, but I am not committing myself to the metaphysics of semirealism. Actually, scientific realism is supposed to consist of three main theses (Psillos 1999, 2000):

1) *The Metaphysical Thesis*: the objective world exists and has a definite structure independent of the content of our knowledge and its organization (ontologically and causally speaking).
2) *The Semantic Thesis*: scientific theories are truth-conditioned descriptions of their intended domains; they should be literally construed as true-or-false.
3) *The Epistemic Thesis*: mature and successful scientific theories are approximately true.

The first thesis is a *modest* statement of scientific realism. It implies only that there is a mind-independent world that scientific knowledge tries to describe. This is a metaphysical thesis very hard to demonstrate, but I will take it for granted and I will not discuss it in depth. On the contrary my discussion is concerned only with the semantic thesis and the epistemic thesis. In fact, scientific realism is characterized by a *presumptuous* claim about scientific knowledge as well: it affirms that, although the world is largely independent of our cognitive abilities, science can succeed in providing an accurate description of it; that is to say, an approximately true description (see Wright 1992: 1-2 for a distinction between modest and presumptuous realism). Scientific realism is traditionally introduced in opposition to anti-realism, a set of theories that includes constructivism, skepticism, idealism, logical positivism, instrumentalism and many others. On the contrary, I will argue about other realist positions, which emerged after *The Structure of Scientific Revolutions*: entity realism, structural realism and semirealism. These theories have been dubbed "selective realism" (or preservative realism) and suggest that we should not believe everything we are told, i.e., we should not believe in all aspects of scientific theories (Chakravartty 2007: 29).

In the following sections I will briefly describe structural realism and entity realism and I will conclude that they are both incomplete positions which imply each other, in spite of appearance; and I will argue that semirealism can answer to some open questions of such theories. But I will also note that both entity realism and semirealism hold a causal theory of reference that I consider inappropriate (although entity realism assumes it in a non-literal interpretation and semirealism in a descriptivist interpretation). On the other hand, I think that structuralism can provide the framework for a suitable theory of reference, which takes into account the "Kantian" or "internalist" concept of reference determined by the lexicon that I have introduced in section 1.

Before we start, let me add the last preliminary clarification. The semantic thesis and the epistemic thesis deal both with the relationship between science and reality and with the relationship between science and truth. Here, I will focus only on the former and I will leave the latter question out. I think that scientific realism and truth are distinct issues and we have to deal with them separately. Therefore, I will discuss truth in Part 2 and Part 3 of my work.

2. From structural realism to entity realism

As a first approximation, structural realism is a theory that emphasizes the importance of relations and holds that "scientific theories offer faithful descriptions of reality" does not mean that they tell us something about the essence of objects; rather, they tell us something about the structure of the world. For example, "informally a structure is a system of related elements, and structuralism is a point of view which focuses attention on the relations between the elements as distinct from the elements themselves" (Redhead 2001a: 74). Structural realism focuses on the relation itself, rather than on the *relata*. It can be methodologically construed (structuralism concerns the nature of scientific theories: we should understand them as sets of models), epistemically construed (structuralism concerns what science can know: we cannot know what there is in the world, we can know only its structure) and ontologically construed (structuralism concerns what there is: the structure is all there is) (Psillos 2006: 560). The main variants of structural realism derive from the epistemic and the ontic versions: epistemic structural realism (ESR) and ontic structural realism (OSR) (see Ladyman 1998).

2.1 ESR and OSR

ESR. It holds that we have to consider the limits of scientific knowledge: the nature of the objects which constitute the world is out of the bounds of scientific practice. Maybe in the world there is more than the structure, but we can know only the structural aspects of reality and nothing about the things whose relations constitute the object of scientific knowledge. ESR has been introduced by Worrall (1989)[7]. Worrall analyzes the Fresnel-Maxwell historical case to argue that, from ESR's viewpoint, scientific progress is cumulative since, over the course of the transition from Fresnel's theory of light to Maxwell's theory of light, scientific progress preserves the *structure* of such theories, i.e. the mathematical core represented by Fresnel's equations about reflection and refraction. Fresnel was wrong about the *nature* of light, since he believed that it consists in vibrations transmitted through the ether. But he was right about the structure of some optical phenomena, since the structure of light described by his equations survives and it

7 A historical reconstruction of epistemic structural realism in (Chakravartty 2004: 152-154).

is common to Fresnel's and Maxwell's theories.

OSR. It holds that only structures exist and therefore we should believe only in the structures described by scientific theories. The concept of object is meaningful only from an "ordinary language" viewpoint, but it is philosophically and scientifically poor. The most widely discussed form of OSR has been presented in (French, Ladyman 2003). OSR proceeds from the analysis of a case study about quantum mechanics. In fact, every kind of scientific realism seems to assume the existence of objects, where "object" means something characterized by identity and individuality. But quantum particles deny this assumption. Consider the distribution of two indistinguishable particles (*a* and *b*) over two states. In classical physics we have four possible situations: 1) *a* and *b* in the first state; 2) *a* and *b* in the second state; 3) *a* in the first state and *b* in the second state; 4) *a* in the second state and *b* in the first state. We usually consider (3) and (4) equivalent (since they result from the permutation of the particles) and then the probability of having one particle in the first state and one particle in the second state is 1/2 (assuming that no combination is advantaged). On the contrary, quantum mechanics claims that a permutation of the particles does not generate a new situation. While in classical physics two particles are distinguishable although they are identical, in quantum mechanics two identical particles are indistinguishable. According to the "Principle of Indistinguishability", quantum mechanics must treat identical particles as indistinguishable: if a particle permutation P is applied to any state function for an assembly of particles, then there is no way of distinguishing the resulting permuted state function from the original unpermuted one by means of any observation at any time (French, Krause 2003: 99). Since in quantum mechanics the calculation of probability in experiments depends on the state function, a permutation does not produce any experimental difference. OSR appeals to this argument to demonstrate that quantum "objects" cannot be considered individuals.

2.2 Objections against OSR and ESR

OSR claims that, since the ordinary conception of "object" is not consistent with quantum objects, we have to reject the metaphysics of objects. But this argument is not convincing because it does not investigate all the possible conceptions of object. For example, Ladyman himself states that "objects are picked out by individuating invariants with respect to the transformations relevant to the context. Thus, on this view, elementary particles are just sets of quantities that are invariant under the symmetry groups of particle physics" (Ladyman, 1998: 42). This requirement is satisfied for at least one theory of objects, which is outlined by Chakravartty and that I will improve in the next sections. Additionally, Chakravartty claims that ESR is unsatisfactory as well. At first, it can be attacked because of its definition of structure. I have taken as my starting point the idea of structure as the fundamental mathematical equations of a theory. Such equations survive over the course of scientific revolutions or we use to consider them limiting cases of the new equations. In order to define "structure", ESR turns to Russell, who provides a very clear definition: "a class α ordered by the relation R has the same structure as a class β ordered by the relation S, if to every term in α some one term in β corresponds, and vice versa, and if when two terms in α have the relation R, then the corresponding terms in β have the relation S, and vice versa" (Russell 1948: 272). The core of this definition is that the members of α and the members of β may be absolutely different and the same idea applies to the relations R and S. Russell considers structure a high-order property, i.e. a property of relations. Roughly, high-order properties are properties of relations, while first order properties are properties of things. For example, bodies have masses and therefore "mass" is a first order property of a body. Moreover every body is heavier than other bodies and lighter than other bodies. "Heavier than" and "lighter than" are relations which depend on the property of "mass", since bodies are heavier or lighter than others because of the relation between their masses. The same applies to properties such as "longer than" or "shorter than". But the relations "heavier than" (or "lighter than") and "longer than" (or "shorter than") have also something in common: for example the property of "total ordering". Total ordering is a property of relations, i.e. a high-order property. However, although this definition of structure is clear, it can be challenged by Newman's objection (Newman 1928: 140): the fact that a system has a structure provides us with information about the cardinality of the system, but any collection of objects (if there are enough) can be arranged to correspond to a given structure (Demopoulos, Friedman 1985). The problem comes from the idea that only high-order properties constitute the domain of knowledge, because it is a consequence of second order logic that, given any set α and any relation T, there exists in α a relation having structure T (assuming that T is compatible with the cardinality of α). Moreover ESR can be attacked by Psillos' objection about the distinction between the form and the content of knowledge, i.e. between structure and nature (Psillos 1995, 1999). He claims that the dichotomy of structure and nature implies that the nature of scientific entities is separated from their structures (ESR states that objects are something above the structure of the theory). But, according to Psillos this is untenable because it is based on the metaphysical distinction between form and substance (Psillos 1995).

2.3 Concrete and abstract structures

Russell's definition of structure uses the concept of high-order property. But Redhead distinguishes abstract structures from concrete structures (Redhead 2001a). Abstract structures are the kind of structure described by Russell: formal properties of relations without the need for knowing first order properties. On the contrary, concrete structure refers to a relation between first order properties: a relation between properties of the things in the world. The concept of concrete structure implies a stronger realist commitment, since a relation between first order properties implies that the entities are correlated as well.

Replying to Newman's and Psillos' objections, Chakravartty proposes that structural realism should suggest that scientific theories are concrete structures. His theory appeals to the intuitive definition of structure as enumerating the parts of something and describing their relations and affirms that we can say that a structure is a relation (or set of relations) between first order properties (Chakravartty 2007: 39-40). The kind of properties we refer to are the quantitative and determinate properties emerging from scientific experimental practice (for example mass, charge and so on). This remark satisfies Newman's objection since, if structures are construed as concrete structures, knowledge is about the concrete. Two structures can share the same high-order, formal properties and, at the same time, be different concrete structures. Moreover, it satisfies also Psillos' objection since it does not hold the distinction between knowledge of the structure and knowledge of the nature. That is because first order properties whose relations constitute the structure are possessed by entities (which can or cannot manifest such properties at a given time) independently of any other circumstance. Knowledge of the structure entails knowledge of properties and their relations and therefore, since properties are first order properties of entities, it is also knowledge of entities. In such a way, we overcome the question of the distinction between the relation and the *relata* in structural realism. It is not true that structural realism must necessarily affirm that we cannot say anything about the existence and the nature of the elements of the structure. On the contrary, if we assume that structures are concrete structures, we will commit ourselves to the existence of some entities. But, to clarify in what way scientific entities exist, I will analyze another kind of selective realism, apparently opposite to structural realism: entity realism.

3. From entity realism to structural realism

Entity realism is a kind of selective realism since it aims to establish the conditions under which we can believe that the entities described by scientific theories exist in the world. It links existence to the causal contact with entities in experimental contexts. Basically, scientific experiments consist in the manipulation of something and the intervention in other things and therefore, since such things demonstrate to have causal power in our practices, we are forced to believe that they exist. Hacking states that “when we use entities as tools, as instruments of inquiry, we are entitled to regard them as real” (Hacking 1989: 578). He describes (Hacking 1983) the experiment that demonstrated the existence of electrons through the detection of fractional charges. In such an experiment electrons (and protons) are “sprayed” onto a metal sphere to neutralize the surplus charges present on the sphere. Scientists succeed in using electrons to achieve this neutralization and therefore we can conclude that they use electrons to manipulate the behavior of the sphere. These considerations lead Hacking to his famous slogan “if you can spray them, then they are real”. This is vague, but tries to save science against the issues related to theory change. In fact, if we associate ontological commitment with the theoretical statements of scientific theories, we should modify our commitment for every theoretical change. On the contrary, entity realism maintains only that we are expected to believe in the entities involved in experimental causal chains, without any requirement about the theoretical background of such experiments. Our beliefs about the existence of scientific entities are justified without recurring to theories. We continue referring to the same entities although some theories about them may be false (or have been falsified). Structural realism and entity realism apparently contradict each other. This is because structural realism is skeptic about entities and realist about theories, while entity realism is skeptic about theories and realist about entities; therefore, they might appear very different. But in the last section I have argued that structural realism can be emended by means of a certain kind of entity realism; now I will argue that entity realism entails a kind of structuralism.

3.1 Objections against entity realism

Entity realism has been harshly criticized by several philosophers[8]. The first objection is that it is very vague. It does not provide a clear criterion to figure out which things exist, among those we find in experimental practice; and, above all, what does it mean that such entities exist? It states that existence consists in having a role in causal experimental chains, but how can we define the concept of causal chain? Does every experiment entail a causal chain? Are there degrees of causality that allow us to believe more faithfully in specific entities? Moreover, entity realism distinguishes between ontological (or existential) claims about entities and theoretical claims. This distinction may be right, but it does not make sense to speak about knowledge of individual isolated entities (Chakravartty 2007: 31). Knowledge of entities necessarily implies knowledge of at least some relations that such entities maintain with other things: for example, the relations with the other entities emerging in the same experiments and the relevant properties; or the relations with the instruments of measurement, manipulation and detection that scientists use to achieve the results of the experiments. Entity realism needs structures, because structures are indispensable in order to identify the very entities to which entity realism is committed. Structures require entities since structures consist in relations between first order properties; but, at the same time, entities require structures since structures allow us to detect the entities whose properties constitute structures. But this is not sufficiently clear yet, because it does not reply to the first objection to entity realism: in what sense should we believe in the existence of entities?

3.2 Detection properties and auxiliary properties

Chakravartty tries to answer to such a question introducing a distinction between detection and auxiliary properties. Detection properties are supposed to be responsible

8 For example Shapere 1993 and Resnick 1994. I will address the questions concerning reference in the next section (Gelfert 2003).

for the causal regularities detected by experimental operations: regularities are such in virtue of detection properties. On the other hand, auxiliary properties are the other properties that theories attribute to their entities. Selective realism is committed only to detection properties (and their causal powers) and remains agnostic as regards auxiliary properties: they may be recognized as detection properties at some time in the future, or be mere fictions, or be discarded in the development of science without major problems. The distinction is epistemic (Chakravartty 2007: 47), since it depends on the conditions under which properties are detected: we believe in detection properties because we have sufficient experimental evidence for considering them real. The epistemic nature of the distinction implies also that the boundary between detection properties and auxiliary properties is a "moving boundary". It depends both on the entities that populate the world and our ways of detecting them, i.e., on the status of scientific inquiry at the time of the detection. Therefore, I am not posing a qualitative (or "real") distinction, but a quantitative distinction based on the degree of causal power required by properties to achieve the results of measurements, detections and experimental operations.

Thus, this distinction can provide a new interpretation of the Fresnel-Maxwell case study about the nature of light analyzed by structural realists. I have said that the structural realist interpretation is not convincing especially because is not clear the relation between structure and mathematical core. On the contrary, by means of the detection-auxiliary properties distinction, we can conclude that structures strictly construed are the sets of detection properties which are preserved in the transition from Fresnel's to Maxwell's theory: for example, being influences propagated rectilinearly and made up of two components manifested at right angles to one another and to the direction of propagation (each of which has an intensity) and so on. On the contrary, the fact that light propagates in an elastic solid medium (ether) is an auxiliary property, which we can eliminate without giving up the theory.

The preservation of detection properties allows us to undermine the ontological ambiguity of the mathematical core of scientific theories. In fact, structures which seem to be empty acquire their empirical content in virtue of their relationship with the relevant applications, i.e. the experimental operations which define the identity of the qualitative detection properties whose relations constitute structures. Unlike mathematical structures (for structural realism), structures of relations between detection properties require a *minimal interpretation*. Structural realism cannot provide such a minimal interpretation since its structures are high-order structures without any knowledge of the first order properties of the things in the world. As I will say in Part 3 – section 2, this problem entails also some unpleasant consequences about the use of the concept of truth in scientific contexts.

Thus, from the historical viewpoint, the hypothesis of the existence of ether can be seen as a heuristic tool, which works in connection with the effective detection properties[9]. These hypotheses can help scientists to describe phenomena and to elaborate successful experiments, but no way they take part in the ontology of the theory. Moreover, the distinction between detection and auxiliary properties can provide a criterion to individuate in a straightforward way the radical breaks in the history of science, or, in Kuhn's words, to distinguish between normal science and revolutionary science. Actually, scientific progress consists in more or less conspicuous changes in the theoretical apparatus of well-confirmed theories. Some changes in the history of science are radical (for example the chemical revolution and the rejection of the phlogiston theory), while we can interpret the other changes as minor or major adjustment of accepted theories. Now we can state that the ordinary adjustments of scientific theories deal with auxiliary properties. This does not mean that such changes are not important, but only that they do not affect the minimal interpretation of the core of the current theory (its interpreted structure). For example $f = ma$ is part of the structure of Newton's physics and we can say that it entails ontological commitment to the entities that (under specific experimental circumstances) manifest the property of mass and the related properties. Therefore, a realist interpretation of the second law of motion has to consider mass a detection property of entities. But Newton presents his theory with many auxiliary hypotheses, for example the existence of absolute space and time. Although they are very important, these hypotheses are not part of the structure of the theory. In fact, in his criticism of the bucket argument about the existence of absolute space, Mach demonstrated that we can reject the hypothesis of the absolute space and, at the same time, maintain the structural core of Newton's physics (Jammer 1954: 139-141). Therefore, the hypothesis of absolute space is an auxiliary property. Again, it is an important concept and it could be very useful, but it is not indispensable to minimally interpret the laws of motion.

I have referred to Kuhn's distinction between normal and revolutionary science

9 Obviously, we can easily reach such a conclusion only with hindsight. Scientists are not necessarily supposed to be able to distinguish "in real time" between detection and auxiliary properties.

because I think that the distinction between detection and auxiliary properties (and their relations with the concept of structure) gibes well with the ideas about scientific lexical structures and their role in the identification of normal and revolutionary changes in the history of science that I have presented in section 1. Kuhn is concerned with the issues in the distinction between the core and the extended core of scientific theories and with their constitutive elements. In section 1, I have stated that, by constitutive elements of scientific theories, Kuhn refers to their mathematical core (for example the laws of motion and the concepts of force, mass, acceleration). But Kuhn, too, is dealing with the concrete core of theories, since he states that mathematical structures are meaningful only if connected with their paradigmatic applications, which consist in the experimental situations associated with the terms involved in the fundamental equations of the theory (for example the experiments, measurements, detections the terms mass is introduced by). For Kuhn, "mass" can be considered a detection property since the experimental detection of the property of mass (by means of the application of the term in experiments) is indispensable for the interpretation of the theory. Aside from the changes involving such detection properties, every change is normal and does not raise doubts about cumulative scientific progress. In the next sections I will use such a comparison between Chakravartty's concept of structure and Kuhn's one to propose a structuralist (and Kuhnian) correction to Chakravartty's theory of reference. But, first of all, let me present some last clarifications and summarize the results of my analysis.

3.3 Final clarifications and semirealism

The kind of selective realism defended by Chakravartty (semirealism), emphasizes the importance of first order properties in the determination of scientific structures. But he does not provide an explicit definition of "property". This is not a weak point since he says that his theory is consistent with any theory about properties (transcendent universal, immanent universals, tropes, resemblance nominalism and so on) (Chakravartty 2007: 40 fn. 4). Anyway, I will drop the metaphysical content of the concept of property and assume a "deflationary" perspective. Thus, by "property", I mean a coherent set of accepted applications of a concept. For example, by "property of mass" (according to Newton's mechanics), I refer to the set of applications of the concept of mass in experimental situations accepted by the relevant scientific community. Obviously, this implies that properties are "open-textured", since the set of applications is open and I will focus on this point in sections 3-4; anyway, it is worthy that it is an open set, since it means that more and more problems may be solved. Furthermore, the applications of the property of mass are unavoidably linked to the applications of the other properties involved in the structure of the theory, since, for example, the application of the property of mass engages the application of the concept of force.

This is consistent with Chakravartty's description of properties as capacities or dispositions "to behave in a certain way in the presence or absence of other particulars and their properties" (Chakravartty 2007: 41). For example, the fact that a body has a mass justifies the expectation that such a body will be accelerated under applied forces and will resist acceleration. The causal power of properties (i.e. the possibility to use them to manipulate some phenomena) depends on the way dispositional properties are mutually linked: it depends on the way sets of causal properties are disposed to act in connection with others. Clearly, with regard to my previous definition of property, this fixes some constraints to open-textured properties, since each property is compelled: 1) by the coherence with its previous applications; 2) by its relations with the other properties of the structure; 3) by the experimental success of its future applications.

Kuhn does not use the expression "disposition"; rather, he appeals to the "projectibility" of scientific terms (Kuhn 2000: 75, 230); but I think that "dispositions" and "projectible" mean the same thing in this context. Kuhn says that the concepts that constitute the structure (detection properties) support induction, i.e. they must satisfy the symbolic generalizations in which they appear (the mathematical core). This applies only to causal or detection properties, because they are linked to the most fundamental equations of the theory and therefore their generalizations do not admit exceptions. On the contrary, the other concepts of the theory admit exceptions, revisions and may be rejected without major problems: they do not take part in the ontology of the theory. Moreover, I shall precise that my conception of properties is also methodologically construed. In fact, I have already said that the distinction between detection and auxiliary properties is epistemic, since it depends on our methods of detection (and on the current state of scientific inquiry). We need detection properties to realistically interpret structures, which are made of relations between first order properties. But "detection property" is an epistemic concept linked to the current capacities of detection of scientific instruments. Therefore, we can say that the ontological commitment of semirealism is method-dependent; I will discuss the methodological nature of scientific laws in Part 2 – section 1.

The fact that entities manifest their properties only when subjected to specific "stimuli" does not undermine scientific realism; it implies only that entities should be considered from a structural viewpoint, since the possibility to individuate the relevant entities depends on their relational dispositions. Before returning to this idea, let me summarize the features of semirealism I agree with:

1) Structural realism and entity realism are incomplete and imply each other. Structural realism implies entity realism because interpreted structures require the existence of the entities whose properties constitute the structure. Entity realism implies structural realism because the experimental recognition of entities requires structural relations with the instruments of detection and measurement and with the properties of other entities involved in the detection.
2) The distinction between abstract structure and concrete structure allows us to provide a clear and intuitive definition of scientific structure as concrete structure and to reject the objections concerning the ambiguity of this concept.
3) The distinction between detection properties and auxiliary properties allows us to answer to the selective realist question: which entities should we faithfully consider existing? It states that detection properties are required to provide a minimal interpretation of the structural core of scientific theories, which is indispensable for its applicability.

4. Theories of reference

In the last section I have said that semirealism is different from structural realism, since it is not skeptical about the entities that scientific theories describe; entities are involved in the detection process through which structures are constituted. Therefore, unlike structural realism, semirealism (as well as entity realism) should justify its criterion for the identification of objects and the fact that some entities participate in normal scientific changes without substantial breaks. We have already seen that the distinction between detection properties and auxiliary properties can help, because it states that only detection properties are supposed to “survive” against scientific changes. But this answer is not entirely convincing as far as it does not tell us how detection properties contribute to the identification of entities. An interpreted structure should associate to relations specific physical objects and represent part of the actual (or possible) physical world (Ainsworth 2010: 50). Therefore, a realist model that takes into account the role of entities has to furnish persuasive replies about the referential function of scientific structures[10].

From this viewpoint, entity realism seems to be even worse than semirealism. In fact, it has been criticized just because of its ambiguous notion of reference, which is at the same time too narrow and too permissive (Gelfert 2003). It is too narrow since there are many entities which do not fit in with entity realism’s criterion (and therefore we should consider them non-existing), but at the same time, we have good reason to believe in their existence: for example entities that cannot be part of manipulative processes, such as theoretical entities in astronomy. At the same time, it is too permissive since (as I have said before introducing the detection-auxiliary properties distinction) it does not provide a norm to distinguish between existing and non-existing entities. For example, quasi-particles fall within existing entities (according to Hacking), but their existence is, at best, in question.

The problem with entity realism is that it accepts a causal theory of reference in which entities are “direct” instances of manipulations and operations. Since semirealism, too, endorses the causal theory of reference (although they do this in different ways), I will address the problem of reference from this standpoint.

4.1 Entity realism and the causal theory of reference

The connection between entity realism and the causal theory of reference is very easy to explain. Entity realism is skeptical about theories and tries to link realism to the direct experience of causal manipulations in experiments. Thus, theoretical changes are not a problem for entity realism, since the cumulative nature of science is guaranteed by the stability of interventions (not by the stability of representations). The causal theory of reference addresses the same problem, since it preserves the stability of reference

10 In Part 3 I will analyze how this idea is related to the theories of truth.

against theory change by means of the idea that reference is fixed by an initial act of direct "baptism" (a dubbing event) and that no description contributes to the definition of reference (Kripke 1980). Therefore, our beliefs about objects may change, but this does not undermine realism, since the causal relation between name and object is independent of our beliefs and descriptions. The causal theory allows us to commit ourselves to the existence of entities without being committed to the respective theoretical claims; but rather, merely by picking an entity out. I will discuss in depth the causal theory in the next chapter; so this will suffice for now.

Hacking himself acknowledges his "debt to Hilary Putnam" (Hacking 1984: 157) and states that Putnam saved us from the problem of meaning change in the history of science (and from the idea that the meaning of a word consists in the theoretical laws it occurs in) (Hacking 1984: 157-158)[11]. But, I think, Hacking's debt to Putnam ends there. Hacking is only struggling to avoid the problems concerning the relation between observation and theory and to make observation as independent of theoretical statements as he can. His acceptance of the causal theory of reference is not justified by a strong theoretical conviction, but only by their common "enemy": theory and meaning change (Sankey 2012: 37). And besides, Hacking is very clear; he compares his approach to Putnam's theory with the relationship between theory and experiment according to his entity realism: "I do not literally believe to Putnam, but I am happy to employ his account as an alternative to the unpalatable account in fashion some time ago" (Hacking 1984: 159). But, in the previous sections, we have seen that Hacking's claim about the existence of entities independent of any theoretical structure should be downsized. Therefore, since it is the only reason provided by Hacking to embrace the causal theory of reference, the connection between entity realism and causal theory is deeply undermined.

4.2 Semirealism and the causal theory of reference

A possible solution to such a problem is to keep holding the causal theory of reference and apply some descriptivist corrections. This is the way followed by Chakravartty's semirealism (2007)[12]. The main problem of the causal theory is that

11 Note that Hacking does not distinguish between the meaning of a term and its reference. Unlike Hacking (and Kripke-Putnam), here I am speaking only about the reference of scientific terms.

12 And also by Sankey (1994, 2012) and Psillos (1999).

ostension and direct application will not suffice to fix the reference of scientific terms. Therefore, something other has to contribute to the process of reference-fixing and the causal-descriptivist theories propose that we should acknowledge that some descriptions play a role in this process, which is causal due to the descriptions of the properties that are responsible for the causal process of baptism. Chakravartty (using his concept of "detection property") improves the causal theory to avoid some paradoxes, which apply to Hacking's theory (and to purely causal theories).

I have said that the difficulties with "direct reference" deal with non-existing entities and referential failure. For example, the causal theories may be right in saying that water is the same entity even if our beliefs about it change. But what about entities such as phlogiston? The phlogiston theory successfully explains phenomena like combustion and rusting posing a colorless, tasteless and odorless substance (phlogiston), whose removal under certain circumstances is responsible for such processes. Contemporary chemistry affirms that phlogiston does not exist and that the same phenomena can be better explained by oxygen. But the point is that phlogiston was used in causal chains and therefore, according to entity realism (as well as the causal theory or reference) we should be allowed to believe in the existence of phlogiston. Radical realists might reply that "dephlogisticated air" (air after the removal of phlogiston) is nothing but oxygen and that referential stability is saved by the recognition of co-referential terms. The causal theory distinguishes between truth and reference: a word can occur in a false statement and, at the same time, be referential. Referential terms are not supposed to be part of true statements.

But, as Chakravartty notes, this counter-objection is a trivialization of the concept of reference (Chakravartty 2007: 55), which violates our basic intuitions about it; actually, reference would be a very trivial concept if very different things (such as dephlogisticated air and oxygen) were the same entity: we cannot consider an accidental or minor question the fact that oxygen has a definite chemical structure (while phlogiston does not). When we talk about oxygen and phlogiston we are dealing with very different sets of detection properties and therefore semirealism (unlike entity realism) can admit that oxygen and dephlogisticated air do not refer to the same entity. According to Chakravartty the price to pay for avoiding such a trivialization is only the introduction of descriptivist elements in the causal theory of reference. In fact, descriptions allow us to understand how entities play a causal role in experimental practices and to figure out in what sense they are existing.

4.3 Semirealism, essentialism and descriptions

On the contrary, I think that, even if descriptions play a very important role in scientific theories, the descriptivist correction to the causal theory of reference arises other problems. I will focus on: 1) essentialism; 2) the introduction of descriptions.

Essentialism. In order to save reference against theory change, the causal theory has to specify how the identity of entities is determined. It states that, just like proper names, kind terms are rigid designators. In order for a kind terms to be a rigid designator, it must have specific essential properties fixed in the course of scientific investigation (Ellis 2001: 54-55). For example, according to Kripke, the chemical structure H_2O is the essential property of water. My question is the following: is essentialism consistent with the concept of detection property? Thanks to detection properties, we can identify existing objects, but they are not essences, because the distinction is based on epistemic arguments and may change with the time. They are linked to the manifestations of the entities (by means of experimental detection) and are not supposed to represent their essence, but, rather, their relations to specific forms of detection. But the followers of the causal theory may disagree about this distinction. Referring to the example of biological species (infra-species variations and overlaps with other species), Chakravartty admits that, strictly speaking, many natural kinds have no essence and therefore we cannot individuate the property responsible for their rigidity (assuming that physical and biological kinds work in the same way). He suggests that semirealism has to accept both essence and cluster kinds, i.e. kinds without essence, which do not essentially result from a (necessary and sufficient) property or set of properties (Chakravartty 2007: 161). In the end, he says only that properties of scientific entities are causal-dispositional, that is, they generate inductive (fallible) generalizations; but we can infer successful inductive generalizations that are not related to the essence of the objects (involved in the generalizations). For example, inductive generalizations about the dangers of fast-moving automobiles are trustworthy, but "automobile" is not a rigid designator or an essence kind (Sankey 1997a and Chakravartty 2007). Basically, I agree with Chakravartty, but the essentialist claims that the discovery of a property is a necessary and sufficient condition for membership in the kind (Soames 2002: 15). Therefore, it seems that semirealism is not consistent with the concept of rigidity.

Causal descriptivism. Causal descriptivism accepts the basic idea of the causal theory of reference, but admits that ostension and direct exposition are not enough to fix reference. Consequently, reference fixing includes also descriptive statements about properties and the relevant theoretical hypotheses. Obviously, this correction is not unproblematic, because it puts theory and meaning change back on the table. In fact, causal descriptivism should clarify how descriptions contribute to determine reference and answer to the question: "what does it happen if theoretical statements and properties are rejected?". Moreover, it has to clarify the relationship between reference and beliefs and mental states of the speakers (a problem that the causal theory of reference can easily give up)[13]. There are many possible answers to these questions and I cannot discuss them here. However, I think that the most compelling objection is the first one, since admitting the existence of non-essential predicates in science is a sufficient reason to acknowledge that we are dealing with a theory very different from the causal theory; and the introduction of descriptivist elements moves along the same line. I will go into the details of the causal theory in section 3. Anyway, now the point is that I do not want to reject the causal theory of reference to endorse a traditional descriptivist viewpoint. On the contrary, I will use the structuralist features of semirealism to present a structuralist viewpoint on reference (see also Part 1 – section 4).

5. Structures and reference

Analyzing the descriptivist corrections to the causal theory of reference, I mean that they aim to specify the "theoretical environment" (Psillos 1999: 275) in which the word is embedded. Pointing to something is not sufficient to single out a natural kind, because we need to recognize non-superficial properties that require theoretical assumptions. The theoretical environment of a word is, to some extent, "constitutive" of the possibility of reference: it is a precondition to identify kinds and objects. Such an environment consists, for example, of the theoretical generalizations in which the term occurs, the other concepts of the generalizations, the relations between the properties we are dealing with and the instruments of detection. Briefly, I think that the theoretical

13 For a discussion of causal descriptivism see (Soames 2002) and (Devitt, Sterenly 1999).

environment that makes reference possible consists in the "concrete structure" the word is part of. This is why I use the word "constitutive": structural realism is strongly Kantian (Massimi 2010) and, as in section 1, I recognize two Kantian-structuralist theses:

1) Scientific structures delimit objective reality (not the other way round). This thesis means that an entity is a potential object of scientific knowledge as far as we can individuate it through its detection properties.
2) Reference is an "internalist" concept: asking for the identity of something is meaningful only within a "theoretical environment". This thesis states that the referential function of scientific languages is related to their structural organization.

5.1 "Internalist" reference

I think that the main problem with the causal theory of reference is the idea of a "direct" linkage between words and entities. This problem makes it actually impossible for the realist to distinguish between existing entities and entities that take part in experiments, but may be non-existing. Semirealism overcomes the problem by means of the distinction between detection and auxiliary properties. The relations between detection properties constitute scientific structures, the things that we have to defend over the course of scientific progress in order to preserve our realist commitment. Moreover, I have said that detection properties are dispositional, i.e. they give rise to inductive expectations about how entities will behave in specific situations. Sets of causal (dispositional) properties act in connection: for example, the fact that a body has a given mass provides us with information about the presence or the absence of impressed forces or about the value of acceleration (in other words, it provides us with information about other dispositions). The detection of a property is a "relational" act: it necessarily involves the presence (or the absence) of other properties (of the same entity and properties of other entities), as well as the contact with detection instruments. Causation and dispositions deal with relations, that is to say, they deal with structures that are indispensable for the recognition of the entities to which properties pertain. What I mean is that the idea of "direct" reference is misleading: the relationship between words and things in the world (and their properties) is not an isolated phenomena. It involves the “theoretical environment” that makes the referential act possible, and consequently, it is a structural event.

Therefore, by external reference, I mean the direct connection that gives its own referent to a word. On the contrary, by internal reference, I do not mean an idealist or “ontologically relativist” position. I mean that the possibility to single out the referents of scientific theories depends on the structures of such theories. As I have anticipated in section 1, the objects (or entities, the *referents* of scientific theories) exist and possess their properties that we detect through scientific experimentation. But *reference* (i.e. the connection between theories and things in the world) depends on the possibility to single certain properties out and therefore on the structural apparatus of the theory. Referents are outside language, but reference is necessarily language-dependent.

My point is that one cannot be able to recognize an object without, at the same time, being able to distinguish it from other objects of the same “theoretical environment”; or one cannot correctly apply a concept without knowing situations in which the same concept does not apply. The identification of an object requires many properties (both detection and auxiliary); this is consistent with semirealism, since Chakravartty says that we can consider objects to be “cohering sets of detection properties” (Chakravarrty 2007: 65). But I add that we require knowledge of properties that the object does not possess as well. The identity of objects is important as well as their differences with the other objects of the structure. This is an advantage of structure toward direct reference: direct reference poses a “direct” relation between name and thing, while internal reference states that the relation between name and thing is not a one-to-one relation; it is partially determined by the relations between properties that constitute the structure. As we have seen in section 1 for Newton’s mechanics, no term of the core of the theory (i.e. no detection property) is available independently of the other terms of the core and the theoretical statements by which they are introduced.

Kuhn concludes that the core of scientific theories constitute a “locally holistic” structure (Kuhn 2000: 2). Here “structure” has the same meaning of “concrete structure” (or interpreted structure). “Locally” means that scientific theories are not globally holistic: we can distinguish between detection and auxiliary properties and only detection properties compose the structure. And “holistic” means that the detection properties are a network of interrelated properties, which are not available for detection and definition independently of each other and of the mathematical equations in which they appear. Local holism includes the structuralist claim that I have just presented. A structure (that Kuhn calls "lexical structure") includes, at the same time, similarities and differences between objects, properties, contexts of application. Kuhn provides also an example from natural languages:

> Let me take "*doux*" to be a node in a multidimensional lexical network where its position is specified by its distance from such other nodes as "*mou*", "*sucré*", etc.; […] the meaning of *"doux"'* consists simply of its structural relation to other terms of the network. Since "*doux*" is itself reciprocally implicated in the meanings of these other terms, none of them, taken by itself, has an independently specifiable meaning. (Kuhn 2000: 55)

Two structures are exactly co-referential if and only if the relations between the detection properties of the structure are the same. Note that we are dealing with concrete structures, since the structural position of each node (the relative distance from the other nodes) is determined by the applications of the same node in experimental practices.

In my internalist view on reference, object are isolated in experience by means of a set of properties (some of them are essential, but only from an epistemic viewpoint), which determines identities and differences with the other objects of the same "theoretical environment". Henceforth, this is not a descriptivist theory, but a structuralist one. The advantage of such a view is that it rules out explicit theoretical statements (which are vulnerable to theory change) by means of the relations between detection properties epistemically construed. If you consider a property the "set of legitimate applications", then the experimental spirit of the causal theory of reference and entity realism is safe. From this viewpoint, properties are functional properties: what is important is their functional role in the structure, i.e., the set of successful experiments which are possible thanks to such properties. The network of identities and differences is generated by means of the exposition to such cases of application: descriptive statements are part of the constitution of the structure (even if the conditions may change case by case), but they are not a sufficient condition.

Finally let me summarize my results:

1) Hacking's entity realism is linked to the causal theory of reference, but he does not provide any good reason to accept the causal theory, except the necessity to

make entities independent of theories. But I have argued that this claim is untenable.

2) Semirealism, too, is linked to the causal theory of reference, but the epistemic distinction between detection properties and auxiliary properties is inconsistent with the essentialist nature of the causal theory.

3) Finally, I suggest to adopt structuralism to avoid the problems of the causal theory of reference. From this viewpoint, reference is language-dependent: it depends on the structure of the current theory and on the network of relations that allows us to identify the referents of scientific theories. Reference is not something that requires only a relation between a name and something: it requires sets of relations with the other "inhabitants" of the "theoretical environment" we deal with.

6. Conclusions

My conclusion, that I will further specify in Part 1 – sections 3-4, is that reference is not merely a connection between world and language. We cannot think that dephlogisticated air exists because we use the word "phlogiston" in successful experiments: the success of scientific theories is not a one-to-one referential relation between words and objects. The referential function of theories involves the whole structure and therefore is plausible that a theory is referentially successful although some entities that the theory postulates do not exist. Phlogiston does not exists; but what is important is that the structural relations between phlogiston and other detection properties (recognized by experiments) that characterize the phlogiston theory are not preserved in contemporary chemistry. And this remark allows us to say that the phlogiston theory and contemporary chemistry are not co-referential. Therefore, by the expression "internal reference", I do not want to suggest that scientific theories idealistically determine the existence and the individuality of their referents. Existence and individuality are presupposed by the operations of detection and measurement, which enable us to speak about detection properties and structures. Finally, by "singling out a scientific entity", I mean isolating it in its spatio-temporal coordinates and distinguish it from similar and dissimilar things in its context or environment.

Section 3: A Kuhnian Perspective on the Meaning of Scientific Terms

1. Introduction

After introducing the concepts of reference and meaning change, now it is time to specify what I mean by these notions: in order to do so, I will continue discussing Kuhn's ideas. Actually, everyone knows the thesis of semantic incommensurability: the meaning of scientific terms changes over the course of scientific revolutions. This thesis has been widely discussed, but sometimes the focus of the discussion is not clear because different philosophers defend rival theories of meaning. Additionally, Kuhn never explicitly clarified which is the kind of meaning theory he endorsed and this undermines the very foundation of his argument. In fact, if what we mean by "the meaning of scientific terms" is not pointed out, then nobody will understand what kind of things change over the course of scientific revolutions. In this chapter I will answer to this question and try to outline the coordinates of the theory of meaning that is consistent with Kuhn's claims. To achieve this aim, I will criticize two theories of meaning which have often been associated with the incommensurability thesis: the intensionalist theory and the causal theory of reference. My aim is not to reject these theories by general arguments: I will acknowledge that, in some cases, Kuhn's arguments against them (especially against the causal theory) are not, by any means, conclusive. Nevertheless, his objections are useful to figure out his approach to the problem of the meaning of scientific terms.

2. The Intensionalist view on referential change

The first referential answer to Kuhn's semantic incommensurability (Hoyningen-Huene, Sankey 2001b: x) was proposed by Scheffler (1967), which includes a detailed attack to *The Structure of Scientific Revolutions*. As regards the question of meaning change, he tries to reject it by employing Frege's distinction between sense (*Sinn,* intension) and reference (*Bedeutong*, extension). In fact Kuhn says that

> The physical referents of these Einsteinian concepts [space, mass, time] are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian 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: 101-102)

From the Fregean viewpoint, the reference of scientific terms (their *Bedeutong*) is determined by their sense *(Sinn)*, that is to say the descriptive content which is satisfied by the reference and that the speakers who understand the term associate with the respective reference.

From these premises, Scheffler argues that two or more terms may have different senses, but the same reference. For example, Frege said that the expressions “Venus”, “the morning star”, “the evening star”, “Hesperus”, “Phosphorus” refer to the same object (the same planet) although they have different senses. Therefore, even if the senses of terms such as “mass” or “time” changed over the course of Einstein’s revolution, this does not mean that their reference changed as well. The core of the argument is that co-referential terms from different theories may differ in sense; and this is supposed to provide a solution to the problem of incommensurability.

However, Scheffler’s analysis employs a concept of “reference” which is inconsistent with Kuhn’s perspective (Sankey 1994: 36-43). In the above passage, Kuhn’s “referent” is not identical with Frege’s *Bedeutong*, since Kuhn uses a concept of reference that is partially language-dependent: as I have said in the previous sections, a sort of “internal reference”. I will return on this point in the following sections, but at first I will challenge the intensionalist interpretation in the exposition of Alexander Bird (Bird 2000: 163-168). In fact, Bird acknowledges that Frege’s distinction between sense and reference is a strictly realist idea that is not consistent with Kuhn’s theses (Bird 2004: 46). But he presents an intensionalist interpretation of Kuhn’s theory of meaning as well: he says that the changes in the extension of scientific terms depend on a change in their intensional meaning; and that in the same way, the change in the intensional meaning determines referential discontinuity (Bird 2000: 168)[14]. Bird’s interpretation is based on the following premises:

1. Scientific terms have an intension, which depends on the theoretical claims of their theory;
2. Such dependence is *thick*, because the intension perhaps depends on all the

14 He claims also that, for Kuhn, although there is a shift in extension, neither the earlier extension nor the later extension is empty (Bird 2000: 168); and this is also the case regarding reference, because it is successful both before and after the change.

theoretical claims of the theory. Every theoretical claim plays some part in meaning fixing;

3. Such dependence is also *strict*, because a property can satisfy the reference of a scientific term if and only if such property is truly described by all the relevant laws and other descriptions contained in the intension of the term.

My view is that these premises are not compatible with some of Kuhn's claims and that therefore the intensionalist interpretation of Kuhn's theory of meaning should be rejected.

The first premise. The first premise states that scientific terms have a descriptive content, which is determined by the theoretical claims of the theory they are involved in. However, Kuhn denies that scientific terms have intensions or descriptive contents, definitions and explicit rules of application. When we ask for the meaning of a term, it is useless to look for a set of necessary and sufficient properties to identify such term. Just like Wittgenstein's games, we do not need "a definition, a list of characteristics shared by games and only games, or of the features common to both men and wolves and to them alone" (Kuhn 2000: 201). This is because no list of that sort exists (for example not all games have a winner), but it is not an effective trouble for scientific language (or for ordinary language). Kuhn suggests to consider the example of a child, Johnny, who learns to identify different species of bird: ducks, geese and swans. This process does not require anything like definitions: one might use universal statements such as "all swans are white", but they are not necessary and sufficient. Under the supervision of his father (who validates the correct identifications and fixes the wrong ones), Johnny will succeed in identifying birds only by means of the direct perception of similarities and differences; kind terms are very similar to Wittgenstein's family resemblances, since scientific terms have no necessary and sufficient conditions of identification and application and "in matching terms to their referents one may legitimately make use of anything one knows or believes about those referents" (Kuhn 2000: 50). The intensionalist view assumes that the reference of a term should satisfy a certain description, which is retained by any speaker who understands the application. On the contrary, Kuhn denies that the understanding and the application of a term depends on descriptions and speakers' beliefs.

This is the first point of my analysis: Kuhn's perspective on the meaning of scientific terms is a Wittgensteinian perspective: the meaning of a term consists in its use in scientific practice: “first, knowing what a word means is knowing how to use it for communication with other members of the language community within which it is current” (Kuhn 2000: 62). Kuhn explicitly relates this statement just to the idea I have sketched: that a theory of meaning does not require descriptions or conditions for the application. People may (correctly) use the same terms, but employ different set of coordinates in doing this (just like the United States can be mapped in different equivalent coordinate systems). Kuhn’s theory of meaning depends on “talking about the way words are actually used, the situations in which they apply” (Kuhn 2000: 77 fn. 25). In this respect, scientific learning plays a constitutive role: the meaning of scientific terms is their use in scientific practice because they are learned in use; that is, someone who can properly use such terms provides the student with accepted examples of application.

The second premise. The second premise states that (all) the theoretical claims of the relevant theory fix the meaning of its terms. This is true in so far as scientific terms are defined by the universal laws in which they occur; and Kuhn does say that the most important function of symbolic generalizations is to define their terms: “they function in part as laws but also in part as definitions of some of the symbols they deploy” (Kuhn 1970: 183). But this is not the whole story, since we have seen that scientific terms do not allow us to define them explicitly, especially because their meanings are not embodiable in definitions. Therefore we shall reconsider Kuhn’s claim. He says that the meaning of scientific terms depends on the theoretical claims of the theory; for example, on the universal laws that constitute its hard-core. But he says also that such universal laws are meaningless in so far as they are not associated with their exemplary and paradigmatic applications. Referring to the difficulties that physics students use to face to solve the end-of-chapter exercises of their textbooks, Kuhn says that the meaning of scientific laws is fixed by their application to the concrete practice of puzzle-solving. For example, so long as they are not learned by means of exemplary cases of classical physics such as the inclined plane or the simple pendulum, Newton’s laws of motion are meaningless. From this viewpoint, exercises and applications are not useful to check whether the student has understood the lesson; rather, they allow the student to understand it. Thus, the relationship between laws and applications is the opposite of the commonsensical interpretation, but I will focus on this point in Part 2 – section 2 (see also Andersen 2000b, Barnes 1982, Warwick, Kaiser 2005). Now, what is important for my purpose is that the meaning of scientific terms depends on the laws in which they occur, but the meaning of the laws depends on their applications to the empirical context. Ultimately, this is basically the same problem of the first premise: the direct attachment of scientific terms to their context of use.

Therefore, let me come back to the example of the child who learns to use the words 'duck', 'goose', 'swan'. Johnny is exposed to many paradigmatic applications of those terms and learns to group objects into similarity-dissimilarity classes (without rules or explicit definitions). In this context, as I have sketched in the previous section, dissimilarities are as important as similarities. The meaning of a term is not positively fixed by means of a simple act of ostension; rather it is determined by contrast through the exposition to different terms which belong to the same semantic network (like swan, duck and goose). Words have no meaning individually: they are meaningful only through connections with other words of their semantic field. Such a group of terms generates networks of inter-defined terms that Kuhn calls "lexical structures". For example, as I have said in section 1, the terms 'mass', 'force' and 'acceleration' are acquired together through the second law of motion and then their meanings cannot be separated. I think this explains the role played by universal laws. Universal laws introduce set of inter-defined terms, which are acquired in use: "'force' must be learned with terms like 'mass' and 'weight'. And they are learned from situations in which they occur together, situations exemplifying laws of nature" (Kuhn 2000: 231). The meaning of such terms is still their use in scientific practice, but Kuhn highlights the fact that the use, for example, of the term 'mass' (in classical mechanics) is not understandable as far as we do not grasp the whole semantic field by which the term 'mass' is introduced.

The third premise. The third premise states that "for some property to be the reference of 'mass' that property must be truly described by all the relevant laws and other descriptions contained in the intension of mass" (Bird 2000: 167). On this specific point, I will be quite brief because I partially agree. But it is useful to clarify a question that may arise from my discussion of the other premises. In fact terms such as 'swan', 'goose' and 'duck' seem to be different from terms such as 'mass', 'force' and 'acceleration'. As regards the former group, it is very easy to point out the network of similarities and differences the learning of such terms is grounded in. But, as regards the latter group, the question is not so clear. That is why Kuhn introduces a distinction between normic and nomic concepts. Normic concepts are exemplified by words such as ‘duck’, ‘swan’, ‘goose’, or, in scientific context, ‘liquid’, ‘gas’, ‘solid’; they are acquired by means of the process of direct inspection and application summarized in the example of Johnny’s training. Instead, nomic concepts are introduced by universal laws, such as ‘force’, ‘mass’ and ‘acceleration’. The main difference between these kinds of concepts is that normic concepts admit exceptions to the generalizations satisfied by their referents (Kuhn 2000: 230). For example “liquids expand when heated” fails for water between 0 and 4 degrees centigrade. On the contrary nomic concepts involve exceptionless laws of nature. Therefore the third premise is correct for nomic terms, but does not apply to normic terms. Anyway, this is the least important point because the distinction between normic and nomic concept is not a qualitative distinction. As we have seen in the discussion of the first two premises, although nomic concepts are not introduced by direct inspection, they are learned by problem situations to which a certain law applies (for example Newton’s laws and the free fall). In this way, problem situations generate classes of similarity and dissimilarity in the same way as normic concepts do. Finally, we cannot consider the difference between normic and nomic a distinction between similarity-dissimilarity classes concepts and concepts that can be explicitly defined; on the contrary, the significant point is the level at which the similarities come in (Andersen, Barker, Chen 2006: 32).

Following the discussion of the premises of the intensionalist interpretation, this is the time to summarize the results of this section.

1. The meaning of scientific terms consists in their use in the relevant field of scientific practice. They have no intension or, if they have any, it is not sufficient for the application of such terms to experimental contexts.
2. The meaning of a term is not isolated; rather, it is determined by contrast and in relation to other terms of the same semantic field, which apply to similar situations. Differences are as important as similarities to identify the meaning of a word.
3. These remarks apply both to terms introduced by direct application and to terms introduced by seemingly descriptive statements. Universal laws cannot be considered definitions because they are meaningless without their paradigmatic

applications.

These results lead to the last point of this section. Since meaning is use and use depends on the learning process experienced by scientists during their student years, obviously the correct application of scientific terms is a social thing: a term is properly used in so far as it is unequivocally used within a scientific community. Scientists do not necessarily share definitions, descriptions, criteria, necessary and sufficient conditions, rules of application and so on; rather, they should share the network of relationships between concepts, the structure of the lexicon. The determination of the meaning of scientific terms is based on scientific learning and learning is a social process, since it requires the existence of a community, which introduces the student to its practice. Kuhn distinguishes between the lexicon as a shared property, linked to a specific community and the lexicon as something possessed by each scientist (or individual) member of the relevant scientific community. Therefore, speaking about the meaning of scientific terms is appropriate only if we consider scientific communities (and not individual scientists) as the main actors of scientific progress.

3. Are scientific terms rigid designators?

The claim that the concept of meaning is accountable only from a social viewpoint and Kuhn's interest in the direct attachment of words to nature seem to provide a connection between this theory of meaning and Kripke and Putnam's causal theory of reference. For example Kuhn says that

> The distinction between a theoretical and a basic vocabulary will not do in its present form because many theoretical terms can be shown to attach to nature in the same way, whatever it may be, as basic terms. But I am in addition concerned to inquire how "direct attachment" may work, whether of a theoretical or basic vocabulary. In the process I attack the often implicit assumption that anyone who knows how to use a basic terms correctly has access, conscious or unconscious, to a set of criteria which define that term or provide necessary and sufficient conditions governing is application. (Kuhn 1977: 302 fn. 11)

Bird recalls this passage and rightly states that Kuhn is rejecting Frege's descriptivist approach to the problem of reference. But he adds that here Kuhn endorses Kripke's theory of reference; thus, he tries to account for Kuhn's later objections to the causal theory affirming that, in the meanwhile, Kuhn changed his mind. In fact the above passage is from a 1974 paper and Kuhn's main papers against the causal theory of reference have been published in 1989/1990. On the contrary, I do not think that Kuhn's view changed on this matter. Although Kuhn agrees with some theses of the causal theory (just like in the quoted passage), it is not consistent with many basic claims of his theory of meaning and, moreover, in 1979 (when he published the paper "Metaphor in Science") he was already skeptical toward Kripke's theory. Perhaps Kuhn's arguments have become stronger in recent years, but I think that he has never been a follower of the causal theory of reference.

Basically, both Kuhn and Kripke-Putnam deny that the meaning of a term *t* is given by a description or a conjunction of descriptions or a cluster of descriptions that the speaker associates with *t*; and that the substitution of *t* for its description preserves meaning and proposition (and vice versa). On the contrary, according to Kripke (1980) (whose theory originally applies to proper names), singular terms are rigid designators: with regard to an object O, the term *t* is a rigid designator if and only if it designates O in all possible worlds in which O exists; and, with respect to any possible world, *t* designates only O. For example the name "Stephen Curry" and the description "the 2015 NBA MVP" designate the same object, but the first is a rigid designator, while the second is not, because we can imagine a possible world in which LeBron James is the 2015 NBA MVP. Referring to the meaning of scientific terms, the basic point is that the rigid designator keeps unchanged its reference although our beliefs about it are false. For example, one might wrongly believe that LeBron James won the 2015 NBA MVP, but my belief (and its revision) does not change the reference of the name "Stephen Curry". Reference is fixed by an act of naming called "baptism" or "dubbing" and then the correctness of the later uses is guaranteed by a causal chain, which links them to the original use.

Putnam applies Kripke's theory to natural kind terms by means of his famous thought experiment of the Twin Earth (Putnam 1975: 223 ff.). His primary aim is to demonstrate that a set of beliefs (Frege's descriptive content) is not sufficient to fix the reference of a term: because, given two speakers (S_1 and S_2) and a term T, T can have different referents when it is used by S_1 and S_2, even though they share all their beliefs about T. Putnam asks us to imagine two worlds (E and TE) which are exactly alike, except that E has our familiar water (H_2O), while TE has a substance superficially identical with water, but with a different chemical structure (XYZ)[15]. S_1 and S_2 are in the same psychological state (because they are not experienced in contemporary chemistry and individuate water only by its superficial properties) when they use the term water, but S_1 (who is an inhabitant of E) refers to H_2O, while S_2 (who is an inhabitant of TE) refers to XYZ. This experiment points out that it is impossible to fix reference in a Fregean way, by means of a set of beliefs "in the head" of the speaker. Putnam accepts Kripke's theory and claims that reference is established by a causal connection: $water_e$ refers to H_2O because it is causally connected with H_2O, but not with XYZ (by means of the act of baptism and the causal chain); while $water_{te}$ refers to XYZ because it is causally connected with XYZ. The causal theory of reference is an objection to Kuhn because it states that our beliefs about objects may well change over the course of scientific revolutions, but the causal connection that involves scientists, their use of scientific terms and the world does not change. Roughly, beliefs change, but reference was fixed independently of those beliefs and then does not change.

Kuhn criticizes the causal theory of reference. Although Sharrock and Read try to defend his viewpoint (Sharrock, Read 2002), Bird demonstrates (Bird 2004: 56-61) that Kuhn's objections cannot be considered a compelling rejection of the causal theory and I agree with Bird. Therefore, I will analyze Kuhn's argument against Kripke-Putnam, but my aim is not to defend Kuhn and argue against the causal theory of reference (as I have done in the previous chapter). Rather, I think that, although Kuhn's criticisms do not sound quite convincing, they can help us to understand his approach to the problem of meaning.

3.1 The problem of reference

Therefore, let me present Kuhn's first argument:

> The terms 'XYZ' and 'H_2O' are drawn from modern chemical theory, and that theory is incompatible with the existence of a substance with properties very nearly the same as water but described by an elaborate chemical formula. Such a substance would, among other things, be too heavy to evaporate at normal terrestrial temperatures. Its discovery would present the same problems as the simultaneous violation of Newton's second law and the law of gravity described in the last section. It would, that is, demonstrate the presence of fundamental errors in the chemical theory which gives meanings to compound names like H_2O and the

15 Actually I am taking for granted the distinction between superficial and essential properties. I will return later on this question.

> unabbreviated form of 'XYZ'. Within the lexicon of modern chemistry, a world containing both our earth and Putnam's Twin Earth is lexically possible, but the composite statement that describes it is necessarily false. Only with a differently structured lexicon, one shaped to describe a very different sort of world, could one, without contradiction, describe the behavior of XYZ at all, and in that lexicon 'H_2O' might no longer refer to what we call 'water'. (Kuhn 2000: 80-81).

According to Kuhn, the only conclusion that we can draw from Putnam's Twin Earth experiment should be something like "back to the drawing board! Something is badly wrong in our chemical theory" (Kuhn 2000: 80). The existence of a substance which, at the same time, shares with $water_e$ all its superficial properties (tasteless, colorless substance which boils at 100 grades centigrade and so on) and has a chemical structure different from H_2O is impossible. Consequently, the existence of XYZ is impossible or inconsistent with contemporary chemistry. Merely, this argument is not good because it does not attack the theory, but the fictional aspects of an invented story; and Putnam never claimed that his experiment was realistic (nevertheless, Bird describes a scenario in which Putnam's thought experiment is scientifically plausible in Bird 2004).

Rather, I think that the most intriguing point of Kuhn's argument is that he was not interested the problem of reference and the stability of reference; rather, he was interested in the consequences of the introduction of new concepts ($water_{te}$ or XYZ) in the lexical structure of modern chemistry[16]. This lack of interest results from Kuhn's rejection of the concept of reference as a neutral connection between words and objects; basically, he rejects the very possibility of an access to the world that is (totally) independent of the lexical structure we are using. This is not antirealist (rather, as I have said in section 1, it is Kantian); but it entails that reference is partially language-dependent, or internal to a lexicon. Consequently, it is not useful to ask whether Newtonian mass and Einsteinian mass (or H_2O and XYZ) refer to the same "object". The point is that there is no way of sensibly posing the question of "extralinguistic reference" (Hoyningen-Huene, Oberheim, Andersen 1996: 135). From Kuhn's viewpoint, meaning and reference are very different things. On this point he follows Wittgenstein's famous starting-point of the *Philosophical Investigations*, where he criticizes the "Augustinian picture of language" (Wittgenstein 1958: 2 ff), which suggests that individual words have meaning, that words stand for objects, that the

16 Sharrock and Read also notice Kuhn's lack of interest in the referential question: "Putnam suggests that 'water' referred to the same substance (H_2O) in 1750 as today, and that's an end of the matter. Kuhn, by contrast, is interested in the structure of (any given) chemistry". (Sharrock, Read 2002: 185-186).

meaning of a word is the object it stands for and that the connection between word and object is established by ostensive definition. Wittgenstein rejects this model for many reasons (Glock 1996a: 41-45), but the distinction between meaning and reference is illustrated by an example: "when Mr. N.N. dies one says that the bearer of the name dies, not that the meaning dies" (Wittgenstein 1958: 20); the death of Mr. N.N. does not make the sentence "Mr. N.N died" meaningless[17]. According to Kuhn, meaning is not reference, but scientific revolutions entail meaning changes which, in turn, entail referential changes. The physical referents of Einsteinian concepts are not identical with those of Newtonian concepts because (for Kuhn) the relativity revolution makes the lexical structures of the theories incommensurable. Referential change exists, but it is a collateral problem; this does not mean that it is not important, but that it is a by-product of meaning change. Therefore, Putnam directly faces the problem of referential change, while Kuhn thinks that it depends on meaning change and therefore, according to Kuhn, the Twin Earth experiment does not offer any potential solution to his problems. These remarks should justify Kuhn's lack of interest in Putnam's purpose.

From this perspective, Kuhn's observations about the introduction of XYZ in the lexical structure of modern chemistry are noteworthy. He says that such an introduction gives rise to internal contradictions within scientific knowledge and therefore it requires a revolution in our beliefs about the chemical world. From his perspective, the causal theory of reference cannot account for a basic feature of meaning that I have stressed in the second conclusion of the last section of this chapter:

- The meaning of a term is not isolated; rather, it is determined by contrast and in relation to other terms of the same semantic field, which apply to similar situations. Differences are as important as similarities to identify the meaning of a word.

In his argument, Kuhn fails to provide a network of similarities and differences which includes both H_2O and XYZ in the language of modern chemistry. As I have sketched in section 1 (and 2), the existence of such a structure is a precondition for the possibility of successful reference (as well as referential failure), since it provides a framework for meaningful scientific sentences. Without such a framework, the problem

17 Probably, another source of Kuhn's distinction between meaning and reference is Quine's distinction between meaning and naming (Quine 1953: 9, 21, 47)

of referential change and stability is misleading, because reference is a function of a lexical structure. For example, take the sentence

- The gold mountain is in California

This sentence has no referent if we understand it as a sentence about geography; but it may refer if we find it in a novel or any fictional context and, to that extent, it can be verified or falsified. The lexical structure to which the sentence belongs pre-exists to referential success or failure. For example, phlogiston theory and modern chemistry categorize the world in different ways and therefore we cannot charge the phlogiston theory with referential failure just because dephlogisticated air does not exist. As I have said in Section 2, the problem with the phlogiston theory is not that phlogiston is non-existing, but that it is associated with false theoretical descriptions and different problem situations. Kuhn makes a similar point saying that, if translation meant "reference determination" (and therefore a translator of the phlogiston theory should leave space blanks when he encounters the non-referential word "phlogiston"), then no work of fiction could be translated (Kuhn 2000: 41). So this explains why, as Bird notices (Bird 2000: 168), Kuhn implicitly says that, although the meaning of a term changes, reference is successful both before and after the change. This would not be possible if reference were independent from the lexical structure of the theory; roughly, according to the causal theory of reference the term "phlogiston" does not refer, while, according to Kuhn, it refers within its theory. In the end, reference is a function of the shared lexical structure and depends on meaning. Thus, the meaning of a term is not its reference, but "its structural relation to other terms of the network" (Kuhn 2000: 55).

3.2 Family resemblances and polysemy

Now let me turn to the second argument provided by Kuhn against the causal theory of reference:

> 'H_2O' picks out samples not only of water but also of ice and steam. H_2O can exist in all three states of aggregation-solid, liquid, and gaseous-and it is therefore not the same as water, at least not as picked out by the term 'water' in 1750. The difference in items referred to is, furthermore, by no means marginal, like that due to impurities for example. Whole categories of substance are involved, and their involvement is by no means accidental. In 1750 the primary differences between

> chemical species were the states of aggregation or modeled upon them. Water, in particular, was an elementary body of which liquidity was an essential property. For some chemists the term 'water' referred to the generic liquid, and it had done so for many more only a few generations before. Not until the 1780s, in an episode long known as "The Chemical Revolution", was the taxonomy of chemistry transformed so that a chemical species might exist in all three states of aggregation. Thereafter, the distinction between solids, liquids, and gases became physical, not chemical. The discovery that liquid water was a compound of two gaseous substances, hydrogen and oxygen, was an integral part of that larger transformation and could not have been made without it. (Kuhn 2000: 81-82)

The second argument (as well as the first one) attacks the fictional aspects of Putnam's experiment and not its philosophical content. Moreover, Bird states also that it is not historically accurate, since the idea that solid, liquid and gaseous water are the same substance is older than the chemical revolution, although some scientists did not agree. However, just like for the first argument, I do not want to defend Kuhn's objection; rather I will focus on its consequences on Kuhn's theory of meaning.

In fact, leaving aside the historical question about the chemical revolution, Kuhn's point is that, after the revolution, the term 'water' is applied by scientists in a different way than it was before the revolution. Before 1780, the term 'water' applied only to liquid water, because the distinctions between chemical substances were based on their states of aggregation; instead, after the chemical revolution scientists discovered that the same substance can exist in different states. The extension of the application-range of the term 'water' is important because it is a good example of the development of science. Assuming that science consists in the application of attested concepts to solve new problems, then, according to Kuhn, scientific concepts should be as elastic as we can apply them in more and more problem situations. Kuhn's exemplary problem solutions work just like this. For example Galileo Galilei used his model of the inclined plane to solve the problem of the free fall. The connection between inclined plane and free fall is not deductive or "algorithmic": it requires the recognition of the analogies between those problem situations and the ability to individuate new patterns of similarities and differences (see Hesse 1966). Roughly, extending the meaning of the term 'water' to solid and gaseous water requires the same process. It is not easy to realize that things that manifest different states of aggregation are really the same substance: one has to discover new similarities among the objects of the chemical world. Specifically, he should replace the network of similarities and differences that he uses to identify and apply chemical terms with a new model that requires new similarities and differences. Before the chemical revolution, such a model was based on the state of aggregation of the substances; after the chemical revolution, new analogies have been recognized. Therefore, Kuhn's question is the following: can the rigid designator account for the analogical application of a term to new contexts? Kuhn's answer is that it cannot.

The rigid designator was originally applied to proper names. Kuhn does not object to that theory: the problem derives from natural kind terms as rigid designators. The first difference is linked to the problem I have faced in the section about meaning and reference (3.1). Kuhn says that pointing to an individual can tell you how to properly use the relevant name; but, as far as ostension is supposed to fix the reference of natural kinds, a single act of ostension is no longer sufficient. This does not mean that pointing to LeBron James is easy, while pointing to swans is difficult. Rather, as I stated before, the meaning of natural kinds involves their structural relations to other terms of the network: ostension can help us to understand the meaning of a term, but it cannot, by any means, fix the meaning[18]. Even more so, this remark applies to scientific terms such as "mass" or "electric charge", which are introduced by physical laws as well (nomic concepts) and should be used in different contexts. Fox example, if someone exhibits to you the needle of a galvanometer and says that its deflection is caused by "electric charge", how can you guess how to apply the term "electric charge" correctly in a thunderstorm? Therefore, isolated words have no meaning; I have already discussed this point, but the application of old terms to new problem situations is strictly related to this question.

The problem is that, while, obviously, proper names are names, sometimes natural kinds behave as names, but usually they are predicates. Like predicates, and unlike proper names, they can be quantified over: for example, *all men* are mortal; moreover, they occur in predicative position: for example, this substance is *gold*. Consequently, the application of the rigid designator to these terms should be justified (see for example Soames 2002 and Devitt 2005). Here I cannot analyze extensively this question; rather, I would like to draw the attention to a problem that is connected to my question about the "analogical" use of kind terms. According to Soames (Soames 2002), the best strategy to clarify what is for a natural kind predicate to be rigid is the following:

18 See Kuhn's distinction between 'ostension' and 'ostensive' (Kuhn 2000: 67 fn. 13); I will discuss it in Part 2 – section 2.

- Define a concept of rigidity for predicates that is a natural extension of the concept that has been defined for singular terms. Then determine whether natural kind predicates are rigid in this sense, and whether so-called theoretical identities involving them are necessary if true. (Soames 2002: 249)

But this strategy has at least two weak points. The first is precisely about the application of old terms to new cases. In fact, a term is a rigid designator when it designates the same object in all possible worlds in which such object exists. For example, at the actual world the extension of the predicate 'mammal' is the set of all objects that are mammals at that world. But this does not turn the term 'mammal' to be a rigid designator, because such claim entails that the term has the same extension in all possible worlds. Basically, we can imagine a possible world in which there could have been different mammals; or we can think to the fact that some mammals that exist could have failed to exists and the other way round. Moreover, we can analyze the taxonomical case of the 'platypus' (Kuhn 2000: 92), which forced scientists to revise their concept of 'mammal', since platypus is a mammal which lays its eggs. The application of the world 'mammal' to the platypus entails an analogical process, which modifies the relations of similarity through which scientists used to classify the animals; as a consequence, the properties they used to attribute to mammals changed.

Thus, we can turn to the second problem: the theory of the rigid designator is committed to an essentialist approach that states that there is a genuine distinction between essential and non-essential properties (that I have criticized in the previous chapter referring to the epistemic nature of detection and auxiliary properties). For example, water is supposed be a rigid designator because the chemical expression H_2O refers only to water (there is only one compound of hydrogen and oxygen in the ratio 2:1). The statement "water is H_2O" is necessarily true and this constitutes evidence for the idea that water has an essence: H_2O. On the contrary, properties like "being tasteless" or "being colorless" are accidental properties. But the case of the platypus seems to challenge this distinction. In fact, assuming that "mammal" is a rigid designator, the proposition "mammals do not lie eggs" should be necessarily true. This is not an insurmountable problem, since the most plausible theories about the metaphysics of biological kind tries to defend essentialism by replacing the concept of metaphysical essence with the concept of "historical essence", defined by the *clade* or ancestor group the organism belongs to. Anyway, the main point that emerges from this objection, and involves the distinction between essential and superficial properties, is about the boundaries of biological *taxa*. In fact, it is quite common that the extension of well-established kinds expands beyond its most representative paradigms. For example we know that *Tyrannosaurus* or *Stegosaurus* are paradigmatic examples of dinosaur; but, according to modern science birds evolved from dinosaurs and therefore they should be supposed to be dinosaurs.

The effective weak point of the causal theory is that, holding that there is a direct relation between words and objects, it assumes the individuality of the objects that should constitute the domain of scientific knowledge (Stachel 2005: 203-204). Both the objections (structure-dependence of reference and no distinction between essential and superficial properties) are structuralist and contextualist. At first, as regards physics, the causal theory fails to recognize that the individuation of physical entities deals with their distinguishability, rather than with their identity. As we have seen referring to selective realism, it is plausible to treat objects as clusters of descriptive properties (French, Krause 2006: 210); and, additionally, the notion of causal chain is problematic for quantum mechanics, since it will not allow to differentially name two photons in the same state. And, secondly, also in biological sciences it is sometimes difficult to figure out the identity conditions for the relevant entities; for example, as we will see in the next chapter, the concept of gene. So, let us see how we can face these problems.

The process of puzzle solving that characterizes normal science consists in the recognition of reasonable analogies to apply old problem solutions (paradigms) to new problems. Therefore, the meaning of scientific terms cannot be rigid: it should change relative to the problem situations it is applied to. Kuhn compares this feature of scientific terms to metaphor: both kind terms and metaphors do not satisfy the traditional conditions to consider an expression meaningful (such as necessary and sufficient conditions, definitions or reference). The meaning of scientific terms is determined by the juxtaposition of series of exemplary cases, which makes the speaker aware of the network of similarities and differences to which the term belongs and highlights the features that permit the term to be applied. And, obviously, the addiction of new exemplary cases can modify the meaning of the term: meaning is not the sort of thing that can be established once and for all. Kuhn has always preferred the flexibility of paradigms to unadaptable rules of application: exemplars are essentially flexible and their usefulness depends just on their flexibility. As Wray points out: “A concrete scientific achievement can only function as an exemplar if it can be altered or modified in ways that enable scientists to solve other problems” (Wray 2011: 61). And the metaphorical juxtaposition of exemplary solutions which determines the meaning of scientific terms cannot be justified from the viewpoint of the rigid designator.

In Section 4, I will specifically explain how exemplary cases contribute to the process of meaning fixing and change. But, actually, it should be clear that Kuhn’s model is Wittgenstein’s concept of family resemblances. Wittgenstein says that when we try to figure out whether all games have something in common, we realize that they do not share a common nature, but a network of criss-crossing similarities and differences. His claim is that there is no set of necessary and sufficient conditions that all games satisfy (and which is satisfied only by games). From Kuhn’s perspective, the fuzzy boundaries of Wittgenstein’s family resemblances are more useful to explain the possibility to extend the meaning of a term to cover new problem situations. Moreover, of course, he agrees with Wittgenstein’s attack to essentialism: he rejects the distinction between essential and accidental properties. Grouping objects in similarity-difference classes, there are no restrictions about which features we can use[19]. And, according to Kuhn, this is not a failure; rather, it has a positive effect on scientific progress: “one should here withhold phrases like ‘vagueness of meaning’ or ‘open texture of concepts’. Both imply an imperfection, something lacking that may later be supplied” (Kuhn 1977: 316 fn. 21). On the contrary, Kuhn says that essential and non-essential properties are not different from a metaphysical standpoint. Therefore such a distinction cannot account for the recognition of the meaning of a term: superficial properties are as important as essential properties. Let us come back to the second argument against the causal theory of reference: the chemical structure H_2O is not sufficient to identify water, because it possesses further properties, such as “being liquid”. And usually we need to recognize several properties that an object should possess to categorize it in a certain set: again, the similarity-dissimilarity relations are used to fix the meaning, that is to say the relevant positions of the terms in the lexical network. So, water should be H_2O and liquid to distinguish it from solid and gaseous H_2O.

One might think that it is a problem of conventions and that we can solve it by recognizing different meanings of the same term. For example, if the term water is a

19 For a discussion of Kuhn’s concept of family resemblance and its application to the problem of the wide-open texture see Andersen 2000a. I will focus on it in Section 4.

flexible term that applies to different situations (without essential properties), we should introduce two or more different terms to express the different meanings of the term water: $water_1$, $water_2$ and so on; a term for each concept of water. This solution implies that the differences between the uses of the term "water" are only semantic and that they can be ruled out by a conventionalist approach. But Kuhn does not agree: the polysemous nature of scientific terms is not an imperfection that we can put right through conventionalist solutions. It is an intrinsic quality of natural kind terms and it is useful for science; it is a question about things and not only about words. In fact, although we can try to isolate the features related to $water_1$ from the ones related to $water_2$, both $water_1$ and $water_2$ are kind terms which generate expectations about the behavior of the objects they refer to. And since scientific concepts have the structure of family resemblances, such expectations criss-cross and overlap each other: the question should be solved by means of empirical evidence and facts, and not by linguistic conventions. Indeed, since terms are polysemous, they can arrange their flexible boundaries to attach to new situations. The example of liquid, gaseous and solid water can appear trivial, but it is more realistic if we think to things such as heavy water: is it really water? Or is deuterium hydrogen? The problem is that sometimes the expectations related to rival scientific terms are problematic for the relevant symbolic generalizations; this is the case of Newton's mass and Einstein's mass (as well as the phlogiston theory and modern chemistry) and it entails referential discontinuity.

So, at this point, it is time to sum up the results of the second section:

1. Meaning and reference are different things. Meaning is determined by the structural relations between the terms of the lexical network. Over the course of scientific revolutions, such structural relations deeply change and therefore meaning changes occur. Referential change is not a primary problem: it is a by-product of meaning change, since reference is language-dependent.

2. Scientific terms are basically polysemous and their meaning is structured according to family resemblances. This is not a problem that we can solve and it is not a problem at all: the vague boundaries of meaning allow scientists to apply attested concepts to more and more significant problem situations; and this is the essence of both normal and revolutionary science.

4. Conclusions

My conclusion is that Kuhn rejects the referential approach to meaning change because it is not consistent with his theory of meaning. Above all, the referential approach does not take into account the fact that, as far as there are no necessary and sufficient conditions to identify a term, meaning is determined by the respective positions of the terms in a lexical structure. And such a structure depends on the use of scientific terms in experimental situations, established by means of paradigmatic examples of correct applications. From this viewpoint, isolated words have no distinct meaning (which can be individuated by their reference); rather words differently actualize their meaning relative to the context they are applied to. This has a positive effect on science, because the application of old concepts to new situations is not an "algorithmic" process. The flexibility of scientific terms is, to that extent, a constitutive condition for scientific progress. Approaching the questions of incommensurability and meaning from a referential viewpoint, therefore, is not useful, since referential change is a function of meaning change. Reference is not a neutral connection between words and reality: it depends on the lexical structure from which it is inferred; it is partially internal and language-dependent. In the next section, I will focus on the concept of lexical structure, explaining what it means for natural kind terms to be part of semantic networks; and, above all, I will analyze the role played by exemplary cases in the determination of meaning, the main open question of this chapter.

Section 4: Lexical Structures and the Open-texture

1. Introduction

In the last section, I have discussed Kuhn's view about natural kinds. Natural kinds are a basic element of any scientific theory, since, in order to organize the scientific experience of nature and to predict new phenomena, scientists use natural kinds to categorize the objects of their inquiry, allowing us to have expectations about their behavior and the properties related to such objects. The theory of natural kinds has many metaphysical implications, since it deals with the idea that natural kinds are not only "words", but, rather, they correspond to some essential aspects of the world (and then the categorization provided by natural kinds mirrors a categorization that is "really there", in the objective world). But I will not discuss these problems here. Rather I will develop other aspects of the meaning of natural kind terms that I have not discussed in the last section. Here, I will go into the details of Kuhn's theory of "scientific lexical structures", that is to say taxonomic structures of kind terms and their mutual relationships, which supply a categorization of experience and the boundaries of possible experience. The main problem, as I have said in section 3, is that the meaning of natural kind terms is at the same time stable and flexible, since they can be applied to new problem situations in unexpected ways (and therefore there are meaning shifts), but there are some constraints to such meaning shifts (otherwise every kind term would be consistent with any state of affairs).

The aim of this chapter is to analyze the consequences of a theory of the meaning of scientific kind terms which takes into account the constitutive open-texture of concepts (including scientific concepts). The premise of this chapter, that I have spelled out in Section 3, is that, from the viewpoint of the dynamic of scientific theories (a historical viewpoint interested in the relations between successive theories), open-textured concepts are useful because they are as flexible as we can apply them to more and more problem situations. Obviously, this does not mean that scientific theories cannot be fully formalized *a posteriori*. The open-texture perspective refers to the dynamic of scientific discovery during "normal science" and the related activity of puzzle-solving. In this context, scientists have to adapt the boxes of their theory to accommodate the experimental data and meaning shifts are acceptable. The concept of open-texture was introduced in the philosophy of science by Friedrich Waismann, referring to the debate about the principle of verifiability. I will start with this concept, but I use a notion of open-texture which is different from Waismann's one and therefore, in the rest of the chapter, I will use Kuhn's theory of concepts and paradigms as an example of open-texture of scientific concepts and apply the results to the theory of meaning that I have introduced in section 3. My conclusion is that the theory of meaning related to the open-texture of concepts should have the following features:

1. meaning eliminativism: words have no core meaning, since the open-texture does not allow us to associate words with abstract descriptions or rules of

application;

2. contextualism: words have meanings only in so far as they are related to their "environment", i.e. their context of application and the relative semantic field;
3. structuralism: since meaning depends on the context of application, it depends on the structural relations between the terms that belong to a given semantic network.

Moreover, I will also conclude that the concept of open-texture is misleading because it suggests the idea that there are no limits to the extension of concepts and that every object can be included in any kind. Since I do not agree, I will propose a hypothesis about the epistemic boundaries of the polisemy of scientific terms.

2. Kind terms and taxonomic structures

As I have said in section 1, for Kuhn, one of the most important functions of kind terms is to provide the taxonomic structure that enables scientists to gain access to a given section of the "world of science". In his latest works, he develops a taxonomic version of his incommensurability thesis (Bird 2000: 191-202, Chen 1997, Hacking 1993, Sankey 1998, Massimi 2015), according to which, since taxonomies supply a principle for the categorization of experience (and, in a Kantian way, they are constitutive conditions for the possibility of experience in a given experimental field) and scientific revolutions involve changes in the taxonomies (for example the categorization of the objects related to the kind terms "planet" and "star" changed over the course of the Copernican revolution; or the chemical categories "compound" and "mixture" referring to the chemical revolution), incommensurability depends on the meaning shift of the most important terms of the taxonomy:

> What characterizes revolutions is, thus, change in several of the taxonomic categories prerequisite to scientific descriptions and generalizations. That change, furthermore, is an adjustment not only of criteria relevant to categorization, but also of the way in which given objects and situations are distributed among preexisting categories. (Kuhn 2000: 30)

As I have already sketched, a taxonomic structure consists in a "lexicon", a network of interrelated terms, which are introduced together by means of the same scientific laws and examples of application. Each term is a node in such a lexical structure and the essence of the structure is the relation between its nodes. They are locally holistic structures, since the term mass cannot be explained without referring to the term “force”, but this does not mean that all the relations between the terms of the network are essential (but, rather, only those associated with detection properties). “Local” means that usually the hard-core of the lexicon consists in a small set of terms (for example, the terms of the second law of motion) and that incommensurability is linked only to these sections of the lexicons[20].

A lexical structure is made of natural kind terms, a specific set of taxonomic terms; it is an organized vocabulary of kind terms, which constitute the taxonomy of the theory. According to Kuhn, kind terms have at least three features (Kuhn 2000: 230-231):

1. They are learned in use, i.e. they are introduced by means of the juxtaposition of a sufficient amount of accepted examples of application. Successful communication does not require necessary and sufficient conditions for the application of kind terms; it requires only that the members of the relevant scientific community operate with homologous lexical structures.

2. They are projectible. This is the reason why kind terms are particularly important for scientific taxonomies and may entail incommensurability. Kind terms give rise to expectations about the behavior of the entities they refer to and their properties, since knowing the meaning of a kind term implies knowing some generalizations satisfied by their referents.

3. In order to save the co-referentiality of kind terms and the communication between the specialists, the expectations and the predictions related to such terms should be compatible. If the expectations are not consistent with each other, the taxonomic structure collapses and the communication between scientific communities that accept different predictions about the same kind term is problematic.

This is one of the most important points of the theory of the lexical structures: the so-called “no overlap principle”:

20 See Kuhn 2000: 36. The thesis of local incommensurability is a modest version of the incommensurability thesis. In *The Structure of Scientific Revolutions* the meaning of scientific terms was considered dependent on the theoretical claims of the theory in a more thick way.

> No two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus. There are no dogs that are also cats, no gold rings that are also silver rings, and so on: that's what makes dogs, cats, silver, and gold each a kind. Therefore, if the members of a language community encounter a dog that's also a cat (or, more realistically, a creature like the duck-billed platypus), they cannot just enrich the set of category terms but must instead redesign a part of the taxonomy. (Kuhn 2000: 92).

The principle states that taxonomies are organized according to a hierarchical structure: the categories should not have occurrences in common, unless one of them includes the other; or, in other words, a natural kind can include another kind, but if and only if it is a high-order kind which subsumes the other in the same gender. According to Hacking's interpretation, taxonomies are structured by the relation K or "kind of" relation, which is transitive and asymmetric. It is transitive since, if horse is a kind of mammal and mammal is a kind of animal, then horse is a kind of animal. And it is asymmetric since, if horse is a kind of mammal, then mammal is not a kind of horse. Given K, the head of each kind that belongs to the domain of K is a category. Hacking accepts the projectibility of kind terms, since it is a logical consequence of the concept of natural kind. Science does not deal only with the organization of the actual experience or with what is the case, but also with what would be the case in other conditions. Every natural kind and the respective laws have unexpected implications, which have to be tested by means of empirical investigation. Obviously, this does not mean that all the predictions related to such terms and generalizations will be experimentally confirmed. The projectibility of natural kind terms does not imply that all the expectations and lawlike generalizations are right; it defines "the class of possibilities envisioned or capable of being taken seriously by a science at a time" (Hacking 1993: 296).

Unlike the principle of projectibility, the no-overlap principle is controversial. I cannot discuss it in depth now, but in the history of science (especially in the history of biology and the history of chemistry) there are many examples of mature theories that accept cross-classification; the isotopes are a classic example, but also the distinction between metals and non-metals presents some exceptions concerning the allotropic forms of the elements[21]. For example, Wiggins states that cross-classification is indispensable to many scientific disciplines such as ethology, linguistics and many

21 See Bird 2000: 200-201 and Wiggins 2001: 67 fn.7 for some examples and references about the question of cross-classification. See also Tobin 2010.

others (Wiggins 2001: 67). Therefore, the use of different overlapping natural kinds to categorize experience and make prediction about future states of affairs does not seem to be a great problem for science; and thus, it is arguable that the no-overlap principle should be rejected.

Moreover, at first sight, the no-overlap principle seems to be inconsistent with some claims that I have presented in the previous sections (especially section 3). One of Kuhn's basic claims is that the boundaries of scientific concepts are as flexible as they can be modified to be applied to new problem situations. Since scientific concepts look like family resemblances (and then, as I will explain in the next sections, they are open-textured concepts), they have no “core” or privileged semantic area which cannot be, in principle, modified over the history of science. On the contrary, the no-overlap principle suggests a limit to the open-texture of natural kinds (although it refers only to those small groups of natural kinds which constitute the lexical structure of the theory), since it states that the meaning of kind terms cannot change in such a way to overlap the expectations related to another term of the same semantic network; if it is the case, we will turn into a scientific revolution and therefore the relevant theories are incommensurable. In the following sections I will discuss the open-texture of scientific concepts, in order to justify the application of some restrictions to meaning change (different from the no-overlap principle) without rejecting the idea that the vagueness of meaning and the open-texture of concepts are not faults to be corrected, but, rather, a condition for the possibility of new applications of old concepts. My perspective takes into account both the importance of meaning changes in the history of science and the stability of meaning referring to the most basic kind terms of the current theory.

3. Open-texture and paradigms

Waismann introduces the concept of open-texture to reject the verificationist program which states that the meaning of scientific statements consists in the conditions for their verification (see Shapiro 2006: 210 and ff). According to Waismann, every statement is linked to an infinite amount of possible verifications and therefore there is a connection between the fact that in many cases there is no conclusive verification and the fact that our concepts are not entirely delimited (Waismann 1968: 120). He means that, when one uses a concept, she cannot be aware of any possible situation to which the concept applies or may apply; and he tries to extend these results to a theory of the meaning of natural kinds. One of his examples is the notion of "gold":

> The notion of gold seems to be defined with absolute precision, say by the spectrum of gold with its characteristic lines. Now what would you say if a substance was discovered that looked like gold, satisfied all the chemical tests for gold, whilst it emitted a new sort of radiation? 'But such things do not happen.' Quite so; but they might happen, and that is enough to show that we can never exclude altogether the possibility of some unforeseen situation arising in which we shall have to modify our definition. Try as we may, no concept is limited in such a way that there is no room for any doubt. We introduce a concept and limit it in some directions; for instance, we define gold in contrast to some other metals such as alloys. This suffices for our present needs, and we do not probe any farther. We tend to overlook the fact that there are always other directions in which the concept has not been defined. And if we did, we could easily imagine conditions which would necessitate new limitations. In short, it is not possible to define a concept like gold with absolute precision, i.e. in such a way that every nook and cranny is blocked against entry of doubt. That is what is meant by the open texture of a concept. (Waismann 1968: 120)

Waismann's point is that a term is ultimately defined when the situations to which it applies are completely described, i.e., when we have a complete list of all circumstances in which we can use the term. This would be a complete definition, which anticipates once and for all every question about the use and the application of such a term. But, according to Waismann, this is not possible, since every situation is characterized by an infinite amount of features and therefore we should accept the open-texture of empirical concepts.

Even if Waismann has the merit of being the originator the concept of open-texture, I think that his theory has a big weakness. In this model the open-texture depends on the world, since the world often manifests unexpected behaviors and we cannot arrange our concepts in order to predict any unpredicted circumstance. But this remark deals with the nature of the world (and the troubles with induction), while the open-texture deals with the nature of concepts and not with the surprising behavior of the world (Kindi 2012: 41). The paradigm of the open-texture is Wittgenstein's theory of family resemblances, the idea that the words cannot be defined by means of a set of necessary and sufficient conditions, because, for example, there is no property that all games share. As I have said in the last chapter, Kuhn, too, uses Wittgenstein's theory to explain how scientific concepts are learned and how they attach to reality by means of paradigmatic examples of applications. In his model the open-texture does not depend on how the world behaves, but on the premise that scientists need the open texture to apply old terms (and paradigms) to new problem situations by means of an analogical process, i.e. the recognition of patterns of similarities and differences between perceptions, objects and problems. Kuhn does endorse the open-texture because paradigms (accepted scientific achievements) are such if and only if they can be modified to solve new problems. According to this model, during their scientific training, scientists learn at the same time something about the world and something about the words, i.e., the meanings of the words and the behavior of the "pieces of world" associated with such words. Kuhn's theory of meaning is part of a more complex question about the nature of concepts and the organization and the transmission of scientific knowledge. But here I will focus on the theory of meaning linked to the open-texture of concepts. For this purpose, I think that such a theory should have three features: meaning eliminativism, contextualism, structuralism.

4. Meaning eliminativism, contextualism, structuralism

a) Meaning Eliminativism. According to meaning eliminativism, words have no core meaning and are not associated with abstract conditions or rules of applications. This idea fits well with Kuhn's paradigms, since they are not comparable to explicit and compulsory rules. No theory of meaning can exhaust the concept of meaning (both in ordinary and in scientific languages) and therefore here the slogan "meaning is use" (Kuhn 2000: 62) means that one can use everything he wants to identify the meaning of a word[22]. Since eliminativism rejects the very concept of meaning (intended as isolated or conventional meaning), it is misleading to continue using the word "meaning". On that matter, Récanati introduces the expression "semantic potential" to describe the new role played by "meaning" according to his perspective. The semantic potential of the word P is a collection of the legitimate situations of application of P (Récanati 2004: 148). Obviously, this is an open collection, because one can imagine new situations of applications, which are consistent with the semantic potential of P. The legitimate situations of application are paradigmatic examples of application of the theory (for example the inclined plane in classical mechanics) and they are learned by students during their scientific training. Training consists in the exposition to a sufficient amount

22 Thus, this model does not deny that intensions and descriptions are important to understand the meaning of a word or a statements; rather, it denies that they are necessary and sufficient conditions.

of situations to which P applies and to which P does not apply. Récanati calls the collection of legitimate applications of P "source situations" and the intended future applications "target situations". In order to apply P to new problems, one has to recognize some similarities between the "source situations" and the "target situations".

b) Contextualism. According to meaning eliminativism, conventional and isolated meanings do not exist. Words are meaningless in so far as they are not associated with their specific context of application. This remark implies that, since there are no necessary and sufficient conditions to apply a word, only concrete applications are meaningful. In the received view about meaning, context is an "accidental" feature which modifies the conventional meaning of the words. For example, coming back to Waismann's thought experiment, the conventional meaning of the word gold is "the element with chemical number 79" or "a dense, malleable and ductile yellow metal". But, depending on the context of application, the meaning of the word "gold" changes and it may refer to some kind of coin or money. However, the conventional meaning is the most important and the others are mere by-products of the combination of conventional meaning and context. On the contrary, the contextualist viewpoint states that the context of application is not a variable, which influences the meaning from the outside: it is a constitutive element of meaning. The meaning of a word (its application to the target situation) is determined by the combined action of accepted applications and the new context. Applying a term to the relevant target situation entails identifying some similarities between the source situations and the target situation, but the relation of similarity is based on the accepted uses of the term and therefore similarity relations are contextually determined as well. Since two perceptions, objects or problems may be similar according to a great amount of features, the similarity between different applications of the same term deals with the open-texture as well. As I have pointed out in section 3, Kuhn applies this argument to the different meanings of the term "water" (solid, liquid and gaseous), since the application of the term "water" to substances which present different states of aggregation is just this sort of analogical process. The same remarks concern borderline cases such as heavy water or the isotopes (LaPorte 2004: 107-110).

c) Structuralism. The model based on the similarity between different problem situations is necessarily a structuralist model. That is at first because, since the meaning of a term depends on the accepted applications and each application involves several terms, no meaning is available independently of the meaning of other words applied to the same empirical situations and puzzles. For example, the basic terms of classical mechanics are introduced together by means of the laws of motions and their exemplary applications and therefore, for example, the meaning of the term “mass” cannot be separated from the meaning of the term “force”. Moreover, the determination of meaning is a “structural” process because it deals with similarities and differences between problem situations. In order to fix the meaning of a term, one has to master the relevant “contrast set” (Tversky 1977)[23], i.e. the set of objects referring to which we are going to establish the relations of similarity. Learning how to use a word does not merely mean knowing the situations to which it applies; on the contrary, one should also know similar situations to which it does not apply and you have to apply another word of the same network. A contrast set gives rise to a lexical structure of interrelated terms, characterized by relations of similarities and differences. The relations of similarity can be introduced both by direct perception (for example by ostension) and by descriptive statements associated with examples of application. What is important is that learning requires the exposition to sets of differences and not only a single act of ostension or a theoretical description. Each term is a node of the lexical structure and its meaning is the relative position in the structure; the position is determined by the past applications referring to the other terms of the network. Kuhn’s example is the following:

> Let me take “*doux*” to be a node in a multidimensional lexical network where its position is specified by its distance from such other nodes as “*mou*”, “*sucré*”, etc.; […] the meaning of *“doux”'* consists simply of its structural relation to other terms of the network. Since “*doux*” is itself reciprocally implicated in the meanings of these other terms, none of them, taken by itself, has an independently specifiable meaning. (Kuhn 2000: 55)

In this kind of network, the use (the relative position) of each word tells us something about the use of the other words, since they are learned together by means of the same set of exemplary applications and laws, determining the circumstances under which each term applies (or does not apply). And, as I have said referring to normic and nomic concepts, this applies both to groups of ordinary term such as “doux”, “mou” and

23 Kuhn describes the importance of the contrast set as follows: “Most kind terms must be learned as members of one or another contrast set. To learn the term 'liquid', for example, as it is used in contemporary nontechnical English, one must also master the terms 'solid' and 'gas'. The ability to pick out referents for any of these terms depends critically upon the characteristics that differentiate its referents from those of the other terms in the set, which is why the terms involved must be learned together and why they collectively constitute a contrast set.” (Kuhn 2000: 230).

“sucré” or “goose”, “swan” and “duck” and to highly theoretical terms such as “mass”, “force” and “acceleration” with some minor differences that I have exposed in section 3.

Finally, in the eliminativist, contextualist and structuralist model, since scientific terms are not characterized by abstract conditions of application or definitions, the meaning of such terms is contextually determined by means of the exposition to their accepted applications (and to examples of mistaken applications) and the relations with the other terms that applies to similar situations. Isolated words have no meaning, but, rather, a semantic potential (the set of the source situations or the paradigmatic examples of application), which manifests itself in different ways according to the empirical context to which the term applies. This allows us to save the advantages of the open-texture of concepts and, at the same time, to introduce some constraints to the open-texture of scientific terms. I will face this problem in the following sections.

5. The structuralist framework

Now, let me try to express this concept in more rigorous terms. A good way to present Kuhn's ideas in formal terms is to appeal to the structuralist program in the philosophy of physics, which was originally born just as an interpretation and improvement of this model of scientific knowledge (for example Stegmuller 1976). There are several variants of the structuralist program, but here I use the terminology and the formalism of Sneed and Stegmuller's program (see Andreas, Zenker 2014). According to this program, basically, a scientific theory consists of the following elements:

(1) M_p is the class of potential models of the theory, that is its theoretical apparatus, including theoretical components. For example, referring to Hooke's law, a set of particles, a set of springs and the relevant constants, the mass of the particles, their positions and the mutual forces at a time *t*.

(2) M is the class of the actual models of the theory, that is the empirical laws related to the theory or, in other words, the set of potential models that are not excluded by its fundamental laws (or the potential models satisfying the system's equations).

(3) M_{pp} is the class of partial potential models of the theory, that is its non-theoretical basis, or the corresponding non-theoretical models. For, example, in the case of classical mechanics, if we consider masses and forces to be theoretical, it will contain only the positions of the particles as functions of time.

Moreover, (4) C are the constraints, that is to say, the conditions connecting different models to the same theory, (5) L is the class of links connecting models of different theories and (6) A is the class of admissible blurs.

The mathematical core of a Kuhnian theory can be represented as K = {M_p, M, M_{pp}, C, L, A}, that is the formal-theoretical part of the theory. But, as I have said, scientific theories are not merely mathematical structures, but also concrete (paradigmatic) applications. So, there is another element:

(7) I is the domain of intended applications of the theory, that is the pieces of the world that constitute its concrete domain or the particular phenomena to which its core applies or is thought to be applicable; for example, the solar system belongs to the domain of intended applications of Newton's laws. So that, a theory element can be defined as an ordered pair T = {K, I}, where, of course, some applications of the theory are sufficiently confirmed by the relevant community, while others are not.

The most important point of I, corresponding to Kuhn's paradigms, is that I is an open set. The structuralist program asks us to give up the concept of *one* domain of individuals to which the theory applies and recommends to use the concept of several intended applications. And the domain I cannot be extensionally given, but only by means of a set of paradigmatic examples (nor it cannot be intensionally given, since there are no shared properties to define the class).

In order to make this clear, let us come back to Wittgenstein's example of games (Stegmuller 1976: 171-172). Recall that, according to Wittgenstein, there is no property which is common to all those things falling under the concept of game, but, rather, only occasional overlapping resemblances. So, to identify the concept of game, we use to start by trying to list typical cases of game and, of course, this list can be modified by addictions and subtractions: some activities classified as games may be dropped at some time or the other way round. In the end, anyway, we should arrive at a minimal list: from now on, new games can be added on the basis of our attested knowledge, but the list must not be reduced. This does not mean that the minimal list constitutes the (core) meaning of the term "game", but that, for epistemic reasons, it is indispensable to apply the term; or, in the terms of scientific theories, it provides the minimal interpretation of the core K. So, assuming that G is the concept of game, we should determine the list G_0, that is the list of paradigms for game, by using the method of the paradigmatic examples. Given G and G_0, the following requirements should be fulfilled:

- G_0 should be extensionally given, that is to say, we should provide an effective list of the elements of that set;
- None of the element of G_0 can be removed without changing the concept of G (or, in the case of scientific theories, giving up the whole theory);
- The elements of G_0 can have some properties in common (for example, all games share the property of being human activities), but those properties, although may be necessary conditions, cannot be sufficient conditions for being members of G_0. No matter how many pertinent examples we may provide, we cannot derive any sufficient condition for membership.
- The conditions for membership in G are necessarily vague and fuzzy. Given an individual x that actually does not belong to G_0, it should share a significant amount of features with the members of G_0, in order to be considered a member of G.
- Given an individual x that actually does not belong to G_0, there is no finite list of properties such that, if x is proven to posses all the properties enumerated in the relevant list, the membership in G is guaranteed for x.

Basing on these concepts, it should be clear how Kuhnian meanings work. The set I of intended applications of a theory is defined by means of a fundamental set of paradigms. I_0 corresponds to G_0, or, in other words, the set of legitimate applications of the relevant theoretical concept: the source situations. Therefore, physics students are introduced to I_0 by a list of paradigmatic examples of application, accepting the vagueness of the concept. This model has two major advantages: at first, as I have said, the fact that I is an open set allows scientists to apply the theory to more and more empirical questions; at second, if the theory fails in applying to some element of I, scientists may merely drop the application, without rejecting the theory. In such a way, as I have said in section 1, the mathematical core of the theory is immunized against falsification. But, at the same time, this immunity does not extend to the concrete applications. If, at a given time, an element of I_0 resits explanation by its theory, we should abandon the theory. This is just the concept of concrete structure that I have outlined in section 2, since the set of applications provide the minimal interpretation of the mathematical formalism. I will come back to the concept of interpreted structure and its relation with the concept of truth in Part 3. But, now, I shall distinguish this model from others that, at first sight, may appear similar.

6. Similarities and differences with other models: prototypes

The model I have sketched in the previous sections presents some similarities and differences with other semantic models, especially with the semantic of prototypes and with the finitist theory of meaning.

The notion of family resemblance has already been applied to the nature of scientific concepts as well as to Kuhn's theory of empirical concepts (Andersen, Barker, Chen 2006). The idea is that we can explain meaning shifts admitting that the different meanings of the same term are not independent from one another, but, rather, overlap each other (like family resemblances). But, according to the standard theory of prototypes, each concept has a "prototype", which is considered the typical example of that concept, that we can infer from its actual applications[24]. Then, once we have been exposed to a sufficient amount of prototypes for an efficient categorization of the world, we can classify the other objects by means of the evaluation of their degree of similarity to the relevant prototype. In other words, the different meanings crosscut to generate intermediate meanings (and intermediate degrees of meaning), which are a mixture of the different meanings (and, of course, their boundaries may change with time). Obviously, this theory maintains that concepts are open-textured, since the set of possible future applications of the concept does not require necessary and sufficient conditions for the applicability, but, rather, the perception and the recognition of the similarity with a prototype or a set of prototypical applications.

This interpretation is convincing and it fits well with many theses that I have presented. Kuhn himself says that a necessary condition for the analogical (or "metaphorical") application of a term to new cases is the knowledge of the primary meaning of such term (its literal meaning), which, according to this interpretation, we can identify by means of the prototypical application (Kuhn 2000: 62). The analogy between different problem situations is the linkage between the literal use of the term and the new accepted application. But this theory has a weak point. In fact, it uses the

24 Some contemporary theories of prototypes reject the existence of a single prototype for each concept (established once and for all). But here I refer to the classic theory of prototypes because it directly addresses the question of the stability of reference.

concept of family resemblance to say that every network of similarities and differences presents a main area, a section of the network that is the most representative of the different meanings of the terms (and that we identify such area with the prototype, i.e. with the primary use of the term); and, finally, we can infer the analogical uses of the term from the prototype. But Wittgenstein's family resemblances do not require the existence of a distinction between "center" and "periphery" of a network. For Wittgenstein, all the nodes of the lexical network have the same importance and therefore, we cannot identify the prototype of a family resemblance.

For example, take the concept of gene, a "concept in tension" (Falk 2000) that evolved during the history of biology. At first, it was introduced by Wilhelm Johannsen, basing on Mendel's hypotheses; a gene was thought to be the "special conditions, foundations and determiners which are present [in the gametes] in unique, separate and thereby independent ways [by which] many characteristics of the organism are specified" (Johannsen 1909: 124). But the meaning of the term "gene" changes over the course of scientific progress and many historians suggest that it is a term that is meaningful only in some contexts in which it is used. For example Griffiths and Stotz (2006) individuate three different meanings of the concept of gene and state that the identification of the right meaning depends on the relevant context in which biologists work.

The first meaning is the instrumental gene. It is the traditional gene: it is identified by means of its phenotypic effects and is employed to predict the phenotypic effects of experiments of hybridization between organisms. Therefore, it is important in the context of the interpretation of experiments that involve the relation between genotype and phenotype by means of hybridizations, since the visible and heritable characters of the organism should be interpreted in such a way to permit their genetic analysis.

The second meaning is the nominal gene. It is a practical tool which does not imply that the relevant scientific community has a clear idea about what is a gene, but only that it is linked to the use of databases containing nucleotides sequences and that genes are sequences which have been codified as genes and have been confirmed by scientific communities. This is something like a description of the gene discovery method, since a gene is such if it is similar to other genes. It is useful for communication between scientists.

Finally, the post-genomic gene is the gene used in molecular biology. It plays a functional role in contemporary biology, since it represents the project concerning the relation between genome structure and genome function, but "with a deflationary picture of the gene as a structural unit" (Griffith, Stotz 2006: 99-100). According to this perspective, the gene is the set of things that you can do with the genome and, although it is still considered a representation in the DNA of the molecule we are studying, it can be distorted and fragmented and this is a problem for the traditional interpretation of the relationship between genotype and phenotype.

Although, with a pragmatic approach, many biologists think that the gene is only an operational concept, all these meanings are grounded in the experimental practice of biology. Therefore, we may think that, rather, the concept of gene is a multi-level concept whose meaning depends on the context in which it is used. This rules out the conventionalist solutions, i.e., a real distinction between the different meanings of the term "gene": $gene_1$, $gene_2$ and $gene_3$. According to the projectibility of kind terms, every term generates expectations, which sometimes overlap each other. This is a matter of facts and not a matter of words, since the problem is that overlapping meanings might lead to different anticipations about certain kinds of phenomena (for example the relation between genotype and phenotype). But, at the same time, referring to the multiple and different uses of the concept of gene, it is hard to find something like a prototype, or a literal meaning, of the concept of gene. Therefore, I will try to reconsider the role of the primary meaning of scientific terms. My idea is that the thesis that scientific terms have meanings, which cannot withstand a major revision over the history of science is, to some extent, correct, but it does not depends on the meanings of the words or their reference. It is neither a metaphysical nor a linguistic argument; rather it is an epistemic point about the organization of scientific knowledge and the testing process of scientific theories. I think that Kuhn was interested in keeping unchanged the literal meaning of scientific terms because, if words had no primary meaning, then (according to Kuhn) we would be allowed to accept any meaning change without major problems, and therefore incommensurability would not exist. But Kuhn could not accept the rejection of the incommensurability thesis (Kindi 2012: 41). I do not agree with this point and I will provide a different interpretation in the following section, where I will distinguish my perspective from meaning finitism.

7. Similarities and differences with other models: finitism

The finitist model is Wittgensteinian as well, since it results from an interpretation of Wittgenstein's theses about rule following and family resemblances. According to the finitist, when we introduce a concept P (showing exemplary applications of P), any possible use of P can be interpreted in such a way to be consistent with the initial introduction of the concept. From a logical viewpoint, the relation of similarity between the source situation and the target situation does not pose any limit to the acceptable interpretation by the speakers (for example, a sample of black can be interpreted in such a way to include gray in the concept of black and so on) This view accepts the open-texture since it states that empirical concepts are not limited in any possible direction. Finally the correctness of later interpretations depends only on the community agreement (or disagreement) about that interpretation. This theory is not consistent with Kuhn's perspective for two reasons.

At first, we need some constraints to the concept of open-texture. It is not true that every application is consistent with any other application; otherwise, it would be meaningless to speak about scientific revolutions, because we could always arrange our concept to save attested knowledge against the introduction of new concepts. Therefore, the concept of open-texture is perhaps misleading, since it might suggest the idea that empirical concepts can be applied without restrictions. As I did in the last chapter, it may be better to use the term "polysemy", to mean that scientific terms may have different meanings (and there may be meaning shifts), but the boundaries of the "degrees of meaning" are not completely flexible[25]. I will explain further below what I mean.

Secondly, both Kuhn's view and finitism are sociological perspctives and take into account the role played by scientific communities in the history of science. But Kuhn does not say that the consensus within a scientific community fixes the correct use of the term (Bird 2000: 223-224). According to Kuhn, as we have seen, the meaning of natural kind terms is not the conventional result of public agreement; in his theory of meaning, world and language are interwoven. Kind terms are projectible and therefore their meanings and correct applications cannot depend on the actual beliefs of some scientific community. The sociological perspective is important because it explains (by

25 Waissman was aware of this problem and he tried to address it by means of the distinction between vagueness and open-texture (Waismann 1968: 120).

means of the description of scientific training) how scientists are introduced to the patterns of similarities and differences, which allow them to react in the same way if they share the same stimuli. Therefore, the operation of categorization of the experimental data that characterizes science does not deal with the consensus within a scientific community. My point is that the question of "family resemblances" and the question of "following a rule" are two different problems, although they share some features. And that from my viewpoint the former is the relevant problem, while I am not discussing the latter.

Finally, I have to clarify what kind of epistemic constraints determines the primary use of scientific terms. Meaning changes characterize both normal and revolutionary science. The difference is that revolutionary meaning changes involve the terms introduced by means of the most important laws of the theory; since, according to Kuhn, the elements of the lexical structures are the terms that scientific laws deploy. These equations constitute the mathematical core of the theory and therefore we can say that two theories (T_1 and T_2) are commensurable if they have the same lexical structure, or if the lexical structure of T_1 is preserved in the term of T_2. This means that the categorization of the world (the patterns of similarities and differences) provided by T_1 can be mapped into the categorization of T_2 without important meaning changes. But this idea is still consistent with finitism, because abstract physical laws do not specify how they should be applied to the empirical context and therefore they can be interpreted in such a way to be consistent with any possible state of affairs (for example the first law of motion cannot be directly falsified, since you can always postulate the existence of invisible forces). But, as I have said in section 2, lexical structures are not the kind of structure (like Russell's structures) such that any collection of objects can be arranged to correspond to that structure. Kuhn's structures are always connected to exemplary applications, which provide a minimal interpretation of them. The meaning of the terms involved in the lexical structure consists in the interaction between laws and sets of intended applications. Therefore, it is not true that every future application is consistent with any past application. Each application should be consistent with the minimal interpretation of the theory, which is connected to the properties of scientific "objects" that are considered significant for the empirical application and testing of the current theory (detection properties); for example, mass and force are this sort of property in classical mechanics. Obviously, following the distinction between detection and auxiliary properties, this is not a metaphysical distinction, but, rather, an epistemic distinction, since it depends on the organization of scientific theories. Specifically, it depends on the parts of the theory that scientists consider empirically testable (or the parts of the theory that are easier to test according to the current status and method of scientific inquiry). But what is important is that, if scientific terms have a "literal meaning" (which cannot be modified), it is not a consequence of our theory of meaning, but, rather, a consequence of the empirical nature of scientific investigation, which forces scientists to select the sections of the theories to apply to the empirical context. Therefore, the theory of meaning that I have presented can account for normal and revolutionary meaning changes by means of the notion of open-texture (or polysemy) without turning into fintism.

8. Conclusions

According to the perspective that I have outlined, the scientific experience of the world depends on the existence of natural kind terms shared by a scientific community, which constitute the lexical structure, or the lexicon, of the theory. The function of such a structure is to organize the experimental data according to a given taxonomy and, thanks to such a taxonomy, to allow scientists to make predictions about the behavior of the objects postulated by the lexical structure and their properties. Since scientific progress consists in the application of attested models to solve new problems, lexical structures present a dual nature. From the one hand, their constituents (natural kinds) should be flexible in such a way that scientists can arrange their boundaries to apply them in new contexts. But, on the other hand, there must be some restrictions to the possible applications of these terms; otherwise, we could not explain theory changes. My hypothesis is that the stability-instability of the lexical structures can be explained on the basis of a theory of the meaning of natural kinds, which accepts three theses: meaning eliminativism (isolated words have no meaning); contextualism (the meaning of the words is "actualized" by their context of application); structuralism (the meaning of a word consists in the relations of similarity and difference with the other words of its semantic field). In such a way, we can accept the open-texture of concepts and, at the same time, some limits to the open-texture. In my model, unlike Kuhn, the no-overlap principle is not one of these limits. Rather, I introduce a constraint related to the minimal interpretation of the structure, i.e., the coherence with the universal laws of the theory on the light of its interpretation provided by paradigmatic examples of applications.

Part 2: Against Correspondence Truth

In the first part, basing on Kuhn's ideas, I have defended a view on the meaning and the reference of scientific terms, which rests on the idea that the main part of scientific theories (the part that we cannot reject without giving rise to a referential change) is constituted by the combination of mathematical structures (illustrated by physical laws) and exemplary applications. One of the main conclusions I have drawn is that mathematical structures should be learned by examples and related to concrete problem situations in order to be applicable in everyday scientific practice. Unlike pure structural realism, I think that we need interpreted and concrete structure to fruitfully talk about the world science describes. Now, in this second part, I will turn my focus from the relation between meaning and reference to the question of scientific truth and show why, according to Kuhn, this view leads to the rejection of the correspondence theory. But, while the first part is intended as a reconstruction and improvement of Kuhn's ideas, the second part aims to be critical. After presenting Kuhn's arguments against correspondence truth, I will conclude that they are wrong and merely fails. I will focus on the elaboration of an acceptable theory of truth in the last part, but, actually I will claim that appealing to the nature of exemplary cases and the language-dependence of reference (the main theses of Part 1) does not entail that we should believe that scientific paradigms are neither true nor false (in any proper empirical sense). To achieve this aim, in sections 1-2, I will carry on the ideas that I have presented in Part 1 (especially sections 1-2) as regards the relation between physical laws and exemplary cases. So, these parts should be read in parallel: they both deal with how the structure of scientific theories is related to theoretical change, to pick out those parts that are essential to their empirical success (and, therefore, the rejection of which causes a theoretical change). I will focus especially on the role played by physical laws according to Kuhn (section 1) and how the process of scientific training is constitutive of scientific practice (section 2). On the contrary, in the next sections, I will directly focus on truth, explaining why, for Kuhn, basing on these claims, we should reject the correspondence theory of truth (section 3); and, finally (section 4), I will conclude that a weak correspondence theory can overcome the challenge of the incommensurability thesis and, in the end, there is no reason to give up truth if we follow the approach that I have defended in Part 1 and 2. This will suffice for the *pars destruens* of my work. In fact, in the last part, as *pars construens*, I will defend the weak correspondence theory against other objections and I will conclude that it is able to account for scientific knowledge.

Section 1: Normativity and Necessity: Kuhn, Wittgenstein and the Physical Laws

1. Introduction

In the first part of my work (Part 1 – sections 1-2) I have introduced the problem of the selection of those parts of scientific theories that cannot be rejected without turning into a radical theoretical (and referential) change; according to the perspective I am developing, those parts are physical laws in connection with exemplary applications, i.e. interpreted mathematical structures. This part (and the following one, that is Part 2 – sections 1-2) should be read in parallel with it. In fact, in this chapter I will discuss the structure and the role played by physical laws in scientific practice according to Kuhn. One of his most original claims, that I have presented in Part 1 – section 1, is that the physical laws are analytic propositions (or quasi-analytic or synthetic a priori). By analytic, he means propositions which describe situations that could not have been otherwise. For example, the state of affairs described by the first law of motion is consistent with any conceivable empirical situation, since one can always postulate the existence of invisible forces; the conclusion is that such propositions look like necessary proposition, or propositions dealing with the "real" essence of the world. But this idea seems to be inconsistent with another famous claim defended by Kuhn: the claim that the relation between scientific theories and reality is neither a metaphysical nor a purely empirical relation. Since the same set of empirical data may well be accommodated by different paradigms (and none of them is "grounded" in the objective reality), paradigms are, to some extent, arbitrary, or, in Kuhn's words, they are not the sort of things which can be true or false; their justification is a pragmatic matter. Therefore, my question is the following. Scientific paradigms have two features:

- (TA) Thesis of Arbitrariness: scientific paradigms do not deal with the essence of reality and cannot be true or false in any empirical or philosophical way. This is a thesis against the idea that scientific language mirrors the real nature of the world.

- (TN) Thesis of Necessity: physical laws are necessary propositions, since the states of affairs that they describe could not conceivably have been otherwise.

If one thinks to the necessity of scientific laws from a metaphysical viewpoint (they are necessary propositions because they reflect the essence of the world), arbitrariness and necessity are inconsistent. But I think that Kuhn does not refer to the metaphysical necessity of scientific laws. I cannot discuss the necessitarian view on the laws of nature here (see for example Armstrong 1983) and the question about the distinction between necessary and contingent truths in the empirical sciences. Rather, following what I have said in Part 1 – section 1 – I will analyze the kind of necessity that Kuhn attributes to the laws of nature and I will return on the comparison between the nature of physical laws according to Kuhn and the nature of grammatical rules according to Wittgenstein. My conclusion will be that, just like the necessity of grammatical rules comes from the normative role that they play in language games, (according to Kuhn) the necessity of physical laws is language-dependent and depend on their normative-methodological role.

2. Are scientific laws analytic propositions?

The arbitrariness of scientific paradigms is a famous topic and therefore I will be very brief about that and I will focus on the concept of necessity. Anyway, in many passages of his works, Kuhn seems to suggest that scientific laws are arbitrary in some way. Roughly, this depends on two arguments: a) the same set of experimental data can be explained and justified by means of many different paradigms, since more than one theory can be pointed out to interpret the same collection of empirical data (Kuhn 1970: 76); b) none of these paradigms can be said "grounded in or justified by experience" (Kuhn 1970: 146-148), since the empirical justification of scientific theories is a problem that cannot be solved through the mere instruments of logic and experience. The conclusion is that no experiment can force the process of theory choice, since the relation between theory and data involves elements that the logic of verification (or falsification) cannot justify; this is why Kuhn says that anomalies are not falsifications.

Thus, the correspondence theory of truth is not applicable to scientific paradigms. They are neither true nor false, because they are antecedent to any such correspondence (see Kuhn 2000: 90-104 for his rejection of the correspondence theory of truth). This is an anti-foundationalist viewpoint and has often been interpreted as a kind of conceptual scheme relativism such that, from (TA) it follows that:

- CSR (Conceptual Scheme Relativism): since no paradigm is grounded in experience, each paradigm is a conceptual scheme (at least partially) untranslatable from the lexicons of the other schemes. The truth-value of scientific propositions is not a matter of relation between theory and reality, but, rather, a question about the internal coherence of our system of beliefs.

I think that (CSR) is false, but I will demonstrate that it is not what Kuhn meant. In fact, I think that (TA) and (TN) can be better understood analyzing the concept of necessity that Kuhn attributes to physical laws and clarifying what he means by "necessary" and on what it depends.

In Part 1 – section 1 – I have explained how, since his "Postscript-1969", Kuhn has clarified the concept of paradigm, individuating its elements; so I will not repeat this point. I have already said that, for Kuhn, basically symbolic generalizations are one of the most important elements: they are the fundamental equations of the theory, usually expressed in formalized language and considered laws of nature by the relevant scientific community. But, as Kuhn says, symbolic generalizations do not specify how they should be applied to the empirical context. He distinguishes symbolic generalizations from exemplary problem solutions (for example $F= ma$ and the inclined plane) and moves the empirical content of scientific laws to their applications: they are applicable only thanks to exemplary cases; an isolated symbolic generalization is something empty of empirical meaning and application.

2.1 Distinction: analyticity and necessity

As I have said in Part 1 – section 1, Kuhn has tried to explain this feature of scientific laws many times. As I have just affirmed, Kuhn realized that these laws appear to be analytic and synthetic at the same time and uses different terms to express this property in tension: “analytic” (or tautologies or purely logical statements) or “quasi-analytic” or “synthetic a priori”. From Kuhn’s viewpoint, their most interesting feature is the ability to resist empirical falsifications, which makes them look like necessary propositions; but, obviously, if we have a more serious look at the nature of physical laws, this is really problematic, since physical laws have a strong empirical import as well. The factual or empirical aspect of symbolic generalizations cannot be ignored, since they work together with their empirical examples of applications (otherwise they would be meaningless) and consequently they are not the mere result of conventional stipulations and arbitrary definitions. The main point of his puzzlement is that, even though symbolic generalizations seem to be analytic, the meaning of their terms is not embodiable in definitions, while those of the terms deployed by analytic propositions usually is. I think that the origin of Kuhn’s trouble is quite clear: analytic does not mean necessary. There are at least three reasons which suggest us to reject the identification between analytic propositions and scientific laws according to Kuhn.

At first, The distinction between analytic and synthetic propositions consists in their logical form, while Kuhn’s distinction is grounded in the role played by a proposition in its own context. Kuhn’s analysis of the process by which physics students are introduced to the second law of motion (see Part 1 – section 1) shows that this law can be considered empirical or necessary depending on the role that we assign to it in the context of scientific training (in classical mechanics). Physics students can be introduced to the principles of classical mechanics in two different but equivalent ways: a) presenting the second law as a stipulation and the law of gravitation as empirical; b) reversing their epistemic status. From this point, Kuhn’s confusion between the notion of analyticity and the notion of necessity is clear. In fact, from this remark about the laws of classical mechanics, he concludes that “in each case one, but only one, of the laws is, so to speak, built into the lexicon. I do not quite want to call such laws analytic, for experience with nature was essential for their initial formulation. Yet they do have something of the necessity that the label ‘analytic’ implies” (Kuhn 2000: 71). Kuhn treats “necessary” and “analytic” as equivalent, but the second law of motion keeps unchanged its logical form independently of the way we are introduced to it. Therefore, physical laws are not analytic in the same way as ordinary analytic propositions: symbolic generalizations are not necessary thanks to their internal properties (such as meaning), but thanks to the role they play in scientific practice (during normal science);

this is not a problem about the logical form, but, rather, about the use of propositions.

Moreover, the necessity of analytic propositions depends on their internal properties and in particular on the meaning of their terms. The proposition "all bachelors are unmarried" is true because of the meaning of the terms involved, since "bachelor" and "unmarried man" mean the same thing (leaving aside Quine's remarks). But Kuhn clearly says that this is not the kind of necessity that concerns scientific laws: "but it is not – unlike the statement 'Some bachelors are married' – false by virtue of the *definition* of those terms. The meaning of force and mass are not embodiable in definitions" (Kuhn 2000: 73–74 n. 19). The truth-value of an analytic proposition is a linguistic question which depends on the meaning of the words, but this is not the case of scientific laws. On the contrary, Kuhn says that the most important function of symbolic generalizations is to define the words they deploy. Kuhn's problem is exactly the opposite of the analyticity problem. Symbolic generalizations cannot be necessary propositions in virtue of the meaning of their terms, because, rather, they are constitutive of the meaning of those terms.

Finally, nobody would classify a proposition such as "$f=ma$" within the set of analytic propositions. Just like the other symbolic generalizations, the second law of motion does not look like analytic propositions (according to the ordinary sense of analytic proposition, such as "all bachelors are unmarried" or "2 + 2 = 4" or "A = A"). Kuhn is aware of this problem and affirms again that the necessity of scientific universal laws does not depend on their form but on their use in scientific practice. While analytic propositions cannot be tested at all, symbolic generalizations can be tested to some extent. We can measure forces and masses and complete the form of the second law of motion by means of our results; in such a way we could falsify Newton's law. But laws, unlike definitions and tautologies, are often corrigible piecemeal. Again, the problem lies in the use of symbolic generalizations in the context of scientific practice.

In this section I have explained why Kuhn's description of the universal laws of science as analytic propositions is confused and unclear. He assumes that analytic and necessary propositions are equivalent, but this is controversial (take, for example, Kripke's notion of necessary a posteriori). The main difference between analytic propositions and symbolic generalizations is that analytic propositions are "necessarily" true in virtue of the meaning of their terms, while symbolic generalizations are true in virtue of the role they play in the empirical context and, unlike analytic propositions, they define the terms they deploy.

3. A grammar for scientific practice

We have seen that symbolic generalizations used by scientists seem to be necessary because, according to Kuhn, they are not falsifiable through normal experience. Moreover, once we have accepted that this necessity does not depend on their internal logical and linguistic structure (i.e. they are not analytic propositions strictly construed), we have to face two main problems: a) what is the relation between the necessity of scientific laws and their arbitrariness? b) assuming that their necessity does not depend on the logical structure, what does it mean that symbolic generalizations are necessary?

I think that the answer to these questions is the same. Moreover, I anticipate that it is summarized by Wittgenstein’s claim that: “the only correlate in language to an intrinsic necessity is an arbitrary rule. It is the only thing which one can milk out of this intrinsic necessity into a proposition” (Wittgenstein 1958: 116); from this viewpoint, arbitrariness and necessity do not contradict each other by means of the concept of “rule”. Roughly speaking, symbolic generalizations are necessary (and therefore non-falsifiable), because they are not empirical propositions, but rather, norms which regulate scientific practice: in Wittgenstein’s words, the universal laws of science constitute the grammar of science (together with their exemplary applications). Universal laws are arbitrary (or conventions) in the same sense as rules are conventions. According to this “conventionalist” position, the members of a (scientific) community deal with necessary truths in a similar way they deal with accepted rules. Both rules and necessary truths have a normative function: they are not exposed to experimental falsifications and alleged falsifications are considered mistakes of the individual scientist. Moreover, both necessary truth and rules are constitutive of human practices, social activities and institutions (including scientific communities and institutions)[26]. But I will return further below on this concept. Now I will explain the parallel between the concept of paradigm and the concept of grammar.

26 For the relation between norms and conventions see Ben-Menahem 2006.

3.1 Comparison between paradigms and grammars

As I have said in Part 1, some scholars have noticed that Kuhn's description of paradigms looks like Wittgenstein's account of grammatical propositions (Malone 1993, Glock 1996: 215, Sharrock, Read 2002: 162-163, Baltas 2004). Anyway, Kuhn himself quotes Wittgenstein when he speaks about whether Newtonian mechanics can withstand a revision of the second law of motion or the third law or Hooke's law or the law of gravity. The main point is that an answer to those questions is not required or anticipated by the structure of the lexicon and therefore they individually cannot have yes or no answers (just like it is meaningless to ask whether we could play chess without the queen).

Both Kuhn's scientific laws and Wittgenstein's grammatical rules are norms of representation, i.e. the normative and constitutive backbone which makes normal experience possible. Since they are constitutive conditions, they are not falsifiable: they do not directly describe the world, but, rather, constitute models of verification (models of reasoning, acting, experimenting and so on). They are not true or false in any ordinary sense, because such models are prior to the possibility of any truth-value attribution. On the other hand, Wittgenstein himself has said that the laws of kinematics work like norms of representation (Wittgenstein 1978: 85-87). Therefore my first point is that, according to the Kuhn-Wittgenstein model, physical laws are norms of representation: they are not descriptions of the world, but, rather, rules for describing the world. Thanks to the constitutive structure provided by scientific paradigms (universal laws and concrete examples of application), scientist are allowed to express meaningful scientific propositions which are verifiable or falsifiable through normal experience (and therefore are true-or-false in the ordinary sense); but the structure in itself, as constitutive condition, is not empirically true-or-false.

Now I will briefly compare Wittgenstein's grammatical proposition and Kuhn's paradigms point by point. They share many features and I will briefly focus on the characteristics that I have exposed referring to paradigms. At first, a grammar is in a certain sense arbitrary (see Forster 2004: 21-65): "the rules of grammar may be called 'arbitrary', if that is to mean that the aim of the grammar is nothing but that of the language" (Wittgenstein 1958: 138). That is because a) we can imagine different grammars (see for example Wittgenstein 1969: 80-81); b) grammars cannot be justified by means of their alleged agreement with the facts. To that extent, they cannot be tested by experience, but, rather, like units of measurement, they are a formal matrix for experimental testing and truth-value attributions (Wittgenstein 1974: 135). Both Kuhn and Wittgenstein compare paradigms and grammars with systems of measurement. In both cases, it is an anti-foundationalist viewpoint, which maintains the impossibility of an external standpoint to evaluate the truthlikeness of any metric system. Since the evaluation of the truth-values is an activity which requires a system of measurement (or a coordinates system), the evaluation of the truth-value of the system itself turns into an infinite regress, since we would need another metric system. Finally, the justification of a paradigm or a grammar is a pragmatic matter.

Moreover, grammatical propositions can be considered necessary propositions[27]: after all, propositions such as "the standard meter in Paris is a meter long" or "this is black" are propositions that we cannot conceive to be false. But at the same time, just like Kuhn's symbolic generalizations, they are not analytic propositions strictly construed and the reasons are approximately the same that we have seen referring to Kuhn's symbolic generalizations.

Firstly, empirical and grammatical propositions are not two different types of propositions; the same proposition can be descriptive or grammatical in different contexts and relative to its role in ordinary speaking. This is very similar to Kuhn's analysis of the dual nature of the laws of motions. The second law can be considered empirical or conventional depending on the training process in classical mechanics and, referring to scientific laws, Wittgenstein talks about the fluctuation of scientific definitions: something that today counts as an observable part of a phenomenon tomorrow may be be used to explain it; or, in other words, that sometimes the same proposition can be used as something to test by experience (empirical proposition) and sometimes as a rule of testing (norm of description) (Wittgenstein 1969: 15). The distinction between empirical and normative propositions is not a distinction between two kinds of propositions; rather, it is a distinction between two uses and it is relative to the relevant language game.

Secondly, just like symbolic generalizations define the terms they deploy, the necessity of grammatical propositions does not depend on the meaning of their components

27 The use of the word "necessity" in Wittgenstein's works is controversial as well. For an analysis of the relationship between grammar and necessity see Baker, Hacker 2005: 241-370.

because, on the contrary, grammatical propositions determine the meaning of such terms: "it is grammatical rules that determine meaning (constitute it) and so they themselves are not answerable to any meaning and to that extend are arbitrary" (Wittgenstein 1974: 184). And like, according to Kuhn, a structure change turns into a meaning change of the terms that occur in the theory (for example the meaning of the term mass changes over the course of Einstein's revolution, since the laws of motion, which define the term "mass", do not work in the theory of relativity), if we change the grammatical rules, meaning will change as well.

In the end, grammatical propositions are not analytic strictly speaking, since it is obvious that "this is black" or "this is the standard meter" are not analytic propositions and their truth-values do not depend on the meaning of words like 'black' or 'meter'. The point is that such propositions include both empirical and conventional features, for example by means of the use of ostension. Anyway, they entail a connection between pieces of world and pieces of languages which cannot be analyzed through the concept of "analyticity". Paradigms and grammatical propositions are grounded in language and reality at the same time: this is a different kind of conventionality, that I will discuss in the following sections, focusing on the relation between normativity and necessity.

But, at first, it is useful to introduce another clarification, which applies both to Kuhn and Wittgenstein. We have seen that the distinction between grammatical and descriptive propositions is not an ultimate distinction: sometimes a proposition can correspond to the former use and sometimes to the latter. But this does not mean that, like Quine, Kuhn and Wittgenstein reject the distinction between empirical propositions and necessary propositions. Although they both aim to avoid the traditional analytic-synthetic dichotomy, they think that grammatical and empirical propositions are too dissimilar to reject such a distinction. We need to distinguish between what is empirical and what is grammatical, but in order to do this in the right way, we have to take in mind that, as we have seen, we do not look for different types of proposition, but for different uses. We may well categorize grammatical and descriptive uses; the fact that some propositions sometimes behave as belonging to the former group and sometimes as belonging to the latter one does not rule out the distinction. Given a proposition, it may sometimes get treated as something to test by experience and at another time as a rule of testing; but the same proposition cannot test experience and be a rule of testing at the same time. In the same way, Kuhn does introduce his analysis of synthetic a priori propositions to distinguish between normal and revolutionary science (see Part 1). Grammatical propositions enable us to recognize scientific revolutions. In fact, while normal science allows significant meaning changes as well, scientific revolutions can be identified through the synthetic a priori criterion. A scientific revolution (a scientific change which gives rise to incommensurable scientific traditions) involves a change in the grammatical propositions. But we have seen that the meaning of the terms that symbolic generalizations/grammatical propositions deploy is determined or constituted by the grammar itself. Therefore, a change in the grammatical propositions entails also a meaning change of the terms defined by means of the relevant propositions (semantic incommensurability).

Once we have established this connection between Kuhn and Wittgenstein, now we can analyze the origin of the notion of necessity that Kuhn attributes to symbolic generalizations. My thesis is that it is the same origin of the necessity of grammatical propositions according to Wittgenstein: normativity.

4. Necessity and normativity

In the last section I have recognized a parallel between Kuhn's paradigms and Wittgenstein's grammars. They are arbitrary in a very similar sense and they are necessary in a sense which is not consistent with the identification of necessary propositions with analytic propositions. In this section I will argue that the notions of necessity and arbitrariness are consistent relative to scientific paradigms in the same way as they are consistent referring to grammars. That is because both paradigms and grammars are necessary in so far as they are normative systems of representation, which constitute the field of possible "normal" experience.

Wittgenstein states that a proposition showing a rule is different from an empirical proposition since the latter can be said "true" if it succeeds in the relevant empirical testing process: the proposition "the desk is one meter long" is empirically testable referring to the standard metric system. But it does not make sense to ask for the length of the standard meter. We are inclined to think that grammatical propositions are statements which express the real structure of the world, because it always seems to confirm propositions like "the standard meter is one meter long" or "black is darker than white"; so that some philosophers (belonging to the metaphysical or empiricist traditions about the status of the laws of nature) consider necessary propositions the universal truth about reality (although there are many different interpretations).

4.1 The “internalist” view on the concept of necessity

Roughly, both Kuhn and Wittgenstein disagree with this interpretation of the laws of nature. Kuhn says that paradigms are not the sort of things that can be true or false; the attribution of truth-values to empirical sentences is possible only by means of a matrix, which makes possible the attributions themselves, that is to say a paradigm or a system of measurement[28]. Wittgenstein solves the problem saying that grammatical propositions (as well as physical laws) do not provide a representation of the structure of the world, but rather a representation of the structure of the grammar itself. Grammatical propositions are not descriptions of reality, but norms that we use to describe reality. Thus, being norms of representation which delimit the meaningful description of the world, obviously reality seems to confirm them: the contradictory of a grammatical proposition is not falseness, but nonsense. Correctness does not mean agreement with reality but with a conventional norm or use (Wittgenstein 1978: 41).

However, although this kind of remarks should justify the necessity of rules, arbitrariness seems to creep through the back door. Wittgenstein is aware of this possible objection:

> What is necessary is determined by the rules.—We might then ask, "Was it necessary or arbitrary to give these rules?" And here we might say that a rule was arbitrary if we made it just for fun and necessary if having this particular rule were a matter of life and death. We must distinguish between a necessity in the system and a necessity of the whole system. (Wittgenstein 1976: 241)

The results of a calculus are necessary from the “internalist” viewpoint of the calculus system itself and depend on the rules of the system. We are dealing with two different kinds of necessity: *in re* necessity and grammatical necessity. These kinds of necessity should not be confused. Necessity *in re* is grounded in the essential structure of the world, while grammatical necessity in the acceptance of our conventions, norms of representation, methods of description. Kuhn, too, is aware of this distinction:

28 For the comparison between paradigms and metric systems see Kuhn 2000: 63. I will focus on this point in Part 2 – section 3, speaking about the incommensurability thesis. See for example Ben-Manhaem 2006: 265-266 for a comparison between Wittgenstein and the conventionalist tradition and the question of the correspondence between grammar and reality.

symbolic generalizations look like propositions concerning situations that could not have been otherwise (i.e. necessary propositions), but only from within a new scientific theory. This is still a question about the use of physical laws: they are necessary propositions, but only from the perspective of the "normal scientist". Again, necessity is internal to a given system of norms and depends on the adherence to a structure of "conventions". But this adherence is related to the nature of normal science and the situation may change during scientific revolutions. Thus, Kuhn rejects the concept of truth as correspondence (intended as the aim of science), saying that concepts such as 'truth' or 'proof' are meaningful only in the context of a shared scientific practice:

> The semantic conception of truth is regularly epitomized in the example: 'Snow is white' is true if and only if snow is white. To apply that conception in the comparison of two theories, one must therefore suppose that their proponents agree about technical equivalents of such matters of fact as whether snow is white. If that supposition were exclusively about objective observation of nature, it would present no insuperable problems, but it involves as well the assumption that the objective observers in question understand 'snow is white' in the same way, a matter which may not be obvious if the sentence reads 'elements combine in constant proportion by weight'. Sir Karl takes it for granted that the proponents of competing theories do share a neutral language adequate to the comparison of such observation reports. I am about to argue that they do not. If I am right, then 'truth' may, like 'proof', be a term with only intra-theoretical applications. Until this problem of a neutral observation language is resolved, confusion will only be perpetuated by those who point out (as Watkins does when responding to my closely parallel remarks about 'mistakes') that the term is regularly used as though the transfer from infra- to inter-theoretical contexts made no difference. (Kuhn 2000: 161-162)

The achievement of infra-theoretical truth or proof (the reasonable agreement between theory and data, according to the current standards accepted by the relevant scientific community) is relative to the norms of representation provided by the paradigm itself. The function of Kuhn's paradigms is not descriptive, but normative: "when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game" (Kuhn 1977: 270). They do not (directly) represent the world or the facts and cannot be evaluated through their agreement with the facts (in the sense that they do not mirror the entities that are in the world). Rather, they are norms of representation which determine a shared (by a scientific community) way of describing the facts. This normative framework is a precondition which allows scientists to work on the agreement between paradigm and reality, i.e., to select the relevant core of the theory they are working with (interpreted structures and detection properties methodologically construed); Kuhn calls this work normal science.

4.2 Methodological necessity

According to Kuhn, scientific laws have a methodological nature. Or, perhaps better, the methodological nature of scientific laws cannot be easily distinguished from their metaphysical nature. That is because the distinction between descriptive and normative propositions cannot be sharply outlined (see the example of the second law of motion). Therefore the metaphysical role of symbolic generalizations (providing a reliable representation of the world) and their methodological role (providing a shared structure for describing the world, directing the "arrow of the falsification" toward the extended core of the theory, and not toward its basic laws) are closely connected. The normative structure of scientific theories is useful because it supplies a stable basis for articulating the work of normal science and the empirical testing of the theory, without questioning the core. Physical laws play a major role in this process, because they "define" the most basic terms of the theory and therefore every modification in the theory (to explain new facts) should take into account the meaning of such terms and preserve the (interpreted) mathematical core of the theory. As we have seen in Part 1 – sections 2 and 4 – this is an epistemic or methodological question, since it depends on the parts of the theory that scientists consider empirical or testable and, as we have seen, this choice may change with time. But it is a metaphysical question as well, in so far as, referring to scientific knowledge, the questions about "the world" require methodological standards: from the viewpoint of scientific knowledge, every metaphysical problem has a methodological framework. Therefore, the normative nature of scientific laws does not imply a purely conventionalist perspective, since the empirical success of our theories depends on the world. The empirical statements articulated by means of the normative structure are true-or-false according to the ordinary tools of experience, evidence, logic and so on. But the normative structure is not directly testable, as far as it is a constitutive condition for the possibility of such empirical statements. I think that this will suffice to say that Kuhn's position is inconsistent with (CSR). This is not a kind of coherentism, since the facts play a fundamental role in the evaluation of scientific theories; rather, it is a perspective which considers physical laws as a normative-constitutive condition for the possibility of scientific practice.

4.3 Constitutive rules

Kuhn says that normal scientific practice is possible if and only if the members of some scientific community share a paradigm. From this viewpoint, it should be clear that we are not dealing with regulative rules but with constitutive rules. Kuhn has often repeated that the “constrictive” nature of paradigms is not related to explicit and mandatory rules. Moreover, paradigms do not require that scientists agree about a common conceptualization or that they share common criteria for the determination of the meanings and the referents of scientific terms, because the juxtaposition of exemplary cases will suffice. On the contrary, as the expression “synthetic a priori” suggests, Kuhn accepts the thesis that paradigms are constitutive of their experience fields. A shared paradigm provides the conditions for the possibility of scientific experience and communication. I have already argued that grammatical propositions determine the meaning of their terms. For example, the rule “the chess king moves one square at a time” is partially constitutive of what the chess king is, since it is essential to the chess king to move one square at time; it is not a question of agreement with reality or behavioral regularity.

Scientific laws are constitutive of concrete scientific practice just like the rules of chess are constitutive of playing chess: both are norms of representation and describe the language-dependent essence of their respective fields; they create constraints for experience and exclude possibilities. This interpretation fits well with Kuhn’s analysis of scientific discovery. Analyzing the difficulties related to this notion (from the metaphysical realist viewpoint), Kuhn discusses the role played by Dalton’s work in the development of chemistry. After summarizing the problems relative to the agreement between “theory” and “facts”, Kuhn concludes that from Dalton’s law (atoms can only combine one-to-one or in some other simple whole number ratio) we can infer that:

> [It did] enable him to determine the sizes and weights of elementary particles, but it also made the law of constant proportion a tautology. For Dalton, any reaction in which the ingredients did not enter in fixed proportion was *ipso facto* not a purely chemical process. A law that experiment could not have established before Dalton's work, became, once that work was accepted, a constitutive principle that no single set of chemical measurements could have upset. (Kuhn 1970: 133)

The law of definite proportions is not merely a true statement about the facts: it is a constraint, which determines what can be considered a meaningful proposition in chemistry; it fixes the boundaries of its own field. For Kuhn, this law is not the result of empirical generalizations, since scientists could not accept it on the evidence, because much of that was still negative. Rather, scientifically significant experience can take place only in virtue of the framework of the paradigm. Obviously, the experience-determination exercised by paradigms does not have to be interpreted from a constructivist perspective, but, as I have said in Part 1 – section 1, from a Kantian one. Paradigms are conditions for the possibility of experience, but they do not determine actual experience. Now, according to the Wittgensteinian model that I have outlined, we can say that the constructivist interpretation of Kuhn depends on the mistaken identification of the essence of grammar with the essence of reality: the former is language-dependent, the latter is "really there".

5. What kind of rules? Regulism and regularism

In the previous sections I have argued that, according to Kuhn, the necessity of scientific laws does not depend on their agreement with the essential structure of the world, but, rather, on their normative use in scientific practice. Symbolic generalizations are oriented on the one hand to the experimental work of scientific communities and, on the other hand, to the facts. Scientists can improve the correspondence between paradigm and data (i.e., practice normal science) only in so far as they share a paradigm. The necessity of symbolic generalizations is not world-dependent, but language-dependent.

However, a questions arises about this interpretation and I will face it in this last section. I have already outlined that, according to Kuhn, the normative view on scientific laws does not imply the explicit formulation of compulsory rules; and, at the same time, he says that symbolic generalization should be connected to concrete applications to be meaningful. The question is about how to interpret a rule.

In his discussion about normativity, Wittgenstein rejects two traditional conceptions of rules: a) "regulism" and b) "regularism" (Brandom 1994: 18-30). Regulism states that norms should be considered explicit rules or principles. Against this claim, Wittgenstein affirms that the application of a rule is not univocally determined by its explicit formulation: a rule can be satisfied by very different ranges of applications (Wittgenstein 1958: 80). Kuhn faces this problem too. We have seen that he rejects the claim that scientific practice is based on explicit rules and shared interpretations. Moreover, he notes that the problem of the application of symbolic generalizations is an everyday problem in scientific training: physics students usually face it when they try to solve the end-of-chapter exercises of their textbooks. Sometimes abstract symbolic generalization are very difficult to apply.

As we have seen in Part 1, Kuhn's answer to this problem is that physics students are not introduced to symbolic generalization in abstract terms, but through exemplary problem solutions: a paradigm includes both a theory and some exemplary applications. Exemplars are the basis of scientific practice and they are more universal than symbolic generalizations, since all physics students start learning by problems such as the inclined plane, the conical pendulum and Keplerian orbits; or with instruments such as the vernier, the calorimeter, and the Wheatstone bridge; furthermore, they are also prior to, more binding, and more complete than any set of rules abstracted from them. Thanks to exemplary solutions, physics students learn to recognize similarities between different problem situations and to use symbolic generalizations as law-sketches or law-schemes.

Just like Wittgenstein, Kuhn does not resolve this problem, but, rather, he deflates it: normativity does not have any effective foundation. A grammar regulates the practice of a community, but the "foundation" of the grammar depends on nothing but the concrete practice itself (see Wittgenstein 1958: 85); the conditions for the possibility of scientific practice are immanent to such a practice. And, for Kuhn, concrete scientific practice consists in the shared exemplary problem solutions. So that the relationship between symbolic generalizations and exemplars is circular:

> The pendulum, the inclined plane, and the rest are examples of $f = ma$, and it is being examples of $f = ma$ that makes them similar, like each other. Without having been exposed to them or some equivalents as examples of $f = ma$, students could not learn to see either the similarities between them or what it was to be a force or a mass; they could not, that is, acquire the concepts of force and mass or the meaning of the terms that name them. Kuhn (2000: 247-248)

The necessity-normativity of the relation between law and application enables us to overtake the difficulties linked to the alleged arbitrariness of the universal laws. But how can we break the circle of rules and applications? I will sketch an answer to this question referring to the second conception of rules criticized by Wittgenstein: regularism. This interpretation of norms considers rules to be behavioral regularities. Wittgenstein says that this kind of regularity cannot determine rules, since grammatical rules govern our activities in a substantial way and not by means of accidental regularities. The question is very complex and here I cannot analyze it as regards Wittgenstein[29]; I will sketch a brief solution for Kuhn's problem, that I will develop in the next section.

According to Kuhn, the foundation of the normative structure of scientific practice ultimately depends on the training process. The normativity of scientific rules is not a mere behavioral regularity since it rests on the institutionalized (i.e. also social) authority of scientific instructors and scientific textbooks: "science students accept theories on the authority of teacher and text, not because of evidence. What alternatives have they, or what competence? The applications given in texts are not there as evidence but because learning them is part of learning the paradigm at the base of current practice" (Kuhn 1970: 80). Of course this is not a metaphysical or causal foundation, but a pragmatic one. If this insight is right, it will be possible to save Kuhn's comments on the role of persuasion and pedagogy in science from a non-relativistic standpoint.

6. Conclusions

Finally, in this chapter I have discussed the relation between (TA) and (TN) in Kuhn's conception of scientific laws. I have argued that the arbitrariness of scientific laws is consistent with their necessity in so far as we interpret necessity from an infra-theoretical and normative viewpoint, that I have analyzed by means of the comparison between Kuhn's paradigm and Wittgenstein's grammar. My thesis is that, according to Kuhn, scientific laws are necessary within a given tradition of scientific research since they constitute the normative and methodological structure which enables scientists to practice normal science, i.e., to solve puzzles without questioning the basic mathematical structure of the theory. Therefore, such a structure is a methodological condition for the possibility of scientific experience in a given field. In this way, Kuhn's

29 See Williams 1999. I think that this exposition is applicable to Kuhn as well.

perspective can account from a methodological and epistemic perspective for the fact that scientific communities are committed only to some parts of scientific theories (that constitute the normative backbone of scientific practice). This position is not a kind of (CSR) because the truth-values of the empirical propositions depend on the world, but it is meaningless to ask for the truth-value of the mathematical structure, because it is a normative precondition for the possibility of truth-value attributions. This is problematic especially because it seems to entail that scientific theories are not true-or-false, literally speaking. It seems plausible that abstract mathematical structures cannot correspond to the world (even though this is what is claimed by some contemporary realist theories), but the very concept of exemplary solution (and interpreted structure) seems to provide a semantic connection between theories and reality that can justify a correspondence theory. I will deal with this question in Part 2 – sections 3-4 and Part 3. But now, I have to conclude my argument about normativity and exemplary applications.

Section 2: Dogmatism, learning and scientific practice

1. Introduction

I have concluded the last chapter sketching a solution about the normative origin of the concept of physical law as rule; according to Kuhn, this is due to the role played by exemplary solutions and applications during the training experienced by physics students. In this chapter I will analyze in depth this point, focusing on paradigms as applications and the use of exemplary cases and scientific textbooks for scientific training and learning. At the same time, I will investigate the concept of "scientific dogmatism", which is one of the most discussed topics of Kuhn's works. In fact, philosophers of science have discussed the dichotomy between criticism and dogmatism in scientific practice since the sixties. The core of the topic concerns the necessity to preserve the stability of science against "permanent (scientific) revolution" whilst at the same time, acknowledging the essential function played by doubt and criticism in scientific progress. Philosophers such as Kuhn stress the constitutive role of "normal science" in scientific practice, and affirm that criticism and doubt are appropriate only in exceptional circumstances, that he calls "crises", the prelude of revolutions. Popper and the Popperians reply that the development of science consists in the falsification of attested theories and that the suspension of doubt has negative consequences for science. The "Popper-Kuhn controversy" is recorded in (Lakatos, Musgrave 1970), where Kuhn says, for example, that "Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts" (Kuhn 1977: 6). Popper replies that, although normal science is a real phenomenon, it is also "a danger to science and, indeed to our civilization" (Popper 1970: 53).

As we can see, the question deals with the social structure of science, and the nature of disagreement within scientific communities. Within what limits can scientists doubt the methods and results of their activities? It is trivial to say that a state of permanent doubt is dangerous for science, since it causes uncertainty, which can turn into skepticism and undermine the trust in scientific institutions (both among the experts and in the public debate about science). On the contrary, it is important for scientific communities to defend the stability of knowledge against pathological doubt and skepticism, and I think Popper would have agreed on this point. However, at the same time, we must acknowledge that the critical discussion of well-confirmed theories is an indispensable tool for the development of science. Therefore, it is useful to propose a model that distinguishes between useful doubt and pathological doubt about scientific practice.

I do not want to stir up the controversy on scientific dogmatism again, even though I think it is less radical than it seems to be[30]. Nevertheless, many philosophers (including Popper) tend to discuss dogmatism as if it were a psychological or ethical attitude of the individual scientist, whereas I will approach the question from a different viewpoint. At first, I will investigate the Wittgensteinian heritage of Kuhn's concept of dogmatism, in order to clarify its function in scientific practice; and then it should be clear that both normal science and doubt are useful only from the social perspective of scientific communities and especially from the analysis of the social nature of scientific training. Secondly, I will argue that this social dogmatism accounts for the rejection of meaningless doubts, which might harm knowledge, and justify the importance of criticism for scientific progress by allowing us to understand doubt from a social standpoint.

I use a definition of dogmatism different to the common sense one and make a

30 For a recent reconstruction of the debate between Popper and Kuhn see Worrall 2003.

distinction between ordinary dogmatism and social dogmatism (the concept I want to endorse), which reflects the distinction between ordinary skepticism and organized skepticism. By organized skepticism, the sociologists of science mean that scientific theories should be tested and challenged by scientific communities. Organized skepticism, regulated by the norms of scientific method, responds to the precise demand for critical examination of knowledge: it is institutionalized skepticism (it is meaningful only from the social viewpoint of the relevant scientific community and depends on peer judgment), which disqualifies indiscriminate attacks on accepted theories. This distinction opposes the justified institutional skepticism to the personal skeptical attitude. My idea of social dogmatism is similar to this distinction. Ordinary dogmatism is the overconfident assertion of opinions and beliefs by an individual, regardless of contrary evidence and argument. On the contrary, by social dogmatism (from now on "dogmatism"), I mean blind (uncritical) adherence of a community to the "formal" system of norms and conventions, which constitutes its practice. It has nothing to do with the personal beliefs and opinions of individual scientists; rather, it deals with the self-regulation of human practices (in this case, scientific practice), since it self-imposes the standards of correctness of such practices and, in turn, the methodological criteria of organized skepticism. In the following sections, I will describe such a systems of norm, the respective adherence and its foundation.

2. Paradigms and their normative structure

In the last chapter (as in Part 1 – section 1), I have analyzed the normative structure of Kuhn's paradigms and I have introduced the concept of exemplary solution. Now, to apply this concept to scientific training and dogmatism, I shall recall the most important concepts relevant to this problem. Since paradigms are the objects of scientific dogmatism, we should clarify them in order to understand dogmatism in the right way. Thus, recall the relation between physical laws and applications. As we have seen, symbolic generalizations are universal statements, expressed in formal language or which can be easily formalized and used by the members of scientific communities without question or dissent. We can compare this element with the hard-core of Lakatos' scientific research programs. We consider them natural laws or the fundamental equation of the paradigm, such as $f = ma$ o $I = V/R$, although we can express some of them in ordinary language (for example the first and third laws of Newton's dynamics). These generalizations allow scientists to deal with scientific theories as mathematical constructions, so they justify the application of logical manipulations. However, symbolic generalizations do not specify how we should apply them to nature. In fact, we express the relationship between paradigm and nature by the most appropriated meaning of the term paradigm: exemplary case solution. As they represent the concrete part of scientific practice, exemplary problem solutions are the elements that deeply determine the social nature of scientific practice, and the peculiar agreement within scientific communities. For Kuhn, basically the empirical content of scientific theories is localized in exemplary cases.

While abstract laws have no meaning, the connection between symbolic generalization and exemplary cases constitutes the normative structure of scientific practice. If we take for granted Kuhn's rejection of the correspondence theory of truth (on which I will focus in the next parts) and the comparison between Kuhn's paradigm and Wittgenstein's grammars (that I have presented in the last chapter), it follows that paradigms are not descriptive, but normative. They do not represent the world or the facts and we cannot evaluate them through their agreement with them; rather, they are norms of representation, which determine a shared way of describing reality. As I have pointed out in the last chapter, a paradigm establishes the limits of meaningful scientific discourse, creates constraints for experience, and excludes possibilities. Kuhn's dogmatism consists in the "blind obedience" of scientists to the norms dictated by paradigms. In the following sections, I will analyze the features of this obedience.

3. Normativity, contextuality, learning

It is important to stress again that the normative power of paradigms is linked to exemplary case solutions, while we can interpret symbolic generalizations in different ways. The relationship between physical laws and exemplary cases is circular. Exemplary cases are applications of universal laws, but universal laws are empirically meaningful only if connected to exemplary cases.

> The pendulum, the inclined plane, and the rest are examples of $f = ma$, and it is being examples of $f = ma$ that makes them similar, like each other. Without having been exposed to them or some equivalents as examples of $f = ma$, students could

> not learn to see either the similarities between them or what it was to be a force or a mass; they could not, that is, acquire the concepts of force and mass or the meaning of the terms that name them (Kuhn 2000: 247-248).

Kuhn refers to the difficulties that physics students have to face when they try to solve the end-of-chapter exercises in their textbooks, and says that universal laws apply to scientific practice because students do not learn symbolic generalization in abstract terms, but by means of exemplary problem solutions. Referring to the training experienced by physics students, Kuhn tries to break the circle between laws and applications: the foundation of the normativity of paradigms is pragmatic, since it rests in scientific practice itself; it links knowledge to practice and action.

This is another common point between Kuhn and Wittgenstein. They both adopt a pragmatic approach and deflate the problem of the justification of norms saying that normativity has no metaphysical foundation. A grammar, or a paradigm, regulates the practice of a community, but the grammar has no foundation beyond its practice. According to Kuhn, concrete scientific practice is illustrated by exemplary case solutions. Kuhn enumerates some of them and affirms that almost all scientists start their education this way (for example with the inclined plane, the conical pendulum and Keplerian orbits). Thanks to these exemplary cases and to others that students face during training, they learn how to apply symbolic generalizations in new situations and problematic contexts using analogies with similar cases (as we have seen in Part 1).

Therefore, the first feature of scientific dogmatism is its contextual nature. Since symbolic generalizations are in themselves meaningless, their normative force is not independent of the actual practices of a scientific community. On the contrary, the meaning of scientific laws is contextually determined if we understand it on the horizon of a practice: rules acquire their normative content only if connected to particular practices of application (see Medina 2002: 141-194 with reference to Wittgenstein contextualism). This idea fits well with Kuhn's interpretation of the second law of motion as synthetic a priori: roughly, we can interpret the empirical content of Newton's law in different ways according to the role we want the law to play in scientific practice and to which terms we prefer to define empirically. Dogmatism makes possible such form of contextualism, which implies that the ability to apply symbolic generalizations presumes a practical context, the consensus of the scientific community. This is a consensus of action, which is possible only thanks to training, since it is what we need if we want to understand a scientific (or linguistic) practice.

The second feature of scientific dogmatism is that it is a social phenomenon grounded in the relationship of confidence between student and teacher, which allows the student to join a scientific community. For both Kuhn and Wittgenstein, the agreement in action depends on the training process that we experience to understand and apply norms. They both refer to the pragmatist tradition, which focuses on the concepts of technique and skill to understand human practices and the structure and acquisition of concepts (see for example Brandom 1994: 362 and ff). As we have seen since the beginning of this chapter, scientists use symbolic generalizations without question or dissent and employ them without allowing for alternatives. This attitude is the result of the training they receive, which, according to Kuhn, is as rigid as in orthodox theology (Kuhn 1970: 166). After all, the students can only accept what the teacher and textbook present as the truth: students do not accept theories because of evidence, but basing on the authority of teacher and text. Scientific training is authoritarian and cannot be otherwise since students lack the competence to evaluate and criticize what they learn. Consequently, the receptive attitude of the student (the blind acceptance of the authority of the teacher) is a prerequisite of the training. The process is successful if we accept the paradigm as the way we ought to do things. As will become clear, the paradigm itself partially dictates the results of an experiment and consequently, if an experiment goes wrong, this discredits only the scientist, not the theory.

Finally, as the third preliminary feature of scientific dogmatism, I shall stress once again its connection to the social conception of science, i.e., the idea that the subjects of science are and must be scientific communities and not isolated scientists. For Kuhn, history and sociology of science are not variables which influence science from outside; rather science is essentially a social and historical enterprise. This feature is strictly related to the second one, since learning is necessarily social. In his comparison between scientific and linguistic training, Kuhn himself affirms that the acquisition of a (scientific) language is part of the socialization process by which we make the scientist (or the child) part of the community and its world. Referring to the social nature of paradigms and dogmatism, Kuhn quotes Wittgenstein again, and specifically refers to his rejection of the idea of "private language": the idea of scientific theories as a private product is problematic as well as the notion of private language; in fact, both knowledge and language are impossible when conceived as something that someone can possess and exercise alone (Kuhn 1977: 148). Clearly, scientists compose scientific communities, but a scientist is only really a scientist as a fellow of his scientific community. The idea is that the public nature of the relevant paradigm pre-exists to the subjectivity of scientists and founds it. Moreover the social nature of science is emphasized also by the comparison between scientific communities and biological species. If scientific progress can be compared with Darwinian evolution, then the main characters of scientific progress are not individual scientists, but rather scientific communities (Kuhn develops this idea in his Kuhn 2000: 90-104).

Thus, in this section I have explained how scientific dogmatism is related to the structure of scientific theories and their components. My first conclusion is that scientists' dogmatic attitude towards the theories they work with consists in the acceptance of a social practice regulated by a paradigm. Thus, we should explain such an agreement within scientific communities by means of the constitutive role played by scientific training. In the following section, I focus on this pedagogical foundation of dogmatism.

4. The foundational role of scientific training

In the last section, I have said that the extraordinary agreement within scientific communities depends on the common scientific training that scientists experience as students. Kuhn provides an original interpretation of the relationship between scientific training and scientific practice (Warwick, Kaiser 2005)[31]. First he notes that the normative power of paradigms does not rest upon explicit, coercive and inviolable rules: sometimes we can abstract explicit rules by scientific practice, but normal science does not necessarily require an interpretation and rationalization of paradigms. Kuhn refers to Polanyi's tacit knowledge and Wittgenstein's family resemblance and I have focused on the latter. In fact, I have just said that, according to Kuhn, the practice of normal science involves the mastery of similarity relationships, which allow the scientist to apply the paradigm-model in new problem situations. Those similarities and regularities in application (often not expressed in explicit propositional form) provide the space to practice normal science, a space in which the actions and reactions of scientists agree.

31 For other discussions of Kuhn's pedagogy of science see Andersen 2000b and Barnes 1982: 16-40.

The training process entails that the teacher shapes the student's reactions, creating a common ground of agreement that we never question except in non-normal circumstances. The acquisition of concepts (as networks of similarities) is normative since the mastery of correct applications (which requires a "must") is constitutive of the concept itself.

Kuhn's most extended discussion of these matters is from everyday experience of language learning. Recall that he considers a child, Johnny, who learns to distinguish different kinds of birds (ducks, geese, and swans) under the guidance of his father, during a walk. The father (who plays the role of the authority and supervisor of the correct usage in his community) uses ostension, and names the birds at which he points. When the child tries to do the same and identify the birds, the father validates or rejects the identification. Thanks to the guidance of his father, and after a certain number of correct identifications, we can say that Johnny is competent in the identification of birds, ducks, geese, and swans, and that his instruction is successful. After the training, Johnny applies these labels to nature, but he does not use anything like definitions or correspondence rules; the child simply employs perception of similarities and differences. Kuhn's theory of the elaboration and acquisition of concepts is pragmatic, which means that the mastery of an empirical concept entails the correct use of the concept within the appropriate linguistic community (Hoyningen-Huene 1993: 110).

Therefore, we can constitute the conceptual structure that scientists share by similarity-difference classes associated to respective concepts without explicit definitions and without necessary and sufficient conditions of identification. They are family resemblances, and any scientist can legitimately use different criteria to identify a class. In order to share a language, the members of a scientific community need not share definitions or criteria of identification and application. What commensurable languages must preserve is only the structure of similarities and differences, which, as we have seen in Part 1 (especially section 4), Kuhn calls "taxonomic or lexical structure". Scientific training allows scientists to enter a scientific community whose practice is regulated by norms implicit in the lexical structure they acquire as students, and which "mirrors" aspects of the world it describes (and limits the phenomena described by the same lexicon). I have faced this point in Part 1 – section 1 – so I will not tell you anymore about that. Anyway, this observation leads to a second feature of scientific training that relates to the role played by exemplary problem solutions in the acquisition of the paradigm.

In fact, together with the absence of explicit rules, there is a second important aspect of the relationship between scientific training and scientific practice. Kuhn draws attention to the difficulties faced by physics students to apply the physical laws presented in their textbooks (whose meaning they believed they had grasped perfectly) to solve the relative end-of-chapter exercises. Grasping the meaning of physical laws requires not only reflection on the structure of the laws themselves, but also the use of canonical exemplary solutions, which the scientific community considers "paradigmatic". A consequence of this is that we should reverse the relationship between exercises and laws. The normativity of paradigms lies in their exemplary nature. Therefore we do not use examples just to illustrate whether the student understands the lesson and the meaning of the terms that recur in physical laws; rather, examples generate the meaning of the same laws. Understanding is not a matter of adequate mental representations, it is the ability to use pre-existing solutions and examples to find a solution to new problems by means of new applications and articulations of the old terms. We do not define Newtonian concepts such as "force" or "mass" by the laws of motion, but by the experimental situation associated with such laws (for example the inclined plane).

A consequence of this approach is that learning by means of examples is important not only to create common patterns of perception and action within scientific communities, but also to institute the connection between scientific language and reality. Training can (pragmatically) "found" normal science because it teaches students how to do things with language; once again, as I have pointed out in Part 1 – sections 3-4 – Kuhn follows Wittgenstein and emphasizes that the meaning of scientific terms consists in their use in scientific practice. We use to learn the words that constitute a lexical structure in use, which implies that we acquire knowledge of language and knowledge of the world together. Basically, just like in language games where there are inextricably linked linguistic and non-linguistic features, scientific training in paradigms is a nature-language learning, in relation to which, Kuhn explicitly speaks of learning language and nature together (as in the analysis of the metaphor of the coinage with two faces that I have analyzed in Part 1 – section 1).

In the next parts I am going to analyze two features of scientific training (ostension and the use of textbooks); but, actually, let my summarize the role of learning in the light of what I have said right now. This summary is based on (Williams 1999: 214-215) about Wittgenstein's language learning theory:

- Scientific training allows physics students to adhere to a social practice, characterized by a set of normative regularities, although we do not express these regularities by explicit and coercive norms.
- Training requires a context whose background consents to the norms of the paradigm to be meaningful. The qualified teacher (the representative of the authority of the relevant scientific community) provides this context: he approves or invalidates the behavior of the students.
- Just like every normative practice (a practice which asks for norms, standards, rules), scientific practice is necessarily social. We cannot consider a solitary man who does not follow a paradigm a scientist, or, as Kuhn says, the results of his activity are something less than science.
- The use of scientific concepts presupposes the mastery of their relative techniques and skills, but we cannot formalize such techniques and skills in a set of propositional norms, definitions, and rules of correspondence. The ultimate foundation of paradigms is pragmatic; it rests on scientific practice itself.
- The general agreement pertaining to scientific communities (the fact that scientists do not usually question the basic elements of their discipline) originates from their adherence to the common patterns of behavior and perception acquired during training. Scientific dogmatism is grounded in the grammatical structure of paradigms.

4.1 Ostension and ostensive learning

There is no doubt that ostension and pointing to concrete objects and problem situations is an important part of scientific training. Both Kuhn and Wittgenstein emphasize the role of ostension in the acquisition of a new (scientific) language; however, this idea requires clarification. Kuhn says that the exposition to examples of ostension is indispensable to understanding some scientific terms by direct application. It is part of the previously outlined process of contemporary acquisition of knowledge of language and nature. The objects involved in ostensive learning are not language-independent: just because we capture them by scientific practice, they have become, in Wittgenstein's words, "part of the symbolism" or "samples". In other words, this means that the objects of ostensive learning begin playing a normative role to fix the meaning of some terms in their respective language games. Clearly, this does not imply that ostension can fix the meaning of a word or generate a standard for future applications, or that ostensive definitions are adequate descriptions of the meaning of scientific terms. For Wittgenstein, ostensive teaching (Wittgenstein 1958: 4-5) plays the function of ostensive definition. That is to say, the part of the training connected to the practice and context in which we embody the expression, helps the student to understand the use of a word and to establish a connection between language and things[32].

Kuhn also acknowledges that ostensive definitions are not enough to fix the meaning of the words and distinguishes between "ostension" and "ostensive". The former implies that we need nothing but the exhibit of a word's referent to define it; the latter that some exhibit is required during the learning process, but that this is not sufficient to learn and define the relevant words (Kuhn 2000: 13). Kuhn's emphasis on ostension depends on his concept of learning through examples. He wants to reaffirm that we do not learn scientific language regardless of the concrete use, and that ostensive learning is an important part, although only a part, of scientific training. While the use of everyday words such as "swan", "duck" and "goose" can be misleading and induce the idea that the meaning of these terms is established by means of the ostensive act, the use of scientific terms immediately clarifies the question. As I have said referring to the meaning of scientific terms (see Part 1 – sections 3-4), Kuhn provides us with the example of the needle of a galvanometer; we can point to it by affirming that the specific cause of its deflection is, for instance, "electric charge", but this provides us with the relevant information to apply "electric charge" only in this situation. While it does not specify how to apply it to other sorts of events to which "electric charge" can or may refer unambiguously, for example a thunderstorm.

Finally, referring to complex scientific terms, such as "electric charge", it is evident that Kuhn does not support the existence of ostensive definitions, but rather wants to stress the role of ostensive learning in the determination of the network of similarities and differences which constitutes the structure of scientific lexicon.

32 For a distinction between ostensive definition and ostensive teaching see Williams 1999: 21.

4.2 The authority of scientific textbooks

In parallel with the relationship between teacher and students, Kuhn often notices that scientific textbooks represent the social authority of scientific communities in the training process. From the first page of *The Structure of Scientific Revolutions*, he says that the most common image of science derives from textbooks and from their pedagogical and persuasive power (Kuhn 1970: 1). Just as in ostensive learning, scientific textbooks constitute evidence for the authoritative nature of the training, since readers of scientific textbooks endorse the theories there presented on the authority of the author, as a member of the relevant scientific community, not because they have experimentally tested such theories. The experimental evidence presented in science texts almost has a pedagogical function, that is to say, they are exemplary solutions that enable an adequate understanding of the physical laws and their practical application. He refers to them as parts of a "context of pedagogy", different to both the context of discovery and the context of justification.

The context of pedagogy represented by science texts corresponds to the anti-historical and dogmatic attitude that Kuhn sees in scientific training. He points out that the most singular feature of scientific training is that we introduce science students to their respective discipline only through textbooks, while other students are encouraged to read the classics in their fields. In contrast to other disciplines, the difference in alternative textbooks is mainly for technical and pedagogical details, but all display the same approach to their problem-fields. This is because, in order to develop its characteristic dogmatism, scientific pedagogy voluntarily refuses a historical approach to its matter, for example the historical approach that characterizes disciplines such as philosophy or arts: obviously, this is not intended as a criticism of scientific learning: science would probably not be possible without such ideas. The question is to distinguish between the context of pedagogy and the history of science (and the contexts of discovery and justification) [33].

33 Auguste Comte had already noticed that the chronological order of scientific discoveries does not coincide with the actual organization of knowledge. Communicating and teaching the achievements of science require a certain reconstruction which produces a new order of the arguments and their mutual relationship ("the dogmatic order") different from the orders of discovery and justification ("the historical order"). Moreover on this matter, Gaston Bachelard has emphasized the role of textbooks for scientific pedagogy and focused especially on their normative and social function. For a comparison between Comte, Bachelard and Kuhn on scientific pedagogy and textbooks see García-Belmar, Bertomeu-Sánchez, Bensaude-Vincent 2005: 219-222. Additionally, Ludwik Fleck studied the authoritative and dogmatic nature of scientific learning and related it to the use of textbooks,

The reference to the typical organization of scientific knowledge in science texts according to the order of pedagogy highlights another aspect of scientific dogmatism: textbooks represent the product of the institutionalized scientific practice, i.e. a social self-authenticating practice that “justifies” the normativity of paradigms. I have already discussed the concept of self-autentication and normativity referring to paradigms in the last chapter. Now the main point is the institutional structure of science, that is to say its social organization through training, textbooks, scientific communities and so on: it is a precondition for the organization of meaningful scientific discourse. This is the nature of the paradigm, it creates and constrains the possibility of scientific practice. What is important for dogmatism is not the acceptance of particular beliefs, but the adherence to the “formal” normative structure of the paradigm. We can only consider what we learn in certain ways and from certain books approved by scientific communities to be scientific knowledge.

5. Dogmatism: a new place for doubt and critique

So, in the end, dogmatism is not the scientists’ psychological and ethical attitude towards the theories they work on, or the unjustified conviction in certain specified beliefs. It might be this way only if the paradigm were a conceptual scheme or a system of propositions we believe to be true, but I have argued that paradigms have nothing to do with the personal beliefs of scientists. On the contrary, paradigms have no descriptive nature, but rather a normative one: they are networks of rules for the production and organization of scientific knowledge. Dogmatism does not refer to a system of beliefs, but to a system of norms, not to the specific content of knowledge but to the way scientific communities authenticate, organize, and transmit scientific knowledge. Although the way we organize knowledge inevitably influences the possible content of such knowledge (and so a distinction between formal and material aspects of knowledge is not satisfactory), paradigms are the formal matrix of our knowledge or a matrix for the construction of knowledge.

This is clear from referring to the interpretation of scientific changes. Let me concede for a moment, for the sake of the argument, the hypothesis that dogmatism is a psychological attitude of the individual scientist (or, as Popper says, a dangerous lack of

---

intended as the main instrument of that “indoctrination” (See Cederbaum 1983: 195-196).

critical approach). This hypothesis does not explain correctly, for example, the distinction between normal and revolutionary change in the history of science. In fact, one should not take the difference between normal and revolutionary science too literally and think that the characteristics of the former are completely opposite to the ones of the latter[34]. Normal science is not a totally crystallized practice and it allows transformations and adjustments, which are sometimes substantial (see Part 1 – section 4). According to the psychological-ethical-individualist interpretation of dogmatism (the scientist irrationally clings to his ideas and beliefs), we can interpret every change as revolutionary, since it requires a suspension of the dogmatic attitude. Kuhn tries to elaborate a more complex theory of revolutionary change by means of the distinction between the empirical features of scientific theories (the paradigm broadly speaking, as system of beliefs) and the normative features (the paradigm strictly speaking, as system of norms). A revolutionary change involves the normative backbone of scientific practice in depth, it is a substitution for the rules of the game.

From the outset, we can see that the role of scientific dogmatism is linked to the idea that some sections of scientific theories (synthetic a priori, detection properties or concrete structures) behave as constitutive (and at least partially implicit) rules of scientific practice. These rules allow scientists to produce empirical propositions, open to criticism, doubt and empirical falsification, whereas, dogmatism deals with the blind adherence to the rules of scientific practice. Of course, as I have already said, the distinction between empirical and normative propositions is not so sharp: it is not grounded in the empirical reality. Just like Wittgenstein, Kuhn does not distinguish two different kinds of proposition, but two different uses. The same proposition can be in certain circumstances an empirical proposition that we can test by experience and, in other circumstances, a normative proposition that we use as rule of testing; but it cannot be at the same time an empirical and normative proposition

Therefore, I do not intend the agreement within scientific communities to be conventionalist or relativist. It does not mean that what is true (or false) is the result of the conventional decision of the specialists and from that moment forward, we consider the result of such decisions unquestionable; just like in Wittgenstein's famous statement, that is "not an agreement in opinion, but in forms of life"[35] (Wittgenstein 1958: 88) or "a

34 Kuhn admits that, in *The Structure of Scientific Revolutions,* he had overly emphasized the normal-revolutionary science distinction and that if he were rewriting his book he would focus less on such a distinction (Kuhn 2000: 57).

35 In order to avoid the conventionalist problems related to the word "agreement", Cavell proposes to

consensus of action" (Wittgenstein 1976: 183-184). This is a plausible interpretation as far as this reference to the agreement in action as an agreement in forms of life explains also a passage of *The Structure of Scientific Revolutions,* which could be misunderstood. Kuhn writes that the choice between competing paradigms concerns incompatible modes of social life (Kuhn 1970: 94). This idea could look like a relativist affirmation about the incomparability of scientific theories, together with an underestimation (or an exaltation) of the intolerance towards different ideas. Instead he says just that different paradigms correspond to different models of social action and, in the end, to different forms of life, but this does not involve considerations about relativism or intolerance. The paradigms do not determine the truth, but the way scientists critically evaluate, discuss, test and challenge truths. Finally, Kuhn's dogmatism reveals a similarity with Wittgenstein's conception of certainty, where accepting a proposition as certain means using it as a grammatical rule (and this is undoubtedly related to the role played by those propositions in the learning process). An important point in this comparison is that both dogmatism and certainty are preconditions for meaningful doubts. In fact, when Kuhn enumerates the advantages of scientific dogmatism, along with the elimination of skepticism and pointless doubts, he says that scientists can recognize the failures and the problems of their theory only by referring to the background provided by the paradigm:

> The practitioners of mature sciences know with considerable precision what sort of result he should gain from his research. As a consequence, he is in a particularly favorable position to recognize when a research problem has gone astray. […] The practice of normal puzzle-solving science can and inevitably does lead to the isolation and recognition of anomaly. That recognition proves, I think, prerequisite for almost all discoveries of new sorts of phenomena and for all fundamental innovations in scientific theory (Kuhn 1963: 364-365).

This is what Kuhn calls the "essential tension" in scientific research: scientific progress needs divergent and convergent thought, dogmatism and criticism, but we can understand the combined presence of these elements only from a social standpoint that acknowledges the centrality of scientific communities in the explanation of scientific development. Both dogmatism and criticism are meaningful only as social phenomena.

---

translate Wittgenstein's term *Übereinstimmung* with the word "attunement" and not with the traditional "agreement". That is because "the idea of agreement here is not that of coming to or arriving at an agreement on a given occasion, but of being in agreement throughout, being in harmony, like pitches or tones, or clocks, or weighing scales, or columns of figures" (Cavell 1978: 32). Cavell and Kuhn worked together at the University of Berkley and Cavell's interpretation of Wittgenstein could have been an important influence on Kuhn (see Kindi 2010).

Consequently, dogmatism leaves space for criticism, except when it is necessary to avoid ceaseless scientific revolutions and theory changes that threaten scientific progress. It only safeguards the normative backbone of scientific theories from skepticism, saving this structure from the possibility of empirical falsification. Normally it involves only a few interrelated terms and laws, such as “mass”, “force”, “weight”, the laws of motion etc., in Newton’s physics. Except for this backbone, scientific assertions produced and organized through it, are subject to criticism, doubt, and rational discussion by means of the classical tools of experience, logic, evidence, persuasion and so on. These tools help scientists to determine what is true (and what is false), although obviously any theory choice involves deliberative and fallible features. The paradigms deal with the determination of what can or cannot be empirically true-or-false, that is to say, they are normative preconditions for the formation of meaningful scientific statements.

6. Conclusion

Finally, in this chapter I have discussed how the normative nature of Kuhn's paradigms is related to the process of scientific learning by exemplary problem solutions and how this contributes to the generation of the typical scientific dogmatism. I have argued that, in a scientific context, the distinction between meaningful doubt (which is positive for scientific progress) and pathological doubt (which turns into skepticism) is clear only from a social viewpoint about the nature of science and the organization of scientific communities. Social dogmatism and organized skepticism are complementary concepts. On the one hand, organized skepticism guarantees the safety of scientific knowledge from skeptical and iconoclastic attacks, since it states that we should regulate scientific doubt institutionally according to methods, criteria, and procedures established at the level of communities and subject to peer judgment. On the other hand, social dogmatism fixes the accepted methods, criteria and the procedures to practice science, and in turn, to exercise doubt and critical thinking. I have stressed again that such methods, criteria, and procedures are “formal concepts”: they do not deal with the content of scientific knowledge, but with the organization and production of scientific knowledge. They do not influence the truth, but the way scientists critically evaluate, discuss, test and challenge truths. In a scientific context, both dogmatism and skepticism are, at the same time, both dangerous and necessary. This does not mean that scientists should be simultaneously dogmatic and skeptical; rather, it means that certainty and non-pathological doubts emerge at the institutional level of scientific communities. According to Kuhn's conclusion, this entails that paradigms are not true-or-false in any ordinary or classic sense (correspondence to the facts). In the next sections of my work I will discuss this claim in depth, starting with Kuhn's arguments against truth (incommensurability thesis and comparison objection) and proceeding with other objections against correspondence truth.

Incommensurability, Truth, Historicism

1. Introduction

In the last chapters I have discussed a perspective on the meaning and the reference of scientific terms based on some ideas defended by Kuhn. But, in his view, this theory entails that scientific theories are not true strictly speaking and, in the next chapters, I am going to criticize this point. I will start with the incommensurability thesis, which is Kuhn's most basic argument for the linkage between meaning of scientific terms and rejection of the correspondence theory. In this section I will not discuss the reception and the different interpretations of the incommensurability thesis, since I think that the discussion has often been out of focus. Rather, I will follow the most recent works about incommensurability, taking for granted that the incommensurability thesis does not entail incomparability or radical untranslatability. The main thesis of this chapter is that the concept of incommensurability is strongly related to the rejection of the correspondence theory of truth and the historicist epistemology. Roughly, incommensurability has three different meanings (see Buzzoni 1986: 111 and Hoyningen-Huene, Sankey 2001b: ix):

1. Methodological Incommensurability: each scientific theory defines its own methods, standards, aims and criteria.
2. Semantic Incommensurability: the meaning of scientific terms changes during the history of science.

3. Ontological Incommensurability: the world changes over the course of scientific revolutions.

Since (3) is a very complex concept which involves many problems such as the relation between theory and observation, the correspondence theory of truth, scientific realism and the concept of reference (and many others), I will focus on (1) and (2). I will argue for the following theses:

1. Both methodological (1.1) and semantic (1.2) incommensurability are closely related to the rejection of the correspondence theory of truth. They do not refer to incomparability and untranslatability in general. Rather, methodological incommensurability means that we cannot compare scientific theories to find out their respective truthlikeness. And semantic incommensurability means that we cannot translate scientific theories keeping unchanged their truth-value relations.
2. The concept of incommensurability depends on Kuhn's historicist epistemology, i.e. the idea that we can study scientific knowledge only from the viewpoint of the dynamic of scientific theories: a historical viewpoint interested in the relations between successive theories. From this perspective, each paradigm has a dual-directionality: from the one hand it looks outside to the world, from the other hand it looks back to the scientific tradition that produced it. The relation between paradigm and reality is not a one-to-one relation: it always involves other paradigms[36].
3. Methodological incommensurability depends on semantic incommensurability. The change in the methodological standards, values and aims accepted by scientific communities is a consequence of the semantic change in the lexical structure of the theory. Or, perhaps better, this is a consequence of the language-learning process experienced by physicists during their student years.

Therefore, at first I will present methodological incommensurability and explain the

36 In my view, historicism has nothing to do with the idea that each scientific paradigm is a historical entity determined by (and relative to) the relevant historical and social conditions. I think that this thesis (a kind of historicist relativism) is false, even as interpretation of Kuhn's ideas (as I have claimed in Part 2 – section 1).

relation between methodological incommensurability, truth and historicism. Then, I will analyze the connection between methodological and semantic incommensurability and I will conclude with the relation between semantic incommensurability and truth.

2. Does methodological incommensurability entail incomparability?

In this section I will briefly introduce methodological incommensurability and explain the difference between methodological incommensurability and incomparability. Here I will focus on the following idea: the proponents of rival paradigms do not agree about methods, standards and aims of science. According to this thesis there are no shared, objective methodological rules or neutral scientific standards for theory comparison and choice; and that is because each paradigm determines its own standards of rational evaluation. Incommensurability is due to the lack of external standards, which do not depend on the paradigms themselves and can reduce theory choice to a neutral and mechanical algorithm. In sum, two paradigms are incommensurable from a methodological viewpoint because: a) they focus on different problem fields; b) they disagree on the priority to be given to these problems in the context of their research program; c) they define in different ways the most basic problems, which reflect the pragmatic, the research strategies and the specific interests of the same paradigm (see Doppelt 1983: 121).

Many scholars have interpreted this claim as something like radical incomparability between rival scientific theories (Lakatos 1970: 179 n. 1, Newton-Smith 1981: 9-10, Putnam 1981: 118, Scheffler 1967: 16-17, Shapere 1966: 67-68). Methodological incommensurability has been regarded as a kind of epistemological relativism about theory comparison: if theories are incommensurable (or, according to this interpretation, incomparable), scientific changes are irrational, since they cannot be explained by means of rational procedures and scientific revolutions would be mere “conversions”. But such an interpretation has been strongly refuted by Kuhn himself: he explicitly says that incommensurability does not imply incomparability (see also Bernstein 1983: 82 and Hoyningen-Huene 1993: 218-221). In fact, he recalls that incommensurability is a mathematical term, which expresses the relation between the hypotenuse of an isosceles right triangle and its side (or the circumference of a circle and its radius); it means that there is no unit of length contained without residue an integral number of times in each member of the pair. But, obviously, this does not mean that we cannot compare them, since incommensurable magnitudes can be compared to any required degree of approximation. Replying to his critics, Kuhn affirms that his aim was not to make theory choice irrational. Rather, he meant that, although theory choice is rational in general, it is not "algorithmic" and regulated by only one scientific method. The evaluation of scientific theories is necessarily a practical process, which involves decisional, deliberative and subjective elements; and logic and experience are not able to force theory choice.

To replace the scientific standards-based model for theory comparison, in the seventies Kuhn has provided a value-based model (Kuhn 1977: 320-339). He lists several values used by scientific communities: a) accuracy (of the factual statements, both from a quantitative and qualitative viewpoint); b) consistency (absence of internal contradictions); c) scope (the domain of possible applications); d) simplicity (the ability to unify apparently different groups of phenomena); e) fruitfulness (the ability to predict and to apply to new phenomena). Scientists do not consider these values rules which determine theory-choice, but rather values, which influence it; moreover they can be interpreted in different ways and, in some situations, they can conflict with one another.

Without going into the details of Kuhn's theory of scientific method (Nola, Sankey 2000b: 26-30), we are probably dealing with a reason which forced Kuhn, in his latest works, to separate the incommensurability from the problem of scientific method. In fact, as Siegel noted, this argument for incommensurability involves only a theory of value-based theory choice (Siegel 1987: 57). Bird, too, says that, in the version of semantic incommensurability defended in his latest work, the relativism-absolutism dichotomy about theory comparison is simply not being asked (Bird 2000: 240-241). At first sight, it seems that Kuhn merely gives up the problem of methodological incommensurability and relegates incommensurability to his semantic aspect. Kuhn himself seems to confirm this interpretation where he says that the differences in methodological standards are a consequence of the language learning process (Kuhn 2000: 34 fn. 2). Kuhn makes methodological incommensurability dependent on semantic incommensurability. But this assertion does not imply that methodological incommensurability is deflated; rather, we have to look for the foundation of this kind of incommensurability in his semantic dimension. For this, I will divide Kuhn's thesis of methodological incommensurability in two sub-theses:

- *There is no scientific method which constraints theory choice and assures its correctness: theory choice is a deliberative process*. This thesis does not necessarily imply relativistic consequences or incommensurability and has been defended by anti-relativist philosophers like Popper as well (Popper 1959: 61).
- *Incommensurability does not mean incomparability*: *we can compare the accuracy, fruitfulness, scope, consistency, simplicity of scientific theories. But we cannot compare them to discover which theory is closer to the truth.* While the first sub-thesis has been abandoned by Kuhn, the second one constitutes the linkage between methodological and semantic incommensurability and has been defended by Kuhn throughout all his works. I will discuss it in the next section.

3. Truth and methodological incommensurability

Discussing some objections concerning epistemological relativism, Kuhn himself relates methodological incommensurability to his rejection of the correspondence theory of truth[37]. Referring to the above analysis of the role played by proof in theory choice, he compares mathematical proof with truth, since they both assume inter-theoretical applications, where incommensurability comes into play. Proof and truth are meaningful concepts only within a shared practical context, which constitutes the basis of the agreement between scientists. But, when we try to extend the use of terms like 'proof' and 'truth' beyond the infra-theoretical context, Kuhn affirms that we should be more cautious. Incommensurability blocks the possibility of any neutral comparison between scientific theories. This statement does not mean that paradigms are incomparable, because we can always compare their accuracy, consistency and so on; instead, paradigms are incomparable relative to the evaluation of their respective truthlikeness. In his evolutionary account of the development of science, truth has no place. Incommensurability, at least, in its methodological sense, does not involve relativism about the rationality of theory choice, but rather it is a form of relativism about truth. Kuhn has always countered the charge of irrationalism, but, about truth, he says that he we can consider him a relativist (Kuhn 2000: 160). Thus, methodological incommensurability does not imply that all theories are equally good, but that all

37 For Kuhn's objections to truth as correspondence, see Bird 2000: 209-266 and Kuukkanen 2007.

theories are equally close to the truth. Kuhn returns more explicitly on this argument in his latest works: the evaluation of theory change is now embedded in the evolutionary dimension of scientific knowledge. This evolutionary account does not try to explain the rationality of our beliefs, but, rather, theory change in itself; this is in opposition to the non-evolutionary viewpoint, which aims to evaluate isolated scientific theories, in order to calculate their degree of truth or probability.

Kuhn insists that truth and proof are not inter-theoretical; a theory cannot be tested by means of any direct match between theory and reality. Moreover, scientific values are meaningless in so far as they do not belong to concrete practice; in such a context the application of scientific values is more fruitful, although it cannot eliminate disagreement once and for all. The main advantage of the evolutionary perspective is that, while a clash between two rival theories is conceivable and can be productive in an evolutionary perspective, a direct clash between theory and reality, in a classic perspective, is just not an option. Theory evaluation is a historical process which can only be realized by a comparative viewpoint; and, as Kuhn says, incommensurability is an essential component of any historical, developmental, or evolutionary view of scientific knowledge.

4. Evolutionary historicism

In the previous section I have argued that two theories can be incommensurable from a methodological viewpoint because they self-impose their standards of correctness and the evaluation of the truth-values of their statements is not a direct clash between theory and reality. We can know which is the best scientific theory, but we cannot know which is the truest theory. Since theory-choice consists in a comparison between two (or more) theories (not between a theory and the world), theory-choice is, in turn, a historical question, from an evolutionary viewpoint. According to my interpretation, a connection between methodological incommensurability, truth and history of science is emerging. This connection is clarified in *The Structure of Scientific Revolutions*, where Kuhn introduces methodological incommensurability by means of a passage about his historical and evolutionary conception of science:

> *Paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them*. They are the source of the

methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely “unscientific”. Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before. (Italics mine) (Kuhn 1970: 103)

Here Kuhn describes the change in scientific standards, problem fields, and scientific aims during scientific revolutions. But, after summarizing the features of methodological incommensurability, he associates it with a remark about the historical structure of paradigms: they are directed not only to nature but also back upon the science that produced them. According to Kuhn, paradigms have a dual-directionality. From the one hand they look at the world and, from the other hand, at their historical tradition. This assertion summarizes Kuhn’s historicism. He does not mean that each scientific paradigm is relative to the historical and social context in which it develops; rather the historical structure of paradigms is inextricably linked to their implicit knowledge.

In fact, as we have just seen, theory-choice is not a direct match, since a direct contact between theories and reality cannot exist. However, Kuhn does not merely affirm that the comparison between paradigm and nature is influenced by the paradigm itself. If this were the case, Kuhn would only say that observation is theory-laden, which is a point accepted by nearly all philosophers of science. Instead Kuhn’s claim is more radical. He states not only that the relationship between paradigm and nature is mediated by the paradigm, but that it is mediated by the relationship between the current and the past paradigms as well. Accordingly, the relation between successive paradigm is incommensurability, and especially semantic incommensurability, since each paradigm inherits his lexicon by the paradigm that preceded it. Roughly, incommensurability influences the connection between paradigm and nature and, if successive theories are incommensurable, we cannot determine which one is closer to truth. To sum up, the historical nature of paradigms (their constitutive relation with the paradigms which produced them) plays a fundamental role in the determination of the relationship between paradigm and the world, which, consequently, cannot be a direct clash, but only a comparative evaluation between two theories. That is because the historical relation between successive paradigms is incommensurability, which blocks the possibility of the evaluation of the pure truthlikeness of a theory. Incommensurability, truth and historicism give rise to a circle.

Before analyzing in depth the relation between semantic and methodological incommensurability, let me conclude the discussion of Kuhn's historicism.

5. The standard meter

I have concluded the last section saying that, for Kuhn, incommensurability and truth are historical concepts. More precisely, the fact that incommensurability is a historical concept does not mean that it is a concept gathered from the analysis of the history of science. Kuhn tells us that incommensurability is the result of his activity as a historian of science (Kuhn 2000: 16-17). The historian experiences incommensurability when he is studying an ancient scientific text and notices seemingly nonsensical passages. While many researchers have considered these passages to be mere mistakes, Kuhn believes that they results from the incommensurability between rival paradigms. Kuhn means that there is no external Archimedean standpoint to see the history of science as a cumulative process.

The connection between this kind of historiography and the alethic relativist conception of methodological incommensurability is remarked by Kuhn: even though he was concerned with the problems of rationality and relativism, what is fundamentally in question is the correspondence theory of truth. As we have seen referring to truth and proof, the concept of an external Archimedean viewpoint on the history of science assumes inter-theoretical applications as well. But, again, scientific theories cannot be evaluated in isolation, since only the change in the beliefs can be justified, while all individual theories are equally far from the truth:

> On the developmental view, scientific knowledge claims are necessarily evaluated from a moving, historically situated, Archimedean platform. What requires evaluation cannot be an individual proposition embodying a knowledge claim in isolation: embracing a new knowledge claim typically requires adjustment of other beliefs as well. Nor is it the entire body of knowledge claims that would result if that proposition were accepted. Rather, what's to be evaluated is the desirability of a particular change-of-belief, a change which would alter the existing body of knowledge claims so as to incorporate, with minimum disruption, the new claim as

> well. Judgments of this sort are necessarily comparative: which of two bodies of knowledge-the original or the proposed alternative-is better for doing whatever it is that scientists do. (Kuhn 2000: 95-96)

In other words, there are only provisional, historical situated pseudo-Archimedean viewpoints, constituted by the very consensus within the relevant community: it is not fixed, but, rather, it moves with time and changes with community and sub-community. To that extent, the traditional non-evolutionary perspective fails because, according to Kuhn, it claims that only neutral languages and observation can serve as the judges of the truthlikeness of scientific theories; or as the Archimedean platform for theory-choice. On the contrary, Kuhn's point is that each evaluation is relative to a scientific community and its shared lexicon.

Thus, just like proof, truth can be only an infra-theoretical and historical concept: truth is not correspondence with a mind-independent world, but only the result of a rational evaluative process. What results from a successful theory comparison is internal to the historical situation which enables it: merely, the problem of the truth or falsehood (as correspondence) is not the question being asked. In the end, referring to the lack of an Archimedean standpoint: “only a fixed, rigid Archimedean platform could supply a base from which to *measure* the distance between current belief and true belief. In the absence of that platform, it's hard to imagine what such a *measurement* would be, what the phrase 'closer to the truth' can mean” (italics mine) (Kuhn 2000: 115).

In the last passage I have stressed the words “measure” and “measurement” because they are strictly related to the incommensurability thesis. As Kuhn has repeated several times, incommensurability is a mathematical term which means “no common measure”. But outside of its original context, it is a metaphor: “no common measure” becomes “no common language”; in other words, two theories are incommensurable if there is no language into which both theories, linguistically construed, can be expressed without loss. But the measure-metaphor does not stop here. As well as denouncing the absence of a common measure to explain inter-theoretical relations, Kuhn does compare paradigms with units of measurements or to metric or coordinate systems.

A metric system is a condition for the possibility of truth-value attributions in the relevant domain and, as I have said in the last sections, here Kuhn refers to Wittgenstein’s discussion about the standard meter[38]. Kuhn’s description of paradigms is

38 For a discussion of Wittgenstein’s standard meter see (Baker and Hacker 2005: 189-199).

very similar to this analysis: truth, proof and justification are meaningful only in an infra-theoretical context, while it is impossible to evaluate the truthlikeness of a paradigm. Each paradigm is a system of measurement which enables theory evaluation and justification by means of shared scientific values such as accuracy, consistency and so on. Thanks to these values, we can compare the results of rival theories relative to their respective methods, standards, aims: but such a meter is not an absolute Archimedean platform, but the relevant historical situation. So, in the end, according to Kuhn, there is no meta-metric system to evaluate the absolute truthlikeness of a theory. Kuhn calls this system Archimedean platform, common measure, neutral observational language, truth, the world-in-itself. All these non-evolutionary concepts assumes that we can compare theories from a non-historical standpoint: a direct clash between theories and reality, that Kuhn considers pointless.

6. The route to semantic incommensurability

In the previous sections I have individuated two theses:

1) The rejection of the correspondence theory of truth: truth is an infra-theoretical concept which depends on accepted standards, values and aims of scientific practice.
2) Historicism: paradigms are not oriented (only) to the world, but to the historical tradition that produced them: they have a dual-directionality.

I have discussed the relation between these theses and the thesis of methodological incommensurability, concluding that, according to this model, both truth and incommensurability are historical concepts, since they depend on the evolutionary epistemology: theory choice is relative to the dynamic of scientific theories, i.e. to concrete scientific practice and its relation with past paradigms. I have showed that:

1.1) from a methodological viewpoint, incommensurability means that we cannot evaluate the "absolute" truthlikeness of scientific theories;

2) that is because, from the evolutionary viewpoint, the relation between theory and

nature is not a one-to-one relation: it involves the relation between successive theories.

But the relation between successive theories is a semantic relation: each theory inherits its lexicon by the relevant scientific tradition. For example, the theory of relativity adopts the term ‘mass’, but the meaning of such a term changed during the transition from classical mechanics to Einstein’s theory. Therefore, before focusing on semantic incommensurability, I will focus on thesis (3): methodological incommensurability is a consequence of semantic incommensurability.

In fact Kuhn, in his latest works, leaves aside the methodological thesis of incommensurability to defend its semantic implications. We have seen that the discussion about the justification of belief change can be meaningful only from an evolutionary perspective which does not aim to overstep the historical situation. Kuhn's point is that only a neutral lexicon, in which the statements of rival theories can be expressed and compared, could constitute a direct access to the facts and a tool for inter-theoretical evaluation. The transition from methodological to semantic incommensurability is due to Kuhn’s analysis of the origin of the agreement within scientific communities about paradigms. As we have seen in the last section, it is the constitutive role played by scientific learning. The applicability of scientific values in theory choice takes for granted a shared perspective, made possible by scientific training (Kuhn 2000: 34 fn. 2. See also Kuhn 2000: 60 fn. 4). Since scientific learning is prior to methodological rules, it moves the foundation of methodological incommensurability to the semantic question of the meaning of scientific terms (their use in concrete scientific practice).

Anyway, this thesis does not solve the main consequence of methodological incommensurability: since a direct clash between a theory and reality is impossible, all theories are equally close to the truth. Thus, basically, this is the main argument for the connection between incommensurability (and meaning theory) and rejection of correspondence truth: from the one hand, semantic incommensurability implies structural non-homogeneity between rival theories; on the other hand, we cannot directly compare theories and facts and therefore we cannot deal with this non-homogeneity from a logical or empirical viewpoint. This argument rests on the so-called “comparison objection” (we cannot compare theories and facts) and I think that it is misguided. But I will reject it in the next chapter; at first, let me proceed with semantic incommensurability.

7. Does semantic incommensurability entail untranslatability?

In the previous section, I have argued that methodological incommensurability is a consequence of semantic incommensurability as well as the different trainings experienced by physics students (thesis 3). Now, I will introduce semantic incommensurability and briefly explain the difference between semantic incommensurability and radical untranslatability.

Kuhn claims that scientific revolutions entail a meaning change in the fundamental terms employed by the theories. For example, Newtonian mass is conserved while Einsteinian mass is convertible with energy; At the same time, he links the question of the determination of the experimental field of the theory (its reference) to the difficulties faced by scientists who try to translate rival scientific theories. Since a neutral language which allows us to express the empirical statements of the theories is not available, the problem is that we cannot find out whether rival theories affirm or deny the same content. In the received view, semantic incommensurability is a kind of conceptual-scheme relativism which entails untranslatability and the irrationality of scientific revolutions. The impossibility to fully translate the lexicon of a theory is not only a persuasive argument against rational theory choice, but also against mutual understanding between human beings. For example, for Putnam

> The incommensurability thesis is the thesis that terms used in another culture, say, the term 'temperature' as used by a seventeenth-century scientist, cannot be equated in meaning or reference with any terms or expressions we possess. As Kuhn puts it, scientists with different paradigms inhabit 'different worlds'. […] The rejoinder this time is that if this 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. In short, if Feyerabend (and Kuhn at his most incommensurable) were right, then members of other cultures, including seventeenth-century scientists, would be conceptualizable by us only as animals producing responses to stimuli (including noises that curiously resemble English or Italian). To tell us that Galileo had 'incommensurable' notions and then to go on to describe them at length is totally incoherent. (Putnam 1981: 114-115)

This objection does not sound good for many reasons. I will briefly summarize two clarifications to reply to this kind of critics[39]. At first, Putnam's argument takes for granted that untranslatability concerns language as a whole. On the contrary, Kuhn argues that semantic incommensurability is always a local event, i.e. it involves only specific "pieces" of language and not language as a whole (Kuhn 2000: 36). As we have seen in Part 1, incommensurability concerns small groups of inter-defined terms, which are usually introduced by the fundamental equations of the theory. For example, Newtonian 'force' and 'mass' are untranslatable into the Aristotelian lexicon because they are acquired together by means of the second law of motion, which does not work in Aristotle's physics.

Moreover, in his argument, Putnam affirms that we cannot understand untranslatable languages. His premise is that "we could not translate other languages" and his conclusion is that "we cannot interpret organisms' noises at all". This inference assumes that interpreting a language means translating the words and sentences of such a language into the respective words and sentences of another language. On the contrary, Kuhn rejects the identification of translation with interpretation[40]. Translation strictly construed consists in the systematic substitution of a set of words and sentences for the respective words and sentences of another language; therefore, a translator should understand both languages. Instead, interpretation is similar to Quine's radical translation, i.e. a situation where the object-language is totally unknown: the radical translator is not a translator, but rather an interpreter, who learns a new language. The interpreter can succeed in understanding the unknown language and fail in technically translating it at the same time. Ordinary translation is always possible, but it necessarily implies compromises, difficulties, meaning shifts, neologisms and so on. Thus, translation strictly construed is impossible, while, *pace* Putnam, interpretation can always be achieved.

Therefore, in the next section, I will argue that, although untranslatability does not entail conceptual-scheme relativism, it involves a kind of relativism about truth (thesis 1.2). This kind of relativism is consistent with Kuhn's rejection of epistemological and conceptual relativism. Semantic incommensurability does not rule out translation in itself, but a technical translation which keeps unchanged truth-value attributions across successive theories.

39 For the translational rejection of incommensurability see Sankey 1994: 102-137.

40 For the relevance of this distinction in the latest developments of Kuhn's incommensurability thesis see Chen 1997.

8. Truth and semantic incommensurability

In this section I will explain thesis (1.2): semantic incommensurability depends on the rejection of the correspondence theory of truth. It means that, although we can translate scientific theories, the preservation of the internal truth-value relationships is problematic.

In fact, while many philosophers argue that semantic incommensurability implies conceptual relativism, I think that incommensurability is deeply linked to Kuhn's skepticism about truth and especially to the impossibility to preserve truth-value relationships across translation. Inter-theoretical translation entails losses and modifications in the truth-values of some statements of the old theory. Kuhn is very clear on this problem: the impossibility to translate (strictly speaking) involves many problems about the preservation of the truth-values: "it is a quasi-mechanical activity governed in full by a manual which specifies, as a function of context, which string in one language may, *salva veritate*, be substituted for a given string in the other" (Kuhn 2000: 60).

Thus, untranslatability refers to the perfect preservation of the relationships between the most basic terms of the symbolic generalizations of the respective theories. In such a sense, translation implies compromises and meaning changes; and these changes provide truth-value alterations:

> The preservation of truth values when translating scientific prose is very nearly as delicate a task as the preservation of resonance and emotional tone in the translation of literature. Neither can be fully achieved; even responsible approximation requires the greatest tact and taste. In the scientific case, these generalizations apply, not only to passages that make explicit use of theory, but also and more significantly to those their authors took to be merely descriptive. (Kuhn 2000: 62)

In Newtonian mechanics, terms like "force", "mass" (and so on) are acquired together through the second law of motion and the experimental situations associated with it. The introduction of such terms, for example, in Einstein's mechanics entails many problems, since Newton's second law of motion does not work in this context. Consequently, the translation (strictly construed) from the Newtonian lexicon into the theory of relativity causes internal (language-dependent) contradictions in the truth-value relationship linked, for example, to the different meanings of the term 'mass':

“enriching the Newtonian conceptual vocabulary with Aristotelian terms (or vice versa) would build contradictions about observable phenomena *into language itself*” (Kuhn 1999: 36). Recall Kuhn's objections against the causal theory of reference (Part 1 – section 3). He does not focus on the alleged referential continuity, but, rather, he is interested in the consequences of the introduction of new information about water in the context of our old lexicon. The only news about chemistry that we can infer from the identification of H_2O with XYZ is that something is badly wrong with our chemical theory.

As we have seen, referential change (which is essential from Putnam’s viewpoint) is, according to Kuhn, a by-product of the influence exercised by the structure of scientific lexicons. Such a structure blocks the expression of both ‘H_2O’ and ‘XYZ’ (and the relevant knowledge embodied in these expressions) into the same language without a substantial revision of our beliefs in this field. And that is because the introduction of ‘XYZ’ in a lexicon in which we have ‘H_2O’ and rival descriptions and applications implies essential errors in the truth-values of the propositions that involve such terms (for example the evaporation temperature of water).

And, as we have seen in Part 1 – section 1 – the origin of this puzzle is Kuhn’s theory of the structure of and the access to the possible worlds of science: the access to certain experience fields may exclude the access to other fields. In front of many anomalies, breaking down the limits of a lexicon implies a change in the meaning of its terms and some modifications in the truth-values. This is another Wittgensteinian theme: a change in the lexical structure might provide major meaning changes, just like a modification in the grammar implies a meaning change (Wittgenstein 1974: 184).

Recall the comparison between paradigm and standard meter. Kuhn says that the analysis of the truth-value of an empirical proposition is necessarily an activity internal to a lexicon. But, for Kuhn, this implies that paradigms are neither true nor false. These remarks clarify the kind of alethic relativism associated with semantic incommensurability. Kuhn does not mean that a proposition, which is true in a given paradigm, may be false in another context; this relativism is not based on context-dependent truth-value attributions[41]. Rather, some propositions, which are true-or-false in a lexicon, are not candidate for truth or falsehood in another one (see Wang 2002). This argument has been proposed by Hacking (1982: 49), in his attack to conceptual

41 This is a discussed position in the contemporary debate about alethic relativism. See for example Kölbel 2002 and MacFarlane 2003.

relativism. In his latest works, Kuhn accepts Hacking's suggestion and affirms that the evaluation of an empirical statement consists in two phases. At first we have to determine the status of this statement: is it a candidate for truth-value attribution? The answer to this question depends on the structure of the lexicon we are using. Only once we have established the status of the statement we can understand if it is "true": this second process involves experience, logic and evidence.

The point is that scientific revolutions imply the revision of the most basic propositions of the respective scientific theories (for example, Newton's laws of motion). According to my previous interpretation, a change in these propositions entails the meaning change of the terms employed (for example 'mass', 'force' and 'acceleration') and then the difficulties to attribute truth-values.

9. Conclusions

I this chapter, I have analyzed Kuhn's incommensurability thesis (methodological and semantic), discussing its relation with the rejection of the correspondence theory of truth and the historicist epistemology. Criticizing the received view on incommensurability, I have argued that methodological incommensurability does not mean incomparability and that semantic incommensurability does not mean untranslatability. Rather, I have argued for three theses:

1) The incommensurability thesis is related to the rejection of the correspondence theory of truth. It claims that there is no meta-theoretical Archimedean platform to evaluate and translate scientific theories:

> 1.1) from a methodological viewpoint, it means that, although we can compare scientific theories, we cannot know their absolute truthlikeness; because, since theory evaluation depends on accepted standards and rules, truth is an infra-theoretical concept.
>
> 1.2) from a semantic viewpoint the translation of scientific theories should preserve the truth-value relations, but, since each theory defines its own terms, a change in the physical laws provides a meaning change such that some propositions are meaningless.

2) The thesis of incommensurability is related to Kuhn's historicism, i.e. the idea that we cannot evaluate isolated scientific theories, but only theory-change. The relation between theory and world is not a one-to-one or direct relation, since it always involves other theories. Specifically, it involves the historical relation between the actual paradigm and the tradition that produced it.

3) Methodological incommensurability is a consequence of semantic incommensurability. That is because the historical relation between successive paradigms is a semantic relation which deals with the different meanings of the same term within rival scientific paradigms. Ultimately, incommensurability depends on the different trainings experienced by physics students introduced to rival paradigms.

But the main controversial point is that the connection between truth and incommensurability is based on the idea that the correspondence theory means that we should be able to compare theories and facts and this is pointless (comparison objection, since we can only compare rival theories from a dynamic and historicist viewpoint). In the next chapter I will focus on this argument to reject it, while in Part 3 (section 2) I will try to save semantic incommensurability.

Section 4: The Comparison Objection to the Correspondence Theory of Truth

1. Introduction

In the last chapter I have presented Kuhn's incommensurability thesis and how it is related to the rejection of the correspondence theory of truth. I have concluded that his argument ultimately depends on the comparison objection, a traditional argument against correspondence truth. Since in the last part I will be concerned with whether the perspective that I have just defended entails some skeptical conclusions about correspondence truth (as Kuhn thinks), I will take seriously this objection. Thus, the aim of this chapter is to discuss the comparison argument and especially Kuhn's version of the argument. Roughly, the comparison objection states that the correspondence theory should be rejected because it entails that we should be able to compare beliefs and reality; and this is pointless because we do not have independent access to one of the things to compare (the world). Kuhn's argument is more complex because it challenges the very idea that scientific progress is a process tending toward a fixed goal, namely, the truth or the "objective reality". Thus, his argument is divided in two parts: the comparison objection to the correspondence theory and a historical argument against scientific progress as ontological convergence (like the confutation of convergent realism or pessimistic meta-induction). I will start showing that, although pessimistic meta-induction is an important problem concerning scientific realism, the argument fails due to the comparison objection (whatever idea one may have about pessimistic meta-induction and scientific realism). So, I will remain agnostic with respect to the historical argument and I will face the epistemological question. After spelling out exactly the steps of the argument, I will reject the assumption that, as far as scientific knowledge is concerned, the correspondence theory of truth implies some kind of comparison between the entities postulated by scientific theories and the "real entities", or the objective world. I will argue that the empirical testing of scientific theories has not just to do with such "ontological" worries. Therefore I will conclude that, while the argument is untenable from an empirical viewpoint, it could make sense from an ontological viewpoint, but it would not threaten the correspondence theory of truth.

2. Kuhn's argument against the correspondence theory

In *The Structure of Scientific Revolution* Kuhn adopts a neutralist attitude toward the relation between truth and science: he does not argue against the correspondence theory. Merely, he says that science can do without truth and, in the famous last chapter, outlines scientific progress by means of the evolutionary analogy: we have to substitute evolution from what we do know for evolution to what we wish to know. But, since the "Postscript-1969" he has formulated an argument against the applicability of the correspondence theory to scientific knowledge:

> A scientific theory is usually felt to be better than its predecessor not only in the sense that it is a better instrument for discovering and solving puzzles, but also because it is somehow a better representation of what nature is really like. […] Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its real counterpart in nature now seems to me illusive in

> principle. Besides, as an historian, I am impressed of the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. (Kuhn 1970: 206)

In the proceeding of this chapter, I will analyze, discuss and reject this argument, since, as we have seen in the last chapter, it is the main assumption of the incommensurability thesis.

2.1 Clarification about truth and scientific realism

Before discussing the argument, there is an important point to make. This argument is quite confused, since it deals both with the relation between science and truth (comparison objection) and with the question of scientific realism (pessimistic meta-induction). But truth and realism are different issues. It has been said that there is a philosophical relation between the correspondence theory and metaphysical realism. For example, Davidson equates the correspondence theory of truth with transcendental realism (Davidson 1990: 308-309), relying on the idea that they both assume truth and knowledge to be non-epistemic. Clearly, "being non-epistemic" seems to be an essential property of scientific truth, but it is not an exclusive property of the correspondence theory: for instance, deflationism and alethic pluralism are non-epistemic as well (Wright 1992: 34 and Horwich 1990: 104-105). The correspondence schema (roughly, a proposition is true if and only if it corresponds to the facts or propositions are true or false and facts make propositions true) requires that a given fact (or state of affairs and so on) obtains and it can obtain in several different ways, even if it is mind-dependent. For example an idealist philosopher may argue that the world consists of her mental states and that truth means correspondence between truth-bearers and her mental states (the facts). Here, truth is non-epistemic, but the facts are mind-dependent (see Kirkham 1992: 133-134 for some examples of non-realist correspondence theories)[42]. Therefore, although some philosophers affirms that correspondence truth and realist commitment cannot be separated, actually every theory of truth is consistent with any metaphysical thesis about the existence (and the essence) of facts. Assuming Tarski's definition of truth, we would conclude that the semantic theory means that the truth property is

42 See also Devitt 1984: 42 and Vision 2004: 14-16 for the distinction between truth and scientific realism.

metaphysically neutral: “we may accept the semantic conception of truth without giving up any epistemological attitude we may have had; we may remain naive realists, critical realists or idealists, empiricists or metaphysicians – whatever we were before. The semantic conception is completely neutral toward all these issues” (Tarski 1944: 362). Hence, I will discuss the correspondence theory regardless of any question about scientific realism. Admittedly, there is some relation between them, but, as I will discuss further below, there is no direct conflict between “realism” and “antirealism” here. Rather, the point that needs to be underlined is about two different kinds of theories of truth: 1) truth deals with the correct representation of the mind-independent world; 2) truth can be defined independently of notions such as “representation”, “reference”, “denotation” (realistically construed) (see Lynch 2009: 36). Consequently, of course, this is not thought to deny that Kuhn is, to some extent, concerned with scientific realism. Perhaps Kuhn’s rejection of the correspondence theory depends on his rejection of metaphysical realism, not the other way round (Bird 2000: 237). But, leaving aside Kuhn’s position in the scientific realism debate, the objection addresses the correspondence theory.

2.2 Comparison objection and pessimistic meta-induction

Coming back to the main issue, in his analysis of the argument, Hoyningen-Huene notes that it can be divided into two sub-arguments: a historical argument (pessimistic meta-induction) and an epistemological one (comparison objection) (Hoyningen-Huene 1993: 262-264. See also Kuukkanen 2007). Although my discussion focuses on the epistemological argument, I will start with the historical one, since it introduces some premises of the comparison argument. Pessimistic induction does not question the correspondence (it questions scientific realism); therefore, let me show to what extent it is relevant to my discussion and to what extent it is not relevant.

The historical argument aims to prove that no evidence is available to demonstrate that the history of science is an “ontologically convergent” process. This argument is similar to Laudan’s confutation of convergent realism (Laudan 1981) and aims to show that there is no connection between the empirical success of science and the increasing closeness to the truth of its ontological claims. According to Laudan, convergent realism is a theory that links scientific realism to the correspondence theory of truth (and reference) in order to clarify scientific progress in terms of improving approximation to the truth. It is based on four premises: 1) mature scientific theories are approximately true and the newer theories are *truer* than the older ones; 2) the terms used by mature scientific theories usually refer; 3) the older theories are limiting cases of the newer ones; 4) the more recent theories should explain the empirical success of their predecessors. If we accept these premises, we will conclude that mature scientific theories should be successful and that their success constitutes evidence for scientific realism, thanks to the realist notion of reference and the relation between truth (and increasing verisimilitude) and successful reference. But pessimistic induction replies that this argument does not sound good, because many mature (and successful) scientific theories were not referring. For example, we are quite sure that dephlogisticated air does not exist, but the phlogiston theory explained many phenomena in the chemical field and therefore the connection between truth and success seems to be undermined.

From this argument, Laudan and many others (including Kuhn) conclude that, since several past theories were non-referring, there is no evidence for thinking that our best theories are currently succeeding in individuating the real entities. Each theory has its own ontology and here the comparison objection comes in: comparing the ontology of our best theories to the real world does not make sense, because we do not have independent access to the objective world. Convergent realism rests on the relation between reference, truth and realism. Arguably, Kuhn would have rejected all the premises of convergent realism, but the main point is (2), i.e., the concept of reference.

(1) depends on (2) because truth is defined by means of the concept of reference, since the terms deployed by true scientific sentences should refer[43]. For example, Tarski's theory of truth is usually supposed to provide this kind of connection. Given a language L, a reference scheme for L is a function which associates one (or no) object or a range of values with any predicate or variable of L. Since reference-in-L is a mapping from names and predicates to denotations and extensions, given a domain of discourse, reference-in-L is one of the reference schemes for L. Additionally, truth-in-L assigns to each well formed sentence of L a truth-value. For Tarski, reference-in-L determines truth-in-L: when we know the definition of reference for L, we can define truth for L. The reference scheme connects any sentence of L to its truth-value (Leeds

43 This does not mean that, on the contrary, every proposition which includes referential terms is true. We can formulate false propositions by using referentially successful terms.

1978).

Moreover, (3) and (4) depend on (2) as well, because, without a realist theory of reference, one may use the concepts of reduction and explanation without any realist commitment, for example from an instrumentalist standpoint. The relation between successive theories can be logically explicated and justified, but this is not sufficient to vindicate scientific realism, unless we interpret scientific theories in a realist way, by means of a definite concept of reference. This thesis holds that the referential function of scientific languages is able to capture some essential properties of the objects with increasing verisimilitude, although sometimes our beliefs about such objects are wrong. And that referential success can be preserved across theory-change. Therefore the main problem is about reference.

Here I do not wish to talk about the confutation of convergent realism in depth. I would just like to clarify that the fact that truth and reference are interdefinable notions does not mean that the semantic theory of truth implies some particular theory of reference, for example a realist theory of reference (or that the semantic theory is necessarily a kind of correspondence theory of truth). For example, the deflationary theory states that "being true" and "reference" work in the same way. Just like the schema "*p* is true if and only if *p*" is exhaustive of the notion of truth, the schema "*a* refers to *a*" is exhaustive of the notion of reference (see Båve 2009) and this does not have any metaphysical implication. This is controversial as well, but what I mean is that one can accept the truth schema (following the correspondentist or deflationary or any other interpretation) without committing herself to a specific theory of reference. And this does not jeopardize the correspondence theory. I will return to this question in the last section, but at first I shall illustrate the connection between reference and comparison objection.

In fact, as I have pointed out in Part 1 – sections 3-4 – Kuhn endorses a non-realist theory of reference. According to his theory, reference is a function of a lexical structure, which, in turn, determines the meaning of the terms of the structure (mass, force and acceleration in classical mechanics; or element and compound in chemistry). In such a way the connection between scientific language and reality is not a neutral or realist connection, but, rather, a function which associates lexical structures with experimental problem situations. Leaving aside the consequences for scientific realism, according to Kuhn, this entails that the entities posited by scientific theories have no independent "ontological nature", since reference does not provide us with reliable evidence about the essence of the objects that are "really there", in the objective world. This is questionable, but, as I have just said, does not deal with the nature of truth. I have stated that the theory of reference we endorse does not necessitate anything concerning our theory of truth, although truth and reference (and meaning) are inter-defined concepts. Thus, since Kuhn's argument is against the correspondence theory of truth, he has to introduce the comparison objection to extend his question about reference to cover the problem of truth (as correspondence). Basically, the language-dependence of reference is not a threat to scientific truth (although it may be a problem for scientific realism); on the contrary, it concerns truth in so far as the non-independent access to scientific reality (for the language-dependence of reference) is used as an argument against the correspondence theory of truth. And that is the comparison objection.

3. The comparison objection

In the last section I have affirmed that the first premise of Kuhn's argument against the correspondence theory is taken from his historical argument (pessimistic induction): the idea that reference is language-dependent and that it cannot be considered a proof of the ontologically convergent nature of the history of science. While I have not taken a stand on pessimistic meta-induction[44], I have stated that a non-realist theory of reference is not a problem for the correspondence theory (and has nothing to do with the theories of truth). By contrast, it may be a question for scientific truth in so far as you can connect it to the epistemological argument that I have sketched at the beginning of the chapter: the comparison objection, which states that (since reference and ontology are language-dependent and we cannot have direct access to the world) we cannot compare our beliefs with reality to check their degree of verisimilitude and therefore the idea of truth as correspondence does not make sense. But, before discussing this argument, I have to make another clarification.

44 For a discussion of pessimistic meta-induction and its consequences for scientific realism see, among the others, Hardin and Rosenberg 1982, Psillos 1999, Chakravartty 2007 and the debate about selective realism.

3.1 Why an epistemic argument?

The claim that truth is non-epistemic is controversial. Of course, there are some venerable epistemic theories of truth, such as coherence or pragmatism. But, even if the non-epistemic nature of truth is not unquestioned, the comparison objection challenges the correspondence theory and it is definitely a non-epistemic theory. This is one of the advantages of such a theory with regard to scientific knowledge, since it states that propositions are true or false and facts make propositions true. And this seems to be exactly the case of scientific knowledge: science consists of empirical propositions, which are verifiable or falsifiable by means of experiments. The correspondence theory affirms that there is no philosophical relation between truth and justification (see Alston 1996 and Devitt 1997). On this perspective, truth depends on the world and the relevant states of affairs; it does not deal with the rationality or the justification of our beliefs and therefore no epistemic condition is included in the correspondence theory. All champions of the correspondence theory stress the non-epistemic nature of truth and therefore the comparison objection has a weak point: it is an epistemic argument. At first sight, it does not attack essence of truth, but our methods of verification (the very idea of comparison). It rejects the correspondence theory in virtue of the fact that we cannot verify the truth-value of scientific propositions independently of our system of belief (since we do need our system of beliefs to refer to these states of affairs). But the comparison is the method by which we check the truth-values of propositions, not the truth in itself. One can reply that method of verification and truth are very different things and that many propositions are certainly true-or-false, although actually we cannot verify whether they are true or false. For example the sentence "all swans are white" is true if and only if all swans are white or, according to the correspondence theory, it is true if it corresponds to the fact that all swans are white. As a matter of fact, this proposition cannot be verified (because it is an unrestricted universal statement), but this does not rule out the correspondence theory as regards this kind of proposition.

All these observations are right and I agree with the distinction between truth and justification. But, as regards the application of the comparison objection to scientific knowledge the question is not so clear. The relation between truth and science necessarily includes methodological considerations. A true scientific proposition is not merely true; rather it is a proposition considered true according to the current and shared standards and methodology of scientific inquiry. Otherwise, it would not be a scientific proposition, but, rather, an ordinary proposition or a proposition from other domains of discourse. Therefore, perhaps the comparison objection does not apply to the correspondence theory of truth, but it applies to the correspondence theory of "scientific truth", since a proposition may be true or false, but (whatever is its truth-value, if any) it is not a scientific proposition if it does not rest on some accepted methodological standards. Therefore, I think that the discussion of the comparison objection is relevant to scientific knowledge (a connection between scientific truth and methodology in Hacking 1979). Moreover, in the next sections, I will further explain that the comparison objection does not reject the correspondence theory because it does not provide a criterion of verification, but, rather, because it entails skepticism. But, at first, let me spell the argument out in more details.

3.2 Analysis of the argument

This argument was first proposed by Kant (Vanzo 2010) and is very popular in the pragmatist tradition (Dewey, Goodman, Rorty, Putnam, Davidson and many others) (McDermid 1998). Roughly, the comparison objection states that, if the correspondence theory were right, we should be able to compare our beliefs (or propositions, or sentences) with the facts to find out whether they agree with the relevant states of affairs or facts. But such (direct) comparison is not possible and therefore it follows that, if we take the correspondence theory for granted, we will not be able to explain knowledge. Thus, if we aim to save knowledge against skepticism, we have to reject the correspondence theory. The argument goes as follows:

1. The most basic claim of the correspondence theory of truth is that *p*[45] is true if and only if it corresponds or agrees with a given "piece" of reality, usually objects, facts, states of affairs or events. You know that *p* is true when you know if it corresponds to that section of reality.
2. If you want to know if *p* is true, you have to compare it with its portion of reality, i.e., you have to check if the facts are exactly how *p* states in that

45 By *p*, I mean a truth-bearer, the kind of things that can be true or false (propositions, beliefs, statements, speech acts, sentences and so on).

section of reality. If the result of the comparison is positive, then *p* is true. Otherwise it is false.

3. But, if you want to compare *p* with its section of reality, you must have some independent knowledge of both the things to compare. Basically, we know *p* with good approximation, since it is the belief we are testing and we have direct access to it. Comparing *p* with its specific state of affairs means knowing if it is confirmed by a certain piece of reality. But how can we have independent access to such piece of reality?
4. We cannot have any independent or direct access to the world. That is for many reasons, for example that our knowledge is fallible or that reference is language-dependent. Consequently, we do not compare *p* with the facts, but, rather, with other "linguistic" or "conceptual" entities, such as judgments or other beliefs and, in turn, such entities should be justified by means of their agreement with reality.
5. From this argument, we conclude that, if truth means correspondence between truth-bearers and reality, truth is a useless concept, since we will not be able to distinguish between true propositions and false propositions. The consequence of the acceptance of the correspondence theory is radical skepticism about knowledge.

At first, note that, from the conclusion of the argument, it follows that the correspondence theory should be rejected because it entails skepticism (there is no effective difference between true and false propositions); therefore, the main point of the comparison objection is not the conflation of truth and justification (see the counter-objection about the non-epistemic nature of truth), but rather, the skeptical results of the adoption of the correspondence theory. This is another reason why I think that the comparison objection is relevant to scientific truth and cannot be addressed by a reaffirmation of the distinction between truth and method.

But, of course, there are many possible options in front of us. Replying to the skeptical challenge, the philosophers who propose the argument usually reject (1) and state that there must be another concept of truth (different from the correspondence theory), which can explain knowledge and defend it against skepticism. They claim that the reason of the skeptical conclusion (that they reject) is the theory of truth we hold. Therefore, for example, we may adopt a pragmatist theory of truth. If we do so, the skeptical conclusion is ruled out, since the pragmatist theory allows us to distinguish true propositions from false propositions. Since it does this (while the correspondence theory does not), many philosophers (such as Rorty, Putnam…) charge the correspondence theory with our skeptical puzzle, and insist that we should reject the correspondence theory and embrace another theory.

But this is not the only possible solution: one can draw different conclusions from this argument. At first, you can accept the argument as well as the skeptical conclusion: all the steps are correct and we cannot find out the truth-values of propositions. This is a good conclusion for all the skeptic philosophers, since they welcome the assumption that in principle human knowledge cannot provide a criterion for identifying true beliefs. Moreover you can reject (4) and say that there are some experimental data or privileged sensory presentations that are available for direct access and knowledge is grounded in these basic experiences. The comparison objection takes for granted the rejection of the "myth of the given", i.e. the idea that the mind is basically a passive receiver and that it mirrors the objects of knowledge without any distortion. But a follower of the "myth of the given" (even in a minimal form) may disagree with (4). Additionally, one can reject (3), for example from a non-realist viewpoint. According to my previous clarification about the relation between correspondence truth and scientific realism (every theory of truth is consistent with any metaphysical thesis about the nature of the world), one can connect the correspondence theory to an idealist metaphysics: the world consists of mental states. Therefore, the problem of the independent access to an unconceptualized reality does not affect such a theory. The comparison does not involve, from the one hand, a linguistic entity (truth-bearer) and, from the other hand, the "real world", but rather, two conceptual entities: truth-bearers and mental states. These options are interesting, but I will not discuss them. That is because I think that skepticism and idealism are not viable choices for scientific knowledge and that the philosophers of science have sufficiently argued against the "myth of the given" (although I do deny that it is still a controversial point). But, before presenting my counter-objection to the comparison argument, I shall say that there is another way of rejecting the argument and that I agree with it. Bird (Bird 2000: 227) argues against the comparison objection by means of an example. Consider a lock and a key and that I succeed in using the key to open the lock; I know that key works because there is a match between the key and the levers of the lock, even if I do not have independent access to the levers of the lock. And, in the same way, the idea of a match between scientific theories and facts makes sense even if I do not have independent access to the relevant scientific facts. Bird rejects the step (3) of the comparison objection and I am sympathetic with this argument. But, although I agree in the substance, my argument differs in two respects from this one: a) it has a more narrow domain of application; b) it is more focused on the correspondence theory. According to (a) Bird's argument applies to knowledge and facts in general, while my argument is limited to scientific knowledge. According to (b), Bird's argument addresses the concept of truth by means of the concept of knowledge: I can know that the facts exist (and play a causal role), even if I cannot directly know them. On the contrary, my argument focus on the correspondence theory straightaway.

So, I will argue against (2): the idea that the correspondence theory requires some kind of comparison.

3.3 Correspondence truth and the concept of "comparison"

In the previous section I have summarized the comparison objection and I have stated that, in my view, the most intriguing premise is (2): roughly, if you want to know if *p* is true (according to the correspondence theory), you have to compare *p* with the relevant section of reality. I do not agree with this claim and I will try to show that truth (especially scientific truth) bears no relation to such kind of comparison. But the idea of comparing a theory and its "real" counterpart is basically the core of Kuhn's argument against the correspondence theory. Therefore, let me come back to the argument.

In fact, on the light of these clarifications, I think that Kuhn's argument is now clear. Since "there is no theory-independent way to reconstruct phrases like 'really there'" (the second premise of pessimistic induction, i.e., reference is language-dependent) and "the notion of a match between the ontology of a theory and its real counterpart in nature is illusive" (the second premise of the comparison objection), therefore the correspondence theory cannot account for the development of science. Since I have stated that the language-dependence of reference is not a problem for truth in principle, the core of the objection is the second premise of the comparison objection. Kuhn affirms that it is pointless to talk about a match between scientific propositions and a purely object-sided reality. That is because this match requires that they are both accessible independently of one another; but if we could have independent (or direct) access to reality, we would not need to elaborate hypotheses about reality and compare them with reality.

Therefore, in order to reject this argument, I will focus on the idea of the match between the ontology of the theory and its "real" counterpart. For example, the comparison objection says that Einstein's concept of space is more similar to Aristotle's space than to Newton's absolute space and therefore it is not clear what it means that the theory of relativity is closer to the truth than classical mechanics. Since, according to the correspondence theory (and the argument from empirical success to truth), both Newton's and Einstein's theories should "correspond" to some aspects of the world, it seems that the correspondence theory cannot account for scientific progress. The idea of a comparison between theory and reality does not make sense here, because we are dealing with very different models and, if scientific progress consisted in the increasing verisimilitude of our theories, this would not be the case.

But, I think that both in the basic version and in Kuhn's version of the comparison objection, the major weak point is the idea that the correspondence theory entails a comparison between *p* and the respective facts and that such comparison requires independent knowledge of *p* and the facts. The comparison objection states that we need *p* to have access to the piece of reality described by *p* and therefore we cannot consider "the fact that *p*" an independent truth-maker of *p*. But the correspondence theory of truth does not entail the existence of theory independent truth-makers. It claims only that there is a certain kind of relation between the truth-bearer *p* and the state of affairs described by *p*[46]. Thus, I have affirmed neither that there is a metaphysical relation (causation, necessitation and so on) between propositions and facts, nor that the correspondence theory entails specific theses about the metaphysics of facts (for example a realist metaphysics) or the epistemology of facts (that we should have independent access to the relevant scientific facts).

According to the comparison objection (and as we have seen referring to the

46 The correspondence relation can be described by very different viewpoints. The most famous are Russell's congruence and Austin's correlation. Anyway, the comparison objection does not attack a specific kind of correspondence: it is a purely epistemological objection (McDermid 2006: 14). This allows us to characterize the correspondence theory in a very minimal way, without dealing with metaphysical implications.

evolutionary epistemology in the last section), scientific theories are always compared with one another, rather than with the facts. One can compare Newton's and Einstein's models to find out which is the best one to explain some empirical facts. But, for the comparison objection, one cannot compare Einstein's model (or Newton's model) with the world. This insight is partially right: comparing theories is very useful because each theory should respond to the anomalies of its rivals and, moreover, scientists are allowed to reject a theory only when they have a better theory. It is meaningless to reject a theory if you do not have another good option. But, at the same time, this does not mean that the determination of the truth-values of scientific theories has nothing to do with the world. In its application to the philosophy of science, the comparison objection poses a problem about the ontologies of scientific theories: the problem of the match between the ontology of a theory and the relative section of reality. But the empirical testing of scientific theories does not mention ontology. Scientists do not compare the ontological claims of their theories with reality; rather, they compare empirical propositions (broadly conceived) with the results of their experiments. For example, if the verisimilitude of scientific theories depended on their ontology, every heliocentric theory would be better than any geocentric theory, since it is quite certain that the Earth revolves around the Sun, but this is not true. From an empirical viewpoint, the Ptolemaic model was more successful than the early Pythagorean geocentric model, since it had much more verified consequences than the heliocentric theory before the Copernican revolution (even if it was based on a false assumption).

Therefore, I think that, when we refer to the verisimilitude of scientific theories, ontology is not the main problem. In order to explain this question, I will introduce a distinction between empirical verisimilitude and ordinary verisimilitude.

4. Empirical and ordinary verisimilitude

In the last section I have summarized the steps of the comparison objection and I have affirmed that it assumes an idea of "comparison" that is not required by the correspondence theory. Specifically, as regards scientific knowledge, it mentions an impossible "match" between ontology and reality and I think that this idea is misleading. On that matter I propose a distinction between empirical and ordinary verisimilitude; it is a distinction between the empirical and the ontological features of science as well. For example, Lakatos claims that:

> 'Verisimilitude' has two distinct meanings which must not be conflated. First, it may be used to mean intuitive truthlikeness of the theory; in this sense, in my view, all scientific theories created by the human mind are equally unverisimilar and 'occult'. Secondly, it may be used to mean a quasi-measure-theoretical difference between the true and the false consequences which we can never know but certainly may guess. (Lakatos 1978: 101 n. 1)

The term "verisimilitude" has two meanings: at first, it refers to the intuitive verisimilitude of the theory, the ontological similarity between the entities posited by scientific theories and the entities that are "really there". This is the commonsensical or ordinary meaning of 'verisimilitude'. Referring to *a*, *p* is verisimilar if it looks like *a*, according to some relevant features. The comparison objection may apply to this concept of verisimilitude, but, as regards ordinary verisimilitude, we can accept the skeptical conclusion of the argument without rejecting the correspondence theory. Since our knowledge is fallible, it is not a problem that all scientific theories are equally unverisimilar from an ontological viewpoint. For example, the analogy between atoms and the Solar System does not entail that they really look like the Solar System.

But what is important is that the empirical statements that we infer from the analogy are confirmed by the relevant experiments. Therefore, let me present the second meaning of "verisimilitude": from a technical viewpoint (presented by Popper and many others), verisimilitude refers to the difference between the true and the false empirical consequences of a theory. Of course, this is a controversial thesis and there are many different accounts of the concept of verisimilitude that I cannot discuss here (See for example Niiniluoto 1987 and Oddie 1986). But my point is that such a theory can account for many important facts about truth that the "ordinary theory" cannot explain. For example that, just like some falsehoods are closer to the truth than other falsehoods (and some truths are closer to the truth than some other truths), sometimes some falsehoods are closer to the truth than some truths: it depends on the empirical consequences of our theories. For example, take a sentence which lacks empirical content by definition, such as a tautology. Tautologies are true: the proposition "all the atoms are atoms" is true. On the contrary the proposition "electrons, protons and neutrons are the fundamental elements of the atom" is false, but, from an empirical viewpoint, it is *truer* than "atoms are atoms". The example of the tautology may be misleading, but take a proposition like “atoms are divisible”; it is true, but, anyway, it is less true than “electrons, protons and neutrons are the fundamental elements of the atom” (if the words “electrons”, “protons” and “neutrons” are meaningful). The same remark applies to the Pythagorean sentence “the Earth revolves around the Sun”, which is true from the commonsensical viewpoint, but it is trivial from an empirical viewpoint, since we cannot verify many empirical sentences if we take for granted such Heliocentric statement. The question is that the correspondence theory does deal with the empirical sentences that we are allowed to express (and test) by means of the theoretical structure of the theory. Here the comparison objection does not apply because no ontological comparison is involved in the empirical testing of scientific theories. Consequently the correspondence theory of truth applies to empirical statements: if *p* is a scientific statement, *p* is true if and only if it corresponds to p; i.e., it agrees with the results of experimental practice. Scientific truth is necessarily an “alleged truth”, that is to say truth according to the current methodology of scientific research (including fallibilism). But this does not put in doubt the correspondence theory.

4.1 Ontological verisimilitude

I have stated that ontological verisimilitude is not a criterion for scientific progress. It is not required that the entities posited by scientific theories look like the “real entities”. Anyway, I think that here we are dealing with reference and scientific realism, not with the truth. Or perhaps, truth and ontological verisimilitude are related in so far as truth involves concepts such as reference and representation. One can think that, according to the correspondence theory, scientific theories should provide us with a “reliable representation” of the real states of affairs. But I have argued that the correspondence theory does not entail a specific theory of reference or representation. As I have sketched at the beginning of the chapter, this is a dividing line between two kinds of theories of truth: the former states that truth deals with the correct representation of the mind-independent world (representational truth); the latter that truth does not depend on (realist) notions such as “representation”, “reference”, “denotation”.

The representational theory of truth (Lynch 2009: 21-32) rests on the concept of reference. It can be expressed as follows:

- RCT (Representational Correspondence Truth): the snow is white is true if and only if the object denoted by “the snow” has the property denoted by “being white”.

The idea is that the truth-values of propositions are determined by the representational relation between their components and the objects and the properties they stand for (and by the logical connectives). Obviously, (RCT) should include a theory of how names and concepts denote the respective objects and properties and there are several possible answers to this question (reflecting our metaphysical commitment). But I think that this is not an effective objection, since, as I have argued in Part 1, a strictly realist concept of reference is not necessary for scientific statements. Reference is not essential to explain empirical success in terms of approximate truth (Hardin, Rosenberg 1982).

At first, as regards truth in general, note that Tarski's theory does not necessarily go in that direction. In fact, Tarski defines the truth of a sentence by means of the concept of satisfaction by every sequence of objects. So the concept of satisfaction (as well as the concept of truth) is defined appealing to the relation between infinite sequences of objects and atomic sentential functions. “Facts” as something referred by whole true sentences do not appear in Tarski's theory, which is an (infinite sequence of) object-based theory, rather than a fact-based theory. From this premise, Davidson concludes that the semantic theory is not a kind of correspondence theory (since if a sequence satisfies a sentence, then the same sequence satisfies any other true sentence; see Davidson 1996). On the contrary, other scholars conclude that the structure of the semantic theory allows us to elaborate a weak or modest correspondence theory that avoid the comparison objection, since there is no need to compare whole sentences with the world (see Marino 2006 and 2008 and Kitcher 2002). My discussion is consistent with the idea of a weak correspondence theory (that I will develop in Part 3), but, since my discussion is restricted to scientific knowledge, there is a more direct way of addressing the problem.

In fact, at second, as regards scientific truth, Cruse and Papineau (2002) argue that, referring to the question of approximate truth, the answer to the problem of reference (and referential failure) is not a “generous” theory of reference (just like the causal theory of reference). For example, a traditional strategy adopted by the generous theories of reference to explain the success of non-referential theories consists in the identification of co-referential terms (see Part 1 – section 2). According to this perspective, ether and electromagnetic field refer to the same entity and this explains the success of the theory of ether (although ether strictly construed does not exist); but, as the generous model argues, the ether theory fails to account for certain phenomena, which are explained by the electromagnetism theory. But this is precisely a too generous criterion, since we may arrange every theory to be co-referential with any other theory, if we admit this kind of exception: for example we might say that the theory of gravitation and the theory of "natural place" are co-referential (because they both explain the cause of fall), although Aristotle's theory fails to account for some phenomena explained by Newton. Cruise and Papineau suggest that the correct solution is exactly the opposite: they reject this concept of reference.

According to this model, the relevant content of scientific theories is captured by their Ramsey sentences. Ramsey sentences express the theoretical predicates of the theory by means of existentially quantified variables. Roughly, the theory

$T (F_1,\ldots, F_n)$

can be expressed as follows by means of its Ramsey sentence

$(E!x_1)\ldots(E!x_n)T(x_1\ldots x_n)$.

The Ramsey sentence says that there are some objects which are correlated to some experimental data through certain postulates. But it does not say what those entities are. Therefore Cruse and Papineau conclude that "the referential success or failure of the terms in a theory is irrelevant to the approximate truth of its Ramsey-sentence, since the theoretical terms do not occur in the Ramsey-sentence" (Cruse and Papineau 2002: 147). For example a theory that uses the word "ether" fails to refer, but the truthlikeness of the relevant Ramsey sentence does not depend on the referential failure of the word "ether". Ether may be not-existing, but a sentence deploying the word "ether" may be approximately true if it says that there is an entity which is the seat of electromagnetic phenomena, involves a given kind of radiation and has other (detection) properties confirmed by the electromagnetism theory[47]. Here I am not committing myself to the distinction between theoretical and observational terms (rather, I am using the Ramsey-sentence to clarify non-referential terms). My point is that the rejection of the generous theories of reference (and the fact that we can "deflate" the non-referential theoretical claims of science) is not problematic for the correspondence theory of scientific truth: we can explain the approximate truth of scientific sentences without appealing to a realist concept of reference and therefore the comparison objection is not effective.

Finally, I can conclude that the comparison objection to the correspondence theory should be rejected, because it does not apply to the empirical statements of science, since their verification does not involve a comparison between "scientific entities" and "real entities".

5. Conclusions

In this chapter I have discussed the consequences for the philosophy of science of the comparison objection to the correspondence theory, which states that we should reject the correspondence theory because it entails the idea of a direct comparison between theory and reality, and such comparison is meaningless. In the first part I have distinguished the comparison argument from pessimistic induction and I have concluded that the former is based on the most basic premise of the latter: that there are genuine cases of referential failure and reference is, to some extent, language-dependent. But this does not threaten scientific truth, since the correspondence theory does not entail a specific theory of reference. Therefore, in the second part I have reconstructed the comparison objection and I have argued that its weak point is the idea that the correspondence theory requires some kind of comparison between real entities and theoretical entities. I have rejected this idea distinguishing the ontological claims of science (from a commonsensical viewpoint) from the empirical claims. From the empirical viewpoint, the verisimilitude of scientific theories does not involve ontologies, but, rather the evaluation of the empirical content of scientific theories; therefore, the fact that successive theories postulate very different ontologies does not question their empirical content. On the contrary, from the ontological viewpoint, the

47 This kind of solution is sympathetic with epistemic structural realism; but ontic structural realism can address the comparison objection as well as every theory that is not literally committed to a one-to-one relation between theoretical entities and "real entities"; I will explain in Part 3 – section 2 why the ontic way fails for other reasons.

comparison objection is not a problem for scientific truth, since the questionable theoretical claims can be expressed by means of the Ramsey sentence, which allows us to distinguish the problem of truth from the problem of referential success and failure. Finally, my conclusion is that the comparison objection should be rejected, and so Kuhn's version of the incommensurability thesis.

Part 3: Defending Correspondence Truth

In the last section I have explained why Kuhn's arguments against the correspondence theory of truth (based on the ideas that I have endorsed in Part 1) do not sound good and I have concluded that a weak correspondence theory (no one-to-one correspondence or isomorphism between theory and facts, no strong commitment to the notion of fact) is consistent with the language-dependence of reference and with the claims that I have presented so far. But now, it is time to develop a positive view about scientific truth, defending the weak correspondence theory that I have sketched in Part 2 – section 4. This chapter is totally apart from Kuhn's works and the debate based on them; even though it is dependent on the Kuhnian concepts that I have exposed in the previous sections, it is an analysis of the concept of scientific truth that take us further away from traditional issues in the philosophy of science. Above all, the main point is that the concept of interpreted structure (and exemplary cases) that I have introduced, far from being a problem for the correspondence theory (as Kuhn thought to be), is a plus point of weak correspondence. In fact, I will argue that a proper correspondence theory is not consistent with the idea that pure mathematical structures are the essential part of scientific theories; on the contrary only interpreted structures (based on previously fixed meanings) can be true or false in any significant sense. So, at first (section 1), I will say that, besides not needing the concept of fact, weak correspondence does not need a unitary account of the correspondence relation as well. So, the fact that, in scientific contexts, the correspondence relation can obtain in different ways is not problematic, once we have realized that this does not imply that there is more than one truth property. Therefore, I will highlight the importance of the concept of interpreted structure (and language) in order to use a proper truth predicate (section 2); this differentiates my approach to the current ones in scientific realism and model theories. Finally (section 3), I will explain the difference between my modest correspondentist approach and pure deflationary theories: to do this, I will focus on the explanatory role played by the truth predicate in the inference from truth to success (and vice versa). In particular, my point is that the deflationary theories fail to account for the concept of approximate truth, while weak correspondence does not. Moreover, in the last chapter (section 4), I will summarize weak correspondence, present a concrete application of the theory and reply to some questions and objections. Thus, in the end, contrary to what

Kuhn himself believed, the Kuhnian perspective on meaning and reference is not only consistent with correspondence truth, but it constitutes, to some extent, evidence for the correspondence theory (for the concepts of exemplary applications and physical interpretation).

Section 1: Can a Philosopher of Science be an Alethic Pluralist?

1. Introduction

In the last chapter I have discussed and rejected a classic objection concerning correspondence truth. Now, in this chapter, I will focus on another important point: the ways the correspondence relation can obtain. In fact, it seems that, at least in scientific context, the correspondence relation can obtain in different ways; and this may be a problem for the correspondence theory, which claims that there is only one way of being true (one truth property). Rather, basing on the plurality of the correspondence relation, one may conclude that we should endorse some kind of alethic pluralism. Thus, in this chapter, I will analyze this pluralist theory in the formulation recently proposed by Ian Hacking (2012). Here Hacking claims that there is a parallel between his theory of the styles of scientific reasoning (and the idea of scientific truth that they entail) and the pluralist theory presented by Wright (1992). But he merely says that his theory (the styles of reasoning) "gibes well with Crispin Wright's pluralism" and does not clarify this idea in depth and at length. Here I will try to explain and discuss this thesis. Roughly, just like alethic pluralism claims that there are as many truth properties as there are discourse-fields (empirical, moral, legal, mathematical and so on), scientific truth pluralism claims that there are as many truth properties as there are styles of scientific reasoning (mathematical, experimental, probabilistic, analogical and so on). Since this is still unclear, I will identify three feasible kinds of scientific alethic pluralism: (1) method-dependent (alethic pluralism follows from methodological pluralism); (2) discourse-dependent (alethic pluralism follows from the plurality of styles of reasoning); (3) language-dependent (alethic pluralism follows from the impossibility to define a language-independent truth predicate). I think that the only tenable position is (3), but it is not a kind of standard alethic pluralism, since it does not imply that there is a correspondence relation for each style of reasoning. So, at first, let me present the terms of the question.

Styles of Reasoning (SR). By styles of reasoning, Hacking means what Crombie calls styles of scientific thinking: simple postulation in mathematics, experimental exploration and measurements, construction of analogical models, ordering by taxonomy, calculus of probability, historical analysis of genetic development. The styles of reasoning allow scientists to express meaningful scientific sentences (candidates for truth-value attributions) in a given context and establish what counts as rational or irrational in that field.

Alethic Pluralism (AP). In the basic interpretation, alethic pluralism is the claim that there is more than one truth property[48]. For example, in scientific contexts, the idea that truth means correspondence between sentences and facts seems to be intuitively plausible. But this idea is less plausible referring to contexts such as ethics or aesthetics. So truth (T) is a substantive property (the property T exists and any true sentence is true in virtue of being T and this is not transparent in our concept of truth), but consists of several properties T_1, T_2, …, T_n.

Moreover, the possible kinds of pluralism that I will propose to apply (AP) to scientific knowledge and practice are as follows:

Methodological Pluralism (MP). Methodological pluralism argues against the idea of a single and universal scientific method, invariant through the history of science and scientific fields (Sankey 2000). On the contrary, we can recognize a plurality of methodological rules governing theory evaluation and choice. Methodological rules may be modified through the history of science, may apply in different ways in different fields of scientific investigation and different scientists may interpret the same rules in different ways.

Discourse-Field Pluralism (DFP). According to discourse-field pluralism, our discourse (D) shows some natural and intuitive divisions, such that we can divide D in some quite stable regions of discourse D_1, D_2, …, D_n (See for example Putnam 1994). In ordinary discourse, such fields are the empirical field, the juridical field, the moral field

48 Here my discussion is based on Wright's standard alethic pluralism (Wright 1992). Actually I am not considering the functionalist variant (see Lynch 2009), claiming that there is only one truth property, which manifests itself in different ways.

and so on. As regards science, (DFP) claims that scientific discourse, too, is divisible in different, natural and quite stable regions of discourse, mainly corresponding to the styles of scientific reasoning.

Language Pluralism (LP). Language pluralism is the thesis that a universal scientific language (*L*) does not exist and therefore scientific language consists of several sub-languages independent of each other (see for example Kuhn 2000). Each language defines its own terms and sentences and the stability of meaning (within a given language L_1 or L_2 or L_n) is a precondition for the definition of a truth predicate for L_1 or L_2 or L_n. Languages are distinguished basing on the semantic relations between the most basic sentences, concepts and terms deployed by the mathematical core (if any, otherwise the general core) of scientific theories.

Therefore, in this chapter I will try to clarify the relation between (SR) and (AP), taking into account the possible kinds of pluralism in scientific discourse and practice, i.e. methods (MP), discourse-fields (DFP) and languages (LP). If my interpretation is right, Hacking's idea rests on a combination of (MP) and (DFP), but I will argue that either it does not entail (AP) (but, rather, only epistemological pluralism) or it is untenable. On the contrary (LP) is tenable, relevant to scientific truth and partially consistent with (SR), but it does not entail (AP), at least in its standard interpretation (the number of truth properties is greater than one). So, I will conclude that the pluralist argument does not threaten the correspondence theory, which is consistent with the idea that there is no available unitary account of the correspondence relation. Moreover, I will go into the details of language pluralism in the next chapter, but actually the point is that is not a metaphysical claim about the essence of truth is, but rather a linguistic question concerning the relation between meaning and truth (and the truth predicate, rather than the truth property).

2. From the styles of scientific reasoning to alethic pluralism

In this section I will explain how alethic pluralism follows from Hacking's conception of scientific knowledge and practice. At first, I shall remind that an explicit definition of "style of reasoning" is not available. Anyway, we can provide a tentative list of the most important styles of thinking in the history of science.

1. Simple postulation in Greek mathematical sciences
2. Experimental exploration and measurement
3. Hypothetical construction of analogical models
4. Ordering of variety by comparison and taxonomy
5. Statistical analysis and calculus of probability
6. Historical derivation of genetic development

These styles are listed by Crombie and Hacking adds the laboratory style characterizing contemporary science: the construction of instruments for the isolation of existing phenomena and the "creation" of new ones. Roughly, the styles of reasoning have four features[49]: 1) they introduce new objects, laws, kinds of sentence and kinds of explanation within scientific investigation; 2) they are self-authenticating, i.e. they define their own criteria of adequacy, rationality and meaningfulness; 3) each style develops its techniques of stabilization (and some techniques are more efficient than the others); 4) at the same time, each style is grounded in the cognitive capacities of human beings (depending on evolution and natural selection) and in human history and culture.

For my purpose, the most important feature is the second one: self-authentication. Roughly, self-authentication (or autonomy) means that the styles of reasoning are not good because they help us to track truth. Rather, they have established standards and criteria of truthfulness and these criteria justify themselves without recurring to further criteria. This is a possible answer to the problems concerning foundationalism and justification, for example the problem of the criterion. Consider the problem of the epistemic justification of a given belief and that this belief deals with something immediately perceptible, for example the presence of an object in the room I am working in. We will say that the belief is epistemically justified if the relevant sensorial experience obtains (for instance, if I see the object with my eyes). But this process assumes an epistemic principle: that we should trust in our senses and use immediate experience as a criterion. But how can we justify the criterion itself? There are three possible answers to this question (Sankey 2011):

- appealing to a further criterion (to meta-justify the original criterion), but, in turn, such criterion should be justified by another criterion, falling into an infinite regress;
- appealing to the original criterion (for example, using sensorial experience to

49 For a recent introduction to the styles of reasoning see Ruphy 2011.

justify our trust in sensorial experience), but this kind of justification is circular;

- blocking the infinite regress, claiming that there is a criterion that does not require justification, but in this case justification would be a dogmatic matter.

The theory of the styles of scientific reasoning provides us with an answer to this puzzle, since it claims that styles do not ask for a meta-justification. They do not respond to any other, independent or higher standard than their own[50]. I will return on the problem of the criterion further below. Now I just mean that the self-authentication of the styles of reasoning consists in the fact that nothing more primitive or fundamental can be said in meta-justification of their standards. But, in order to understand how self-authentication is related to scientific truth, I have to introduce the notion of candidate for truth or falsehood.

In fact, defining its standards, a style of reasoning defines the kinds of sentence that are available for truth-value attributions. A candidate for truth or falsehood is a sentence that can be expressed and tested or justified according to the current and shared standards of scientific investigation (formulated by the relevant style of scientific reasoning). Hacking suggests to consider the truth-value of a sentence such as "the gross national product of Württenberg was 76.3 million adjusted to 1820 crowns" and claims that, before the beginning of the 19th century, it was not candidate for truth-value attribution. From the one hand, it is because the concept of gross national product was not defined before, but, above all, it is because "there was no procedure of reasoning about the relevant ideas" (Hacking 1992: 143) since the statistical style of reasoning is a precondition for the possibility of being true-or-false of this sentence. Of course, this does not apply to all sentences, but only to those sentences whose conditions of verification (and expression) are strictly related to the existence of the relevant style. For example, it is arguable that the sentence "the cat is on the mat" is true-or-false independently of any style of reasoning and at any time of human history and culture. Moreover, the theory of the styles of scientific reasoning is neither anti-realist (the truth-makers of scientific sentences are language-dependent or style-dependent or mind-dependent) nor relativist (the truth-value of a sentence may vary across cultures, theories, styles and individuals). The style determines the sentences having positivity

50 Now one may associate the styles of reasoning is with pragmatism, since it is based on the fact that styles of reasoning work. But this is not the case, since success helps us to determine what will count as success, since it characterizes what will count as success in the future.

(the candidates for truth and falsehood), but does not determine the actual truth-value of such sentences, while the world does. The facts make sentences true (or false), but sentences are available for truth-value attributions thanks to the style they belong to. The combination of these two claims (self-authentication and candidate for truth-value attributions) has two consequences about the relation between truth and the sciences.

From the one hand, at a macro-level (scientific progress as increasing convergence to the objective world) the pursuit of truth cannot be considered the general aim of science: since each style justifies itself and is true according to its own standards, it is pointless to look for a uniform and coherent direction in the history of science, namely increasing approximation to the truth. This deals with Hacking's conception of the disunities of sciences (Hacking 1996) and could be an argument for pessimistic meta-induction, but I will not discuss it here.

From the other hand, at a micro-level (the truth-value of individual scientific sentences), we might conclude that the correspondence theory of truth is not sufficient to account for scientific knowledge and practice. Hacking does not deny that scientific sentences are true because (to some extent) they correspond to the world; rather, he rejects the pretension to universality of the correspondence theory (Hacking 1992: 135). This is the origin of alethic pluralism. According to the monist theories, all true sentences share a property, namely the property of "being true", where "being true" may mean "corresponding to the mind-independent world" or "belonging to a coherent set of beliefs" or "being warrantedly assertible" and so on. To the question "is there something that all true sentences share?", the monist would respond, for example, that all true sentences correspond to the facts; or that all true sentences belong to a coherent set of beliefs; or that all true sentences are warrantedly assertible. On the contrary, according to the theory of the styles of reasoning, the answer is negative: there is no theory of truth which applies to all the contingent empirical sentences and therefore there are as many ways of being true as there are ways of reasoning about the relevant sentences[51].

In the following sections I will discuss how truth works at the level of the truth-values of scientific sentences; but now, I have to outline the pluralist theory entailed by the styles of reasoning.

51 Some theories of truth affirm that truth is correspondence, but there are many ways of corresponding to the facts (see for example Horgan 2001 or Sher 2013). But they claim that the correspondence theory is the common basis to understand how truth works, while Hacking denies that it is exhaustive of the notion of truth.

3. Outlining a pluralist theory of truth for scientific sentences

Therefore, to summarize, the relation between alethic pluralism and styles of reasoning is based on two premises:

- Self-authentication: each style defines its own standards of meaningfulness, rationality, objectivity (and so on) and such standards are not asking for a meta-justification. The relation between style and truth is circular: "the truth is what we find out in such and such way. We recognize it as truth because of how we find it. And how do we know that the method is good? Because it gets at the truth" (Hacking 1992: 135).
- Candidates for truth or falsehood: some scientific sentences are not true-or-false in general, but, rather, true-or-false referring to the style of reasoning they belong to. That is because some sentences are not available for empirical procedures independently of the existence of such styles. A sentence that is true-or-false according to a given style of reasoning could not be candidate for truth-value attribution in another style.

Basing on these premises, Hacking concludes that the number of truth properties is greater than one in scientific context as well. Roughly, such theory of truth has three features and shares them with the standard interpretation of alethic pluralism:

1) Substativism: unlike the deflationary theories, the substantivist theories of truth claim "being true" to be a genuine property and that the schema "p is true if and only if p" is not exhaustive of the notion of truth. Better, according to both the theory of the styles of reasoning and to alethic pluralism, the nature of the truth property is not transparent from the analysis of the concept of truth (just like the property denoted by water, i.e. "being H_2O", is not transparent from the concept of water). And, of course, claiming that there is more than one way of being true is not consistent with the idea that truth is transparent[52]. Since the relation between style of reasoning and truth is circular, if truth had no nature, we would not be able to individuate the styles of reasoning; that it

52 In some works, Hacking seems to suggest that the deflationary theory can be applied to scientific knowledge. But I think that this claim should not be literally interpreted and he really means that: 1) there is no unitary theory of truth for scientific knowledge; 2) truth cannot be used as explanation for any metaphysical issue.

because, just like a style of reasoning allows scientists to express true-or-false sentences, we would appeal to what counts as true-or-false in a given context to distinguish that context from the others. If truth has no nature, we will not be able to appeal to the truth to recognize the differences between the various styles of reasoning (since we cannot use truth as explanation. See Lynch 2009: 5 for this argument).

2) Objectivism: unlike the subjectivist theories of truth (coherentism and pragmatism), both the theory of the styles and alethic pluralism claim that the truth (or falsehood) of a sentence depends on how things are and not on how someone (individual or group or culture) might think or wish them to be. So, the relativization of truth to the styles does not undermine its objectivity. Both truth-makers and truth are mind-independent (and language-independent and culture-independent), but the relation between sentence and truth-maker can obtain in different ways according to the relevant way of reasoning about it. The actual truth-value of scientific sentences is determined by the world, while the possibility to attribute a truth-value to such sentences (the sentences whose essence is strictly related to the style) is determined by the style of reasoning. The sentence "the gross national product of Württenberg was 76.3 million adjusted to 1820 crowns" is made true by the relevant empirical facts, but it is meaningless (it cannot play any role in scientific investigation) in so far as it is not related to the style it belongs to.

3) Pluralism: while the monist theories of truth claim that there is only one truth property, which is shared by all true sentences, pluralism claims that the there is more than one truth property. In its application to the philosophy of science, it states that in the history of science many different truth properties coexist. This means that some scientific sentences are true in virtue of the property T_1, others are true in virtue of the property T_2 or T_n. Standard alethic pluralism states that the correspondence theory does not apply to all sentences (for example it does not apply to moral sentences), but that only empirical sentences are true in virtue of their correspondence to the facts. On the contrary, the theory of the styles of reasoning claims that the correspondence theory is not satisfactory even in scientific context; and therefore we need a plurality of scientific truth properties, explaining the several ways of being true (or false). A true sentence from the probabilistic field does not have the same property of a true sentence from the taxonomic field.

So, in this section I have sketched the theory of truth related to the theory of the styles of reasoning. I have concluded that, basing on the self-authentication of the styles and the idea of truth-value candidate, this theory is substantivist, objectivist and pluralist (just like standard alethic pluralism). But this claim is not sufficiently justified yet. The fact that scientific sentences are part of different procedures of reasoning about them is not sufficient to justify the existence of more than one truth property. One may object that there is only one property (correspondence) and that the styles of reasoning entail only epistemological pluralism and not alethic pluralism. Therefore, in the next sections I will discuss three possible sources of alethic pluralism in scientific context: methodological pluralism, discourse-field pluralism and language pluralism.

4. Option (1): methodological pluralism

In the last section I have stated that scientific alethic pluralism is a theory of scientific truth (substantivist, objectivist and pluralist) claiming that there is more than one truth property in scientific discourse. In order to justify such claim, one should show how the metaphysical thesis about the existence of a plurality of truth properties is related to the epistemology of the styles of reasoning. In this section and the following ones I will discuss three possible answers: methodological pluralism, discourse-field pluralism and language pluralism.

In this section I will focus on methodological pluralism. Theories of scientific method are usually divided in two main sets: monist and pluralist[53]. Roughly, methodological monism argues that there is only one universal scientific method, which applies to all scientific puzzles, independently of historical changes, research fields and subjective preferences. On the contrary, methodological pluralism is the claim that, through the history of science, many different (and valid, even if not necessarily equally valid) scientific methods were used. Moreover, scientific method does not change only from a historical viewpoint. Scientific communities working in different fields can adopt different methods and, finally, the same methodological rules may be interpreted in different ways by individuals or groups.

Methodological pluralism can be construed historically and/or normatively. From a historical viewpoint, it claims that the succession and coexistence of several scientific methods (and interpretations of methodological rules) is merely a matter of fact. And

53 Of course there are many intermediate positions, for example Worrall 1988.

from a normative viewpoint it claims that methodological pluralism is worthy, for example because scientific problems are complex and we are more likely to solve them with multiple approaches[54].

Methodological pluralism is obviously consistent with the theory of the styles of reasoning. Actually, it is a direct consequence of the plurality of the styles: in fact each style is related to a set of (more or less explicit) different methodological rules. The methodological patterns used by scientists working in the laboratory sciences are different from the ones used, for example, for computer simulation. But methodological pluralism is a kind of epistemological pluralism and not of alethic pluralism. It states that there is more than one way to achieve significant scientific results and therefore, in the realist interpretation of scientific practice, to get closer to the truth. But this does not mean that there is more than one way of being true. The extension of the methodological claim to the metaphysical one is based on the argument of the criterion and the self-authentication thesis. Recall that self-authentication means that each style of reasoning defines its own standards and no meta-justification is required. Moreover, in the previous sections, I have argued that the problem of the criterion goes as follows:

- the acceptance (or rejection) of a belief (or a set of beliefs) is regulated by epistemic norms, stating the conditions under which a belief is justified;
- the acceptance of epistemic norms should be justified by means of other epistemic norms;
- the attempt to justify epistemic norms leads to a) infinite regress; b) circularity; c) dogmatism.

The styles of scientific reasoning welcome circularity since each style justifies itself. This is a controversial but defensible claim (see D'Amico 1993). What, I think, is not defensible is the claim that the plurality of truth is a consequence of the plurality of methods. The argument seems to be something like this:

1. rival theories are justified by different standards (problem of the criterion);
2. dealing with justified beliefs as if they were approximately true is rational;

54 This and other two arguments are provided in Chang (2012), a recent defense of scientific pluralism: 1) since liberal democracy is the best form of social organization and pluralism is essential to democracy, science should be organized in the same way; 2) the plurality of actual scientific methods cannot be reduced to a single and universal scientific method in any way.

3. truth is relative to the standards of scientific research.

This argument does not sound good, even if it is based on a correct insight. The correct insight is the strong methodological import of scientific truth. Of course, scientific research is not interested in trivially true sentences, but, rather, in sentences considered approximately true according to the current and shared standards of scientific inquiry. But this is a methodological advise to scientific communities: that one should not compare scientific theories recurring to the ordinary standards of truth (for example isomorphism between the structure of the truth-bearer and the structure of the state of affairs), but recurring to other standards provided by the community (for example the possibility to apply the actual achievement to other problems). And the fact that there is more than one truth property does not follow from this methodological advise. For example, take the property of "being yellow". Such property can be detected in different ways according to our purposes and fields of inquiry: the sentence "x is yellow" is considered true or false according to different standards. For example, for x=my t-shirt, I can trust my senses and my immediate sensorial experience. But, for x=a chemical substance, the property of "being yellow" is usually a consequence of the excitation of electrons due to an absorption of energy (by the chemical), where what I see by my senses is not the absorbed color, but the complementary color. But obviously this does not mean that there is more than one "yellowness" property.

Therefore, methodological pluralism is an epistemological thesis (plurality of scientific methods) which does not necessitate any thesis about the metaphysics of science (like alethic pluralism). In the next sections I will discuss other options to justify alethic pluralism.

5. Option (2): discourse-field pluralism

In the last section I have considered whether or not the methodological pluralism entailed by the styles of reasoning is a source of alethic pluralism. Since my answer is negative, in this section I will focus on the second option, which is more true to the spirit of standard alethic pluralism. In fact, while in the last section I have discussed the methodological nature of the styles of reasoning, here I will discuss the style as a whole, and how it divides scientific discourse. The hypothesis that I am going to examine is that scientific alethic pluralism depends on discourse-field pluralism.

Roughly, discourse-field pluralism claims that human discourse (*D*) presents some stable and intuitive divisions: *D* is divided into D_1, D_2, …, D_n. Alethic pluralists add that there are many truth properties (T_1, T_2, …, T_n) and each truth property is such for a given discourse-field. Discourse-fields can be considered mere fragments of a language, i.e. proper and disjointed sub-sets of the sentences of that language. The distinction between domains of discourse is ambiguous, but intuitively clear. They are bare areas of thought and we can identify them by means of the kind of sentences they are composed of; and, in turn, the kind of sentences can be identified by means of the concepts they use and that we employ to speak about specific matters (see Lynch 2004: 399-400). This is ambiguous since it assumes that we already have a clear classification of the relevant concepts and the discourse-fields they belong to. But obviously it is quite clear that sentences such as "torture is wrong" belong to the moral field. And the same applies to sentences about aesthetics or the physical processes that shape the world.

The same considerations should apply to the sub-set of *D* called "scientific discourse" (D_S). According to the theory of the styles of scientific reasoning, D_S is divided in (at least) seven sub-sets corresponding to the styles that I have listed in the previous sections. Therefore, the styles of reasoning are obviously consistent with discourse-field pluralism. From the one hand (historical viewpoint), the coexistence of several fields of discourse in the history of science is a matter of fact. From the other hand (normative viewpoint), such a plurality of styles is both a means and an end of scientific progress: it is a means because different styles are necessary to solve problems from different fields; and it is an end since styles are cumulative and the accumulation of styles constitutes scientific progress in itself (see Hacking 2012: 607-608).

But, as regards the styles of reasoning, the question of the individuation and the mutual relationships between them is even more crucial. In fact, while the distinction between discourse-fields can seem arbitrary, but it is intuitively clear, the question of the styles is more controversial. Since the beginning of this chapter, I have recognized that there is no definition of style of reasoning (even if there are some basic features). Hacking is aware of this and states that it is impossible to define a style: they have no essence and are bound together only by family resemblances (Hacking 2012: 601). This is not necessarily a problem, but it is such in so far as alethic pluralism entrusts an important task to the sub-sets of *D*: the possibility of distinguishing the truth properties.

Hacking does not seem to be too concerned with this question: he does not go beyond Crombie's list and restricts himself to add the "laboratory science" style. In the previous section, I have summarized the features of the styles (new objects, laws and explanations, self-authentication, techniques of stabilization, interaction of cognitive capacities and culture) and Hacking has often stressed the role played by the techniques of stabilization (to exclude "humanistic" styles of reasoning)[55], the techniques that enable a style to stabilize itself. But he does not elaborate this criterion in depth and, on the other hand, the intuitive criterion used by standard alethic pluralism (the kinds of sentences and concepts used in a given domain) is suspicious for the styles of reasoning. In fact, in scientific context, it is quite difficult to distinguish between the lexicon and the concepts of a style of reasoning and those of the leading theories in that context. We run the risk of moving from a style-based theory to a theory-based theory of science. And one of the main points of the theory of the styles of reasoning is the possibility to downsize the importance of theories to explain scientific knowledge and practice (see in particular Hacking 1983).

So, in the end, the theory of the styles claims that 1) there are no necessary and sufficient conditions to identify a style and demarcate it from the others; 2) that the style of reasoning are interwoven and "can all be called upon in a single research project" (Hacking 1992: 137). While the former claim is consistent with alethic pluralism, the latter is not. According to discourse-field pluralism, the sub-sets of D (and D_S) should be disjointed[56]. That is because there is a mutual relation between the truth properties T_1, T_2, …, T_n and the domains of discourse D_1, D_2, …, D_n. From the one hand, the features of a given domain of discourse allow us to understand how the truth property works in that context. For example, the presence of terms such as "good", "wrong", "just" is characteristic of moral language and creates expectations about the kind of truth property that we will find in this domain of discourse. On the other hand, the substantiveness of the truth predicate allows us to identify the difference in content between sentences from different fields. For example the content of a moral judgment is different from the content of a physical statement because the former pretends to be "super-warranted" and the latter pretends to "correspond to the facts". Otherwise, the appeal to different truth properties to explain the differences between domains of

55 For example Hacking 1996, where he suggests that they can "characterize and constitute a style" (Hacking 1996: 72-73). For the question of the identity of the styles and their mutual relations see Kusch 2010.

56 See Lynch 2004: 400. For some objections see Sher 2005.

discourse (and the other way round) would be superfluous (Lynch 2004: 402).

In fact, if this relation between truth properties and discourse-fields does not obtain, we will find the same method-dependent alethic pluralism that I have rejected in the previous section. If we cannot really individuate the different domains and truth plays no role in this distinction, what we can conclude is that truth is merely dependent on the styles. In other words, there is no mutual dependence (the kind of dependence which allows the pluralist to recognize the differences in the content, for example, of moral sentences and non-moral sentences) between truth properties and discourse-fields and the result is that truth is style-relative. In other words, following the methodological insight that I have sketched in the previous section (the fact that scientific truth is methodologically-oriented), we should give up truth and replace it with an "objective surrogate for truth to be found in methodology" (Hacking 1979: 386), as Hacking says commenting on Lakatos and concluding that the task of post-Kantian philosophy of science is to replace the concept of representation (namely, truth as correspondence) with the concept of methodology. So, my claim is that, as a consequence of the impossibility to provide a clear distinction between the styles of reasoning (and the impossibility to use the truth property to achieve this aim), scientific alethic pluralism collapses into epistemological pluralism: truth plays no role (except some minimal logical role), unless we consider truth a mere by-product of the styles. To that extent, Hacking's pluralism seems to be more similar to the theories that, resting on the dissatisfaction with the correspondence theory, ascribe a limited role (or no role) to truth (like historicism or the sociology of scientific knowledge), than to alethic pluralism.

Moreover, to strengthen this interpretation, the hypothesis of the interwoven styles seems to be more plausible than the hypothesis of the disjointed sub-sets. Take, for example, an important issue in the logic and methodology of science: the conjunction between scientific theories. As pointed out, for example, by Friedman (Friedman 1983)[57], we can conjoin the molecular theory with other theories, such as the atomic theory, to explain atomic energy and therefore the molecular theory will receive indirect evidence from electrical phenomena. Additionally, a theoretical assumption can receive a double confirmation at the same time: the molecular theory contributes to the gas laws and, in turn, they play a role in the explanation of other facts. The same applies to novel

57 Note that Friedman's discussion about theory conjunction is part of an argument for scientific realism. Here I am not committing myself to his conclusions; for an extensive analysis of Friedman's realism see Morrison 2000.

predictions. Take two reductions or explanations *A* and *B*. Both *A* and *B* receive individual confirmation at different times (t_1 and t_2). Imagine that the conjunction of *A* and *B* entails a novel prediction x that does not individually follow from *A* or *B* and that is confirmed at another time t_3. Indirect confirmation is an important methodological feature of scientific practice and we are allowed to make these predictions only basing on the truth of the conjoined explanations. Since we believe that *A* and *B* are true, we are allowed to deduct the prediction x and to verify (or falsify) this prediction. Logical laws such as conjunction are truth-preserving, but, taking this for granted, we should recognize problems related to the conjunctions involving sentences employed by theories from different styles. Recall that sentences from different styles are true in different ways. As a consequence, if *A* belongs do D_1 and then it is T_1, and *B* belongs do D_2 and then it is T_2, it is not clear what kind of prediction we can deduct from the conjunction of *A* and *B*. And, in this case, in what sense the conjunction of *A* and *B* is true (or false). But, in the end, we cannot find any kind of methodological rule about that and then we can conjoin explanations from different fields of discourse[58]. And this seems to show that the difference between the styles of reasoning does not stand at the level of the logic and the metaphysics of truth, but, rather, only at the epistemic level.

So, in this section, I have argued that scientific alethic pluralism does not follow from discourse-field pluralism. Especially because the relation between truth and style of reasoning does not reflect the relation between truth property and domain of discourse. To sum up, my argument goes as follows:

1. alethic pluralism is an inflationary theory of truth (by definition);
2. according to the inflationary theories, the truth property must play an essential metaphysical and explanatory role (according to the concept of explanatory property);
3. the only explanatory role that different truth properties could play in the theory of the styles of reasoning is to enable us to distinguish a style from the others (for the relation between truth and content);
4. but truth does not play this role (for the identity conditions of the styles of reasoning).

58 Maybe Hacking may disagree about that, for his theory of the disunities in science. But, again, I am not considering unity either a metaphysical virtue (and a desiderata for scientific explanation and progress) or evidence for metaphysical realism. I am only dealing with the logical structure of conjunction.

6. Option (3): language pluralism

In the previous sections I have tried to explain how scientific alethic pluralism follows from the pluralist epistemology of the styles of scientific reasoning; I have analyzed two hypotheses that are consistent with Hacking's theory (methodological pluralism and discourse-field pluralism), but I have concluded that they do not entail alethic pluralism. In this section I will analyze the last option, which is less related to the styles of reasoning than the other ones: language pluralism.

Language pluralism is the idea that, given a language L, some parts of L can be isolated and formalized independently of each other, giving rise to several languages L_1 or L_2 or L_n, whose meaning conditions are stable (within the relevant sub-set of L). Kuhn claims something like this when he talks about the possibility to divide scientific languages into sub-sets (or lexical structures), that we can individuate in a quite similar way to Lynch's identification of the different fields of discourse: by means of the kind of proposition they employ and their key concepts, i.e. the concepts deployed by the most basic assumptions of the language we are interested in (for instance, as we have seen in Part 1, the mathematical core for the theories of mathematical physics). For example, the language of classical mechanics is characterized by the concepts that occur in the laws of motions and their relations. In such a way we can establish the minimal identity conditions of the sub-sets of L; these identity conditions guarantee the stability of meaning within L_1 (or L_2 or L_n), which is a precondition for the definition of a truth predicate for L_1 (or L_2 or L_n). In fact, the connection between language pluralism and alethic pluralism is based on Tarski's definition of truth and the idea of truth-for-L. Thus, the definition of a restricted truth predicate for a fragment of L, given a truth predicate T and a proper fragment L_1 of L, goes as follows:

$T_R(x) := T(x) \text{ and } x \in L_1$
(Beall 2013)

But the main problem is that language pluralism is not consistent with the styles of scientific reasoning. Ideally, a style of reasoning should be distinguished from the theories that we are allowed to express by means of the style (and Hacking does aim to undermine the role played by theories); on the contrary, language pluralism is more theory-oriented, since the identity conditions of the different languages depend on the concepts employed by the theories. But, as I anticipated in the last section, it is hard to distinguish the lexicon of a style from the lexicon of the most important theories in that field.

So, since there is no correspondence relation between styles of reasoning and fragments of language, language pluralism cannot entail that there are several ways of being true, and therefore it is not an argument against the correspondence theory. I shall focus on how language pluralism is related to scientific truth in the next chapter, but, right now, the point is that the correspondence theory (I mean, one truth property) is consistent with the fact that the correspondence relation can obtain in different ways.

7. Some morals about correspondence truth

Before concluding, it is time to draw some morals about the correspondence theory. The main point of this chapter was the question of whether there is a correspondence principle underlying all areas of scientific truth. Basing on the theory of the style of reasoning, it is plausible to think that the answer is negative, since principles of correspondence are systematically different in specific areas of scientific practice. But this is not a question that can be solved *a priori*, by means of platitudes, as alethic pluralists usually aim to do: it is a practical matter, that should be solved looking at the various fields of scientific investigations, and not only at the truth. Anyway, what we can conclude about correspondence truth and the correspondence relation from this discussion of the styles of reasoning is that (see Sher 2013 for some of these conclusions):

- we cannot determine in advance the way the correspondence relation between sentences and reality will obtain; and this is not a task of the theory of truth to do this;
- it admits that the correspondence relation can differ from one field to another, obtaining in direct ways in one field, in indirect ways in other fields and being sometimes influenced by contextual and epistemic concerns;
- but, after all, it demands a systematic and significant connection between true sentences and the world in all genuine fields of knowledge (and especially

scientific knowledge).

So, in the end, the correspondence theory maintains that there is a relation between sentences and the world; and the truth property expresses such a relation. But that, at the same time, a theory of truth is not required to give advice about how this correspondence relation obtain; this is a problem that we can solve only by going into the details of how scientific practice goes on, as the theory of the styles of reasoning does, for example.

8. Conclusions

Finally, in this chapter I have analyzed the possibility to apply alethic pluralism to scientific knowledge and practice, in order to overcome the alleged limits of the correspondence theory to account for the different ways of "being true" in scientific context. I have discussed Hacking's thesis that there is more than one truth property in scientific context and, after showing how this thesis is related to the theory of the styles of scientific reasoning (by means of the concepts of self-authentication and candidate for truth or falsehood), I have discussed how alethic pluralism in scientific field is related to methodological pluralism, discourse-field pluralism and language pluralism. But I have concluded that the first two options do not entail the existence of more than one truth property (so the correspondence theory is consistent with several correspondence relations), while the third option does not entail the existence of more than one way of being true (so it is not an argument against correspondence).

Section 2: The Ultimate Argument for Incommensurability

1. Introduction

In the last chapter I have concluded that the correspondence theory of truth is not committed to a unitary account of the correspondence relation and that the fact that it can obtain in several ways (one for each style of reasoning) does not entail that we should endorse alethic pluralism. But I have left open the question of language pluralism and whether it can threaten scientific truth. In this chapter I will face the language-dependence of the truth predicate and I will conclude that it can be a compelling argument for incommensurability. Actually incommensurability is out of fashion and most philosophers of science agree that the most widely accepted realist theories succeed in resisting the attacks of the incommensurability thesis; from this perspective, there is little point in trying to reflate the incommensurability challenge. Therefore, the perspective that I will present in this chapter is quite different from the received view about incommensurability: unlike Part 2 – section 3 – this chapter is not intended as a historical reconstruction or interpretation of what Kuhn (or Feyerabend) really meant. To that extent, my discussion is apart from the reception of Kuhn's works and sometimes it conveys different picture. Rather, the motivation of this chapter is that I do think that incommensurability thesis (properly updated on the light of the current theories about scientific realism and truth) can play a role in the debate about scientific realism and truth. *Pace* Kuhn and Feyerabend, I do not believe the incommensurability thesis to be a serious threat to scientific realism. But, assuming that every complete and satisfactory theory pretending to be realist should accept the semantic thesis broadly conceived (see for example Psillos 1999: scientific theories are descriptions of their intended domains and are literally construed as true or false), the incommensurability thesis can help us to understand what kind of theory of truth is involved in the semantic thesis (plausibly a correspondence theory) and what problems it arises.

The main claim of this chapter (the conclusion of my argument for incommensurability) will be that even the best realist theories actually at the heart of the debate (like structural realism) cannot overcome the challenge of the definition of a proper cross-theoretical truth predicate between non-homologous structures. Structural realism aims to solve the problem of inter-theoretical relations by means of the possibility to preserve the structure of one theory in that of a second, regardless of the "objects" mapped by the respective theories. But my point is that the definition of a truth predicate does depend on the denotation relation, since the truth predicate can be defined only for interpreted languages. Structure preservation can constitute evidence for scientific realism, but it is not sufficient to include a theory of truth into scientific realism. Furthermore, I will show that there is a tension between the deflated concept of reference associated with structural realism and the semantic theory of truth. To achieve this aim, this chapter is divided in two parts: in the first part I will explain the relation between incommensurability and language-dependence of the truth predicate; in the second part I will argue that, in so far as incommensurability is intended as a thesis about the relation between truth and the sciences, structural realism cannot face its challenge.

Finally, before I start, I shall recall some points that I have discussed in the previous chapters and that I will take for granted now. The first and most important is that we should not interpret incommensurability as incomparability or untranslatability; I have discussed this point in Part 2 – section 3. Rather, following Kuhn's latest works (Kuhn 2000), here incommensurability is construed as a structural dissimilarity between scientific theories, which prevents the truth predicate from properly working in inter-theoretical contexts. But I will not literally follow Kuhn's approach, since his attempt to connect incommensurability and rejection of the concept of truth (especially Kuhn 1970: 206) is completely different and merely fails[59]. Secondly, I will assume that the semantic theory of truth is the best way to define a truth predicate for a given language (including scientific languages) and my criticism against structural realism is based on its incompatibility with the semantic theory. I do not exclude that it might be consistent with other kinds of theories of truth.

2. True-in-L

In this section I will introduce the notion of true-in-L by means of Tarski's semantic theory of truth and, in parallel with Quine's attack to the notion of analyticity, I will claim that the semantic theory does not fit well with the general or relational concept of truth.

Tarski claims that the concept of truth can be defined only referring to a given object-language L_1. Roughly, this is for two reasons: 1) the choice of sentences as truth-bearers; 2) the necessity to construe a meta-language to define truth (Haack 1978: 114). Taking for granted Tarski's approach, the problem is which part of scientific theories (sentences, models, mathematical structures...) we can correctly say to be true-or-false. I will come back to this question in the second part of this chapter, but, anyway it is intuitively clear that, from the viewpoint of the semantic theory, only the sentences of an interpreted language can be true-or-false. Thus, Tarski defines truth only for given

59 Since, as I have argued in Part 2 – section 4 – it is based on the comparison objection. See for example Marino 2006.

object-languages. Where he defines the concept of "true sentence", he restricts the definition of sentence to the sentences of a given language:

$x \in Tr$ if and only if $x \in S$ and every infinite sequence of classes satisfies x (Tarski 1983: 195).

Where "Tr" denotes the class of all true sentences and the definition (in the meta-language) "x is a sentence" ($x \in S$) is the limiting case of a sentential function without free variables. And the definition of sentential function appeals to the basic vocabulary of the object-language. So, if the concept of sentence is defined as a limiting case of the sentential function, "S" does not denote the concept of "sentence", but, rather, the concept of "sentence of L_1". In turn, the concept of true sentence "Tr" is defined only for the relevant language L_1. If we accept this interpretation of Tarski's theory (see David 2008), we will conclude that Tarski did not define the concept of truth, but a more restricted concept of truth, relative to a given language. For example, basing on language pluralism we can divide a given language L (for example English) in several (well-behaved) fragments and call them L_1, L_2, …, L_n; and finally we can follow Tarski's guidelines and define a proper concept of truth for each well-behaved fragment of L.

What conclusion should we draw about the cross-linguistic applications of the truth predicate? Should we give up the universal concept of truth and embrace the more rigorous concept of true-in-L? This argument is very similar to Quine's rejection of Carnap's concept of analyticity (David 1997)[60]. Carnap's account of analyticity is quite simple: a sentence of a language L is analytic if and only if it is implied by the conjunction of the meaning postulates of L. Given s $\in L$, and P (the conjunction of the meaning postulates of L, by enumeration), s is analytic if and only if it is a logical consequence of P in L. The role played by the meaning postulates is to stipulate the logical relations between some non-logical constants of L. To that extent, sentences such as "all bachelors are unmarried" count as analytic in L.

Quine raised against Carnap's analyticity because the "meaning postulates" strategy does not define the general concept of analyticity, but rather, the more restricted concept of "analytic-in-L", that is to say, referring to one language and the relevant meaning postulates. But, in so far as the concept of analyticity is defined referring to the

60 My discussion of the relation between truth and analyticity in this section fundamentally follows David's one.

language L, it remains undefined for the sentences of the languages L_1, L_2 ... L_n. Therefore, according to Quine, Carnap's approach does not make clearer the notion of analyticity for variable languages (the general or relational notion of analyticity), since the definition of the restricted concepts of analyticity does not enable us to carry out the definition of the general concept of analyticity.

Additionally, Quine's objection is enough general to apply to several semantic notions as well, including reference, satisfaction and truth; that is because no definition of these notions is available for variable languages (including formal languages). The only way we can define analyticity for different languages is by enumeration: given "analytic-in-L_1", we can introduce other languages L_2 or L_n and define additional concepts of analyticity (analytic-in-L_2, analytic-in-L_n). But Tarski's definition of truth is just the most famous example of definition of a semantic notion resting on the enumeration of restricted or indexed predicates and the truth predicate does not differ from the analyticity predicate in that respect. One might claim that "true" and "analytic" work in different ways because "being true" is less obscure than "being analytic", since the schema "p" is true(-in-L) if and only if p provides us with some information about the truth predicate, such that it would be able to justify a unitary perspective on the truth predicate (unlike the analyticity predicate).

But this point is not sufficiently clear. The Convention T loses its explanatory power in so far as the language L in which the truth predicate is defined is the same (or a fragment of) by which the Convention T is expressed (in this case English). Were this not be the case, the Convention T would be meaningless for each case concerning non-English sentences (or false as regards sentences having different meanings in different languages). Given this point, the definition of the truth predicate is threatened by the same objection that concerns the notion of analyticity: Tarski defined true-in-L_1, that is an indexed and restricted notion of truth, in the same way as Carnap defined analytic-in-L_1, an indexed notion of analyticity. Finally, neither the relational notion of analyticity nor the relational notion of truth have been correctly clarified.

Thus, in this section I have showed why, taking for granted the semantic theory of truth, there is no evidence for stating that the different predicates "true-in-L_1", "true-in-L_2", "true-in-L_n" share a principle of unification, namely the general or relational notion of truth.

3. Incommensurability in action

In the last section, I have introduced the connection between the semantic theory of truth and the linguistic considerations that this theory assumes. Of course, the relation between different languages whose meaning postulates differ is not the only linguistic weakness of Tarski's theory. For example he assumes a “Principle of Uniformity” (Andjelković, Williamson 2000) such that the same sentences of L should keep their meaning unchanged within L (“sentences say exactly one thing” or that snow is white is exactly what “snow is white” means); and this principle seems to be particularly implausible, especially as regards the cases of ambiguity (ambiguous sentences do not say exactly one thing), context-dependence (context uses to play a fundamental role in the determination of meaning) and conventionality (sentences do not necessarily say what they say). But, even if these problems concerning Tarski's theory of meaning are particularly urgent, I will not focus on them (see Dutilh Novaes 2008). That is because, for my purpose, I am primarily interested in the problem of the cross-theoretical uses of the truth predicate, since those are the situations where incommensurability comes in. To that extent, incommensurability has been considered a structural non-homogeneity between the linguistic formulations of different theories, such that the structure of one theory cannot be preserved in that of a second and the referential relations invoked by one cannot be mapped into the structure of their counterparts (see for example Chakravartty 1998: 401-402).

This formulation is clear and insightful, but incomplete. If incommensurability were a mere structural discontinuity, it would be easy to demonstrate that the structure of a successful scientific theory can easily be mapped into the structure of more recent (and more successful) theories; and that referential continuity is not an essential desiderata for scientific progress. This is just the strategy adopted by structural realism (both epistemic and ontic) and it has succeeded in defending scientific realism against incommensurability and theoretical discontinuity. So, the aim of my argument is not to defend again this naive version of the incommensurability thesis. I have said that this account of incommensurability is incomplete, since it does not give sufficient consideration to the role that truth play for the incommensurability thesis. The main point is not that different theoretical structures cannot be respectively mapped or that they cannot be rationally compared. The point is that different structures do not allow the truth predicate to properly work in cross-linguistic (or cross-theoretical) contexts (as I have said in Part 2 – section 3). Note that this interpretation assumes a linguistic conception of scientific theories that the semantic view (accepted by most philosophers of science nowadays) absolutely rejects. I will come back to this question in the next sections, showing that the incommensurability thesis does not question the semantic view in itself, but the possibility to introduce a proper truth predicate (from Tarski's viewpoint) for a scientific theory semantically construed. But, at first, let me analyze the argument in depth.

In Kuhn's words the problem goes as follows:

> The semantic conception of truth is regularly epitomized in the example: "Snow is white" is true if and only if snow is white. To apply that conception in the comparison of two theories, one must therefore suppose that their proponents agree about technical equivalents of such matters of fact as whether snow is white. If that supposition were exclusively about objective observation of nature, it would present no insuperable problems, but it involves as well the assumption that the objective observers in question understand "snow is white" in the same way, a matter which may not be obvious if the sentence reads "elements combine in constant proportion by weight". (Kuhn 2000: 161)

The conclusion of this passage is that truth works only in infra-theoretical applications. But the main claim is that incommensurability deals with the difficulties concerning the cross-theoretical applications of the truth predicate (construed following Tarski's semantic theory). If one would like to explain the general predicate "true-in-L" by means of the enumeration of several restricted predicates (true-in-L_1, true-in-L_2 and so on), she should assume a notion of translation between the language referring to which the truth predicate is defined and the meta-language (for example English). In this way, the we would be allowed to explain the relational concept of truth by means of the indexed concepts of truth *via* the concept of translation. But this is not correct, since the restricted truth predicates are not indexed to fragments of the meta-language, but rather, to fragments of arbitrary languages (David 1997: 292-293). According to Tarski, the definition of the truth predicate for a given fragment of language can be achieved by means of the reference and satisfaction clauses for the relevant language:

"…" is satisfied-in-L_1 by ….

"…" refers-in- L_1 to …

In order to understand and fulfill those expressions one has to know how to translate them from L_1 into English; and knowing how to do this does not help us construe the relevant reference and satisfaction clauses for other languages. Thus, again, there is no reason for thinking that the general notion of truth can be properly used.

So, in the end, the argument for incommensurability goes as follows:

> (Premise 1) The truth predicate can be defined only referring to the relevant fragments of L, so that it is, to some extent, language-dependent (for the semantic theory of truth)
>
> (Premise 2) Scientific theories present some structural non-homogeneity at least from a linguistic viewpoint, such that the referential function of some scientific languages cannot be fully mapped into the structure of some rival theories.
>
> (Conclusion) The differences related to the different meaning conditions of rival scientific languages prevent us from adopting a cross-theoretical truth predicate and therefore we should not use the relational or general concept of truth in scientific contexts as well as in ordinary contexts.

Note that, while (Premise 1) is uncontroversial, at least in so far as you take for granted the semantic theory, (Premise 2) is not unquestioned: at best, philosophers of science disagree about the scope and the consequences of this premise. Since here I cannot discuss in depth and at length such an issue, I will adopt the perspective of selective realism. That is to say, I will hold that (Premise 2) is right in highlighting the (sometimes) radical discontinuities between successive and rival theories (since if Kuhn were completely wrong about scientific revolutions, the very motivation of my work would be scrapped), but that (*contra* Kuhn) these discontinuities are not an effective threat to scientific realism, since the cumulative nature of scientific progress is guaranteed by the preservation of the relevant parts of scientific theories. I have already discussed selective realism in Part 1 – section 2. Now, I will not directly focus on scientific realism, but, rather, on the role played by truth in scientific languages. My argument concerns scientific realism only in so far as the adoption of an acceptable theory of truth is considered to be a fundamental part of the realist thesis. For example, according to Psillos (1999) scientific realism consists of a metaphysical thesis (the world is mind-independent), an epistemic thesis (mature and successful scientific theories are approximately true) and a semantic thesis (ST):

(ST) Scientific theories are truth-conditioned descriptions of their intended domain. They should be literally construed as true or false.

Thus, the incommensurability thesis does not directly attack scientific realism. Rather, it does so in so far as scientific realism is 1) based on the preservation-relations between successive theories (for the very concept of preservative realism) and 2) committed to the truth predicate (for ST). That is to say, it implicitly assumes a cross-theoretical use of the truth predicate that is problematic according to (Premise 1), i.e. for Tarski's theory. According to the incommensurability thesis, you can legitimately use the truth predicate in any infra-linguistic or deflationary way (See Kuhn 2000: 99 and Leeds 1978); that is to say, you can use it for logical purposes to express and test the claims of the relevant theory. But you should not use it to express the relation between successive theories and, as a consequence, to argue for scientific realism for a diachronic perspective.

Therefore, in the rest of the chapter, I will show how the best realist theories (especially structural realism) react to (Premise 2), asking whether they can incorporate a proper theory of truth or not. The main point I shall discuss is that the semantic theory insists that sentences are the fundamental truth-bearers, while actually scientific realism rejects the idea that the linguistic formulation of scientific theories is satisfactory. But, at first, let me make some clarifications.

4. Clarifications

Properties and Predicates. The first point to make deals with the theories of truth. Kuhn's attack to truth *via* incommensurability is primarily concerned with the correspondence theory, perhaps because he thinks that scientific realism is necessarily tied to the correspondence theory. But there is no unavoidable relation between realism and correspondence (Devitt 1997 and Vision 2004: 14-16) and his argument does not harm the correspondence theory more than it harms any other theory of truth.

Incommensurability focuses on the cross-linguistic applications of the truth predicate and therefore it is at first a linguistic thesis about the behavior of the truth predicate, not a metaphysical thesis about the essence of the truth property (like the correspondence theory). The correspondence theory claims that the essence of truth consists in the correspondence relation between truth-bearers and the world. On the contrary, the linguistic argument for incommensurability claims that we should define a different truth predicate for each fragment of language. Predicates are linguistic things, while properties are extra-linguistic entities[61]. The predicate "white" is tied to the English language (just like the predicate "blanc" is tied to the French language), while the property "being white" is not, since it should be universal across languages. Basically, an *n*-place, *m*-order predicate is a form such that F(x), where x should be replaced by *n* terms of order *m*; for example MAN(x) is a single place, first order predicate and x is to be replaced by a single individual term. Therefore, MAN(Socrates) is a well formed sentence. On the contrary, an *n*-place, *m*-order property is a metaphysical entity that belongs to *n* entities of order *m*. For example, the property of "being a man" is a one place, first order property and it belongs to single individuals. Thus, even if the relation between semantic theory and correspondence theory is controversial[62], there is no prior connection between incommensurability and correspondence theory.

Putnam on Incommensurability. The claim that incommensurability is linked to the linguistic relations between languages and meta-languages has been already defended by Putnam (1975: 198), so one may ask the difference between my account and Putnam's one. Putnam analyzes the example of a language containing the world "electron" in respect of which we can construe a meta-language to define the concepts of truth and reference (and where "electrons" refers to electrons is a trivial theorem). But rival languages characterized by different meaning postulates relativize truth and reference to their meaning conditions, so that "electrons" refers to electrons only in the sense of L_1. This approach differs from mine in several respects. At first, Putnam refers to languages as a whole, while incommensurability usually deals with specific fragments of languages. It is a local concept that does not involve, for example, the meaning of ordinary sentences like "the snow is white", which can generally be expressed by all languages without special problems. Secondly, Putnam is concerned

61 Even if some properties are language-dependent, for example the property of "belonging to the English language".

62 See for example Fernandez Moreno 2001, Horwich 2005, Kirkham 1992: 170-173, Kunne 2003:208-213, Patterson 2012: 140-143.

with the relativity of truth, since, according to his interpretation of incommensurability, the sentences “electrons exist” or “electron refers to such-and-such things” could be true in L_1 and false in L_2. On the contrary, according to my interpretation, truth is not relative in this sense. It is objective and entirely consistent with the correspondence theory. What is (language)-relative is the possibility to define a truth predicate for a given language or fragment of language, since it depends on the relevant language itself. And finally Putnam's aim is to reaffirm the language-independence of reference and, of course, his causal theory of reference goes in that direction. But, for my purpose, the language-dependence of reference is a secondary problem; I am concerned with the truth predicate and the semantic theory does not force us to commit ourselves to a specific theory of reference. What is important is the notion of translation and it can be defined independently of the concept of co-referential terms. Thus, the language-dependence of reference is not in question, unless one thinks that reference and translation are inter-defined.

5. Incommensurability and structural realism

In this section I will explain how the most plausible forms of selective realism have challenged the incommensurability thesis, concluding that, although they have solved many important puzzles related to the “received view” about incommensurability, they did not defeat the threat I have presented in Section 3. In fact, structural realism has faced the incommensurability thesis especially as regards the problem of referential continuity, which has always been considered one of the most compelling arguments against scientific realism. But my point is that structural realists' referential concerns are mainly gathered from pessimistic induction and, even if their solution applies to it, it does not apply to incommensurability (at least as regards my formulation of the argument).

Roughly, pessimistic induction goes as follows:

1. During the history of science, many mature and successful scientific theory have been proven to be false and their theoretical terms were recognized as non-referential;
2. Actual scientific theories widely accepted by scientific communities are

not essentially different from past discarded theories, so there is no evidence for thinking that they will not be rejected in the future as well.

So, by induction, we conclude that probably our best theories are not referring and that they will be replaced at some time. While standard scientific realism argues for the connection between successful reference and empirical success, pessimistic induction replies that successful reference is not necessary to explain the predictive success of a theory (Laudan 1981).

There are two possible ways to face pessimistic induction from the referential viewpoint. The former consists in adopting a generous theory of reference, aiming to analyze rival theories to identify co-referential terms. From this viewpoint, dephlogisticated air and oxygen refer to the same theoretical entity and, therefore, abandoned terms successfully refer after all, even if they are seemingly non-referential. This strategy has been carried on by Putnam as well as by entity realism, but I will not focus on that. As I have said in the previous sections (especially Part 1 – section 2), its main week point is that it trivializes the concept of reference, since every couple of theoretical terms can be arranged in such a way to be co-referential; for example “ether” and “electromagnetic field”, or “gravity” and “natural place” since it permits a too strong gap between what a term refers to and the relevant theoretical descriptions and intended applications[63].

Thus, let me proceed with the latter and more plausible strategy, which focuses on the idea of selective or preservative confirmation (Stanford 2003a): we should realistically commit ourselves only to those part of theories that play an essential role in the process of empirical confirmation and prediction. To that extent, the parts of scientific theories that had been rejected over the course of the history of science are considered non-essential for the empirical success of the relevant theories. Structural realism is perhaps the most widely accepted form of selective realism. Roughly, it claims that scientific knowledge does not catch the hidden nature of scientific entities; on the contrary, it successfully describes the *structure* of the world with increasing verisimilitude. Structural realism does not care about referential discontinuity between successive theories, since it is not committed to the existence and the essence of the

63 Psillos 1999, Cruse and Papineau 2002, Cumminskey 1992. Roughly, as I have said in Part 1 – section 2, the problem is that the generous theories of reference (especially the causal theory) aim to account for referential success, but fail to account for referential failure.

objects that are supposed to populate the mind-independent world. Even if epistemic and ontic structural realism (see Ladyman 1998) disagree on the nature, scope and structure of scientific knowledge, they agree in deflating the notion of reference: "ESR and OSR both depart from standard scientific realism in rejecting term by term reference of theories, and hence standard referential semantics, and any account of approximate truth based on it" (Ladyman 2011: 97); moreover they both agree in displaying the structural similarities between different theories (rather than their "ontological" disagreements). Henceforth, in so far as structural realism is concerned, scientific realism is not undermined by theory change and referential discontinuity.

For example, take one of the most famous cases of alleged incommensurability between successive theories: the phlogiston case. Ladyman (2011) (following Kitcher 1978) summarizes a series of empirical regularities the phlogiston theory accounts for:

- metal + heat (in air) = calx (metal oxide) + phlogiston (de-oxygenated air)
- calx + charcoal (source of phlogiston) = metal + fixed air
- metal + acid = salt + inflammable air
- metal + water = calx + inflammable air
- water = inflammable air (hydrogen) + dephlogisticated air (oxygen)
- animals phlogisticate air, while plants dephlogisticate it

These regularities are explained by the following hypotheses:

- metal = calx + phlogiston
- charcoal = fixed air + phlogiston
- salt = calx + acid

Finally the above empirical regularities are deducted by means of the categorization of chemical phenomena in phlogistication and dephlogistication reactions (where the inverse relation between them is confirmed by the distinction between reduction and oxygenation reactions). According to Ladyman's interpretation of the chemical revolution, modern chemistry preserves the fundamental identity relation between some chemical reactions (calcination, respiration, combustion), and that these reactions have an inverse; that animals and plants generate opposite processes concerning the changing properties of air and some other points.

The conclusion is that, for structural realism, the referential question is of little interest, since the empirical success of the phlogiston theory is not explained by the co-referentiality of dephlogisticated air and oxygen (even if some tokens of "dephlogisticated air" may refer to "oxygen"), but by the preservation of the real patterns discovered by the phlogiston theory and retained by contemporary chemistry. Structural realism (ontic as well as epistemic) claims that it is arbitrary whether a theoretical term is referring or not. The "ether" theory has not been rejected because ether does not exist, but because it was associated with the idea of absolute space and other claims rejected by Einstein's theory. Therefore, according to structural realism, the referential role of scientific languages is irrelevant to the approximate truth of scientific theories and has nothing to do with the adoption of scientific realism.

This solution is supposed to apply both to pessimistic induction and to the incommensurability thesis. As regards the former, referential discontinuity does not threaten scientific realism anymore, since it has been demonstrated that it is not a necessary condition for scientific progress. As regards the latter, from structural realism's viewpoint, scientific theories are completely commensurable, in so far as the content and the structure of one theory can be mapped into the structure of their rivals. And Ladyman's analysis of the phlogiston theory does succeed in showing that it is commensurable with contemporary chemistry from this perspective. This is sufficient as far as scientific realism is directly concerned, but does not entirely fit with my description of the incommensurability thesis, since I have showed that the indented domain of the incommensurability thesis is not the referential relation, but, rather, the definition of a cross-theoretical truth predicate. Therefore, in the next section, I will wonder whether the deflation of the referential function of scientific languages is consistent with my characterization of the incommensurability thesis; and whether the semantic theory of truth can do without reference.

6. The semantic theory and the concept of interpreted language

In this section I will discuss whether structural realism can avoid the problem of reference without harming the semantic theory of truth. As I have sketcher in Section 3, Tarski's works defined the predicate "true-in-L" for a set of well-behaved object languages. In the same way, the Convention T is expected to establish the conditions according to which a definition of a predicate such as “true-in-L” (in the meta-language) is an adequate definition of true-in-L. Thus, Tarski did not define the concept of truth, but the more restricted and rigorous concept of “true-in-L”; and he provided us with the guidelines to define other analogous predicates. One of the reasons he did so is that he assumed that sentences are the most basic truth-bearers, where, very roughly, by “sentence” we mean any grammatically correct and complete string of expression of a well-behaved language. I have recalled that Tarski exploits grammatical structure in his definition of truth and, obviously, sentences have grammatical structure, while, for example, propositions or statements have not, since they are extra-linguistic things[64]. And this is not an accidental claim, since it is strongly related to the idea that the truth predicate can be defined only referring to *interpreted* languages (including formal languages). For example “we shall also have to specify the language whose sentences we are concerned with; this is necessary if only for the reason that a string of sounds or signs, which is a true or a false sentence but at any rate meaningful sentence in one language, may be a meaningless expression in another” (Tarski 1969: 64. See also Tarski 1935: 263 and Tarski 1944: 342).

According to Tarski we should focus on an interpreted language (L) with fixed meanings, because if one changes the interpretation of the syntactical symbols of L, it will result a different language L'. And since the truth predicate is defined only as regards L (interpreted at a given time *t*), it will remain undefined as regards the new language L'. Finally, a T sentences for L will not count as T sentence for L' anymore and different sentences will be instances of the Convention T. For example, if white referred to a different color (for example red) in L', we should conclude that “the snow is white” is true in L' if and only if the snow is red.

This is quite different from the current work in model theory (for example Raatikainen 2008). Here given a language L, a structure W and a domain D, we should fix the interpretation function I, a mapping from from the non-logical symbols of L to the elements of D. So that an L-structure W is defined as a pair (D, I), consisting of the domain D and the interpretation function I. Since L is a purely syntactical and uninterpreted language, an interpreted language can be defined as a pair (L, I). In model theory the interpretation function I establishes a link between the object language and a

64 See Haack 1978: 79-83 for an introduction to the problem of the truth-bearers referring to Tarski's theory.

domain of extra-linguistic objects and therefore it is a semantic notion, just in the sense that Tarski aims to avoid. In model theory languages are uninterpreted and, when one switches from a model to another, the language will remain the same, even if the interpretation varies. On the contrary, Tarski insists that the languages referring to which we are defining the truth predicate are always already interpreted[65]. The meaning of the terms of L should be already fixed in another way, without recurring to an interpretation function; and, as I have just said, if one changes the interpretation of the non-logical symbols of L, the language will change to another language, and the definition of the truth predicate for L will not obtain anymore. For Tarski, the interpretation of L (and therefore his definition of truth as well as of satisfaction) is established *via* primitive denotation (denotation for individual constants and application for predicate constants), which specifies the translation of the object language in the meta-language (Field 1972). For example, as regards names, assuming that the object language L is a well-behaved sub-set of Italian, denotation for names would work as follows:

Denotes_L (x, y) ↔

[(x = "Italia" & y = Italy)

(x= "Germania" & y = Germany)

….

(x = "Francia" & y = France)]

The same results can be achieved for predicates:

Applies_L (x, y) ↔

[((x = bianco) & white(y))

((x = rosso) & red(y))

…

((x = nero) & black(y))]

One may ask a more substantial account of the notion of denotation, but I will not

65 For some paradoxes related to the purely syntactical view on language see Kunne 2003: 181-182. For example, if languages were individuated by their syntactic properties, the sentence "one billion is one thousand million" would be true in American English and false in British English. Here he gives a different definition of Tarskian sentences: "an orthographically individuated declarative type sentence".

analyze this point in depth. Therefore, let me come back to the main question of this chapter. In the last section I have affirmed that structural realism tries to overcome the incommensurability thesis bypassing the referential function of languages, since it focuses on the idea that we can justify the logical relations between successive theories without recurring to their physical interpretation; where, roughly, by physical interpretation I mean the denotation relations generated by the relevant theories at a given time. Surely, being concerned with structures and models gives many advantages, at first as regards the process of empirical confirmation, since it would be much more difficult to compare linguistic entities like sentences and non-linguistic entities like "facts". And, of course, dismissing the linguistic formulation of scientific theories allows us to overcome the problems related to the meaning of scientific terms, their reference and the troubles with theory change and referential discontinuities. But, in so far as scientific theories aim to tell us something substantive about the world (something true-or-false), one should explicitly state something asserting a relation between some descriptions and some aspects the world. And this requires a linguistic formulation, in terms of models, mathematical formalism and *interpretation* in terms of (at least weak) correspondence (see Chakravartty 2001: 329-330).

Now I shall come back to the physical sciences. Surely, you can think to Newton's law $F = ma$ as a purely mathematical equation, and, to that extent, it is preserved by Einstein's theory of relativity. But, as we think to the second law as well as the other laws of classical mechanics in the context of physical inquiry, all the components of the mathematical patterns (and the variables involved by the laws) should be replaced by their physical intended counterparts (Wójcicki 1994: 350-351). This process is what I have called physical interpretation and arguably it is the most compelling argument against the applicability of Tarski's theory to scientific languages. As I have sketched in Section 3, Tarski's theory of meaning is quite defective and it assumes that scientific languages do not present ambiguities. But the concept of physical interpretation is always "vague" or "fuzzy", since the words often refer to blurred phenomena and this makes reference unstable and open to modifications.

Note that, anyway, it is not sure that the semantic theory is not consistent with an unstable theory of reference (like that I have defended in Part 1 – sections 3,4). As Wójcicki argues, think to the fact that a theory may be interpreted in different ways in its intended models: for example consider how physical objects are treated in classical mechanics. In some applications of classical mechanics, the celestial bodies of the planetary system are considered mass points. But, for instance, in order to explain why Kepler's laws are not satisfied by the orbit of Mercury, one may correctly say that this is because of the oblateness of Sun, but of course, this is not a property that can be possessed by a mass point. This seems to be just the kind of ambiguity that the semantic theory excludes: celestial bodies are treated both as mass point and as extended rigid bodies (and it is not clear under which conditions we are allowed to consider a moving body a mass point). Anyway, one can answer that the truth-values of the sentences of classical mechanics is established separately for each application (Niiniluoto 1999: 101); and, relatively to some applications, the theory may be true strictly speaking, while, relatively to other applications, it may be approximately true to some degree. The possibility to apply a theory (i.e., to interpret its language, giving some physical referents to its terms) necessarily involves pragmatic considerations that, as I have said in the previous parts, cannot be fully expressed by an interpretation function. They are related to the competence of scientific communities, which, basing on the success of the accepted applications, are able to distinguish between proper applications and non-proper ones[66]. Thus, I am inclined to think that this problem is related to Tarski's theory of meaning, rather than to his theory of truth and that the semantic theory is not necessarily inconsistent with a pragmatic process of meaning fixing.

My point is that, resting on Tarski's concept of interpreted language, in order to apply the semantic theory to scientific languages (for example, the language of classical mechanics) one should start assigning physical referents to the terms deployed by the mathematical structure of the theory. You should also establish the intended domain of objects to which such a theory is applicable and the relevant spatio-temporal notions. And this is a puzzle for all theories (like structural realism) that claim that the realist content of scientific theories does not depend on the referential function of scientific language.

Admittedly, French is aware of this problem and acknowledges that, without a proper linguistic formulation, it is meaningless to talk about a Tarski-like definition of truth and therefore, as far as we are not prepared to make some concession to the linguistic perspective, truth remains an unclear notion (Da Costa, French 2003: 33). According to

66 For the problem of the relation between mathematical and applicative content of scientific theories see Stegmüller 1976 as well as the structuralist program in the philosophy of physics that I have introduced in Part 1 – section 4).

French, we can distinguish the problem of the structure of scientific theories from the problem of the truth-value attributions. From the one hand, scientific theories are intrinsically non-linguistics, as the semantic view pointed out, and structural realism is able to account for the relevant cross-theoretical relations from a realist viewpoint. But, from the other hand, truth-value attributions are tied to the linguistic expression of the theory (as well as our epistemic attitudes, which are naturally sentential). One can agree about that, since, as far as I am concerned, I am not questioning structural realists' attitude toward scientific realism; anyway, this does not overcome the incommensurability thesis, since it attacks just the cross-theoretical applications of the truth predicate. Finally, one may also ask whether scientific realism can do without a general concept of truth without turning into something different (taking into account the semantic thesis scientific realists usually hold), but this goes beyond the boarders of my work, whose aim is to demonstrate that incommensurability can survive against contemporary forms of scientific realism.

Ultimately, my point is that, in order to assign truth-values, we need an interpreted language, such that the reference of the terms deployed by physical equations should be assigned at a given time. And, even if Tarski never specified a theory of reference, his semantic theory is not consistent with the idea that non-linguistic scientific theories are true-or-false in any logically proper and significant (preferably correspondentist) way. To that extent, contemporary selective realism should specify how it intends to incorporate a cross-theoretical theory of truth or how it can stay realist without the semantic theory.

7. Conclusions

So, in this chapter, I have pointed out the consequences of language pluralism about scientific truth and proposed a novel argument for incommensurability (even if I am dealing with a form of incommensurability different from Kuhn's or Feyerabend's ones): and finally, I have explained how it is related to the semantic theory of truth. Roughly, for my argument, the incommensurability thesis prevents us from using a cross-theoretical truth predicate, because the semantic theory allows us to define truth predicates (indexed or restricted) tied to specific well-behaved languages. Accordingly, the general or relational truth predicate remains undefined and therefore the cross-theoretical relations between successive or rival theories cannot be justified from a truth-friendly viewpoint. Moreover, I have claimed that the best forms of selective realism (like structural realism) cannot overcome the incommensurability thesis in this respect, since they do not focus on the idea of interpreted language, but, rather on the notions of structure and model, deflating the concept of reference (since they are interested in dismissing its ontological value). On the contrary, taking for granted Tarski's concept of interpreted language, the possibility to apply the semantic theory of truth to scientific languages depends on: 1) the assignation of physical referents to the terms deployed by the mathematical structure of the theory; 2) the relation between a theory and the intended domain of objects to which such a theory is applicable and the relevant spatio-temporal notions. Thus, I have shown that there is a sort of incompatibility between the semantic theory and the non-linguistic view on scientific theories. To that extent, I am not questioning how selective realists approach the problem of scientific realism; I am not denying that the fact that some parts of scientific theories are preserved across theory change is an argument for scientific realism and that we should commit ourselves to those part of scientific theories. But I wonder whether scientific realism can do without a cross-theoretical truth predicate, assuming that, according to preservative realism, our best argument for scientific realism is basically cross-theoretical (how the structure of one theory is mapped into the structure of its counterparts); and that scientific realism usually holds that scientific theories are literally construed as true-or-false (for the semantic thesis). So, my conclusion is that, in so far as structural realism treats scientific theories as set-theoretical structures, they are not true-or-false in any Tarski-like sense. And this constitutes additional evidence for the concept of interpreted structure by concrete applications that I have presented in Part 1 – sections 2-3-4: the point is that mathematical structures are not scientific theories until we provide a physical interpretation, that is a connection between structure and applications.

Section 3: The Inference from Truth to Success and from Success to Truth: what the Deflationist May Say about Scientific Truth

1. Introduction

In the last sections I have defended a minimal account of the correspondence theory, based on Tarski's semantic theory. Here, truth is thought to be a relation between linguistic and non-linguistic entities, but I have not provided a unitary account of the truth-bearers, the truth-makers and the correspondence relation in itself, since I think that they are not necessary conditions for a proper correspondence theory. And I think that this is not a weak point, especially in scientific contexts, where, as I have sketched in the previous parts:

- the correspondence relation can obtain in several ways (according to different methods and styles of reasoning and so on)
- scientific theories express their claims in different ways (models, sentences, axioms, set-theoretical structures and so on)
- there is no agreement about what the world is made of (entities, mathematical structures, properties, physically interpreted structures and so on)
- the identity conditions of scientific theories and languages may change with our inquiry and with time (and actually they vary)

But one can object that, since the account of correspondence truth that I am proposing is really minimal (no metaphysical commitment at all), there is little difference between minimal correspondence and deflationism and therefore it would be worthy to give up correspondence and endorse a correspondence-friendly deflationary theory. And in fact, some philosophers of science have suggested that deflationism will suffice to account for scientific knowledge (Giere 1988, Kuhn 2000).

In this chapter I will discuss how the deflationary theory reacts to some problems concerning scientific knowledge and in particular I will focus on the explanatory role of truth and how it can face the inference from truth to success (and *vice versa*). In fact, according to the correspondence theory, it is intuitively clear that there is a relation between approximate truth and empirical success. In the previous chapters I have discussed this point from several perspectives, but now the problem is whether the concept of truth can play an explanatory role (for example that true beliefs are worthy of being pursued since they are a better guide for action than false beliefs) and, according to the correspondence theory, it can, since it is a substantive relation. On the contrary, the deflationary theory replies that truth is not a property (or a property without essence) and therefore it cannot play any role in explanation; as a consequence, the fact that true beliefs lead to empirical or practical success can be explained without appealing to the truth, but only to facts and questions of local epistemology.

Thus, in this chapter I will challenge this problem. At first, I will introduce the deflationary thesis, without going into the details of the different deflationary theories (minimalism, disquotationalism, prosententialism and so on) and make some clarifications about the intuitive applicability of the deflationary theory to scientific contexts. Then, I will compare the approaches of minimal correspondence and deflationism to the problem of the relation between empirical success and truth. And, finally, I will conclude with some objections to the purely deflationary perspective, which may account for truth-talk in scientific contexts at the low level of individual sentences (even though it does it in an elaborate and non-economic way), but is problematic as regards how the truth works referring to whole theories.

2. Deflationism and the world

2.1 What is a deflationary theory?

It is hard to characterize the deflationary theory in a positive way, since it is usually introduced by means of a distinction between deflationary and inflationary theories; that is to say, by showing what the deflationary theory has to say against the traditional theories (correspondence, coherence, pragmatism). In contrast to the "received view", deflationism holds that truth is not a property (or a property with no nature or essence); but this characterization is too wide, since it would include all kinds of nominalism (since nominalism claims that all properties have no metaphysical essence, including truth)[67]. More precisely, we may say that what is claimed is that there is no substantial

67 And perhaps it is too narrow at the same time, since some deflationary theories, such as Horwich's minimalism and Field's disquotationalism, do not claim that truth is not a property, even if it is a "deflated" one.

property of truth or an underlying nature to truth of the kind that philosophers might create theories about. Basically, there are two kinds of questions about truth that deflationism and inflationism answer in different ways:

- (Analysis) What is truth?
- (Conceptual role) What is the conceptual (and linguistic) role of truth?
  (Armour-Garb and Beall 2005: 2)

The first question appeals to the analysis of truth, something that must have the following form:

x is true if and only if *x* is T

Unlike the inflationary theories, deflationism does not take the analysis problem seriously and focuses only on the conceptual and explanatory role that the truth predicate plays in our talk, language and thought.

Even though deflationism has received several formulations, generally it rests on the Equivalence Schema (ES):

(ES) "*p*" is true if and only if *p*

Where "..." are a kind of appropriate name-forming device (and can be replaced, for example, by "the proposition that …") and "p" is replaced by sentences to provide instances of the schema. (ES) is a fundamental assumption of both inflationary and deflationary theories, but the key claim of the deflationary theories is that (ES) is fundamental, that is to say that the instances of the schema are exhaustive of everything significant we can say about truth. This is what I meant suggesting that deflationism dismisses the question about the analysis of truth: while inflationary theories hold, for instance, that a proposition is true if and only if it corresponds to the facts, deflationary theories does not provide an explicit definition of truth. For example, Horwich says that his minimalist theory does not offer any explicit definition of the concept of truth (Horwich 2001: 567), but this does not imply that the meaning of the word "true" cannot be fully grasped by a competent speaker. In fact, deflationism defines the concept of "having the concept of truth": someone has the concept of truth if he is disposed to accept all the non-controversial instances of (ES), that is all the non-paradoxical sentences having the form "*p*" is true if and only if *p*.

According to deflationism, (ES) is both (1) conceptually fundamental and (2) explanatorily fundamental. (1) means that the instances of (ES) do not follow from a definition of truth in more basic concepts by which truth can be defined (like correspondence, coherence or warranted assertibility). (2) means that there is no possible unifying account of why the biconditionals hold and that the instances of (ES) are fundamental explainers of truth-talk. For example, the deflationary truth predicate explains the use of indirect assertion sentences such as "what Tarski said is true" in terms of endorsing functions; or the use of logical laws in the form of sentences such as "every sentence of the form "everything is φ or not φ" is true"[68].

Since deflationary and inflationary theories strongly disagree about the very possibility to analyze the concept of truth, a fruitful way to approach the problem is to give up the question about what truth is and ask what we can *do* with the truth predicate, or how truth is explanatorily related to other concepts, such as meaning, belief, truth-conditions, assertion, success, goal of inquiry. In the next part I will discuss how deflationary and correspondendist theories face the problem of the relation between truth and empirical success of scientific sentences; but, firstly, I shall make some clarifications about the applicability of deflationism to scientific knowledge.

2.2 Deflationism and empirical claims

By now I have assumed a minimal correspondence intuition, without appealing to metaphysical notions such as proposition and fact, such that, if something is true, there is something that makes it true. This seems to be particularly intuitive at least in the contexts where truth is immediately related to factual or empirical questions, and scientific knowledge seems to fit well with this account. Moreover, several philosophers take the correspondence intuition to be a fundamental truism that applies not only to the correspondence theory, but to any satisfactory theory of truth (even though you are not building your theory of truth around this intuition). For example, according to Lynch the objectivity platitude is logically related to (ES) and claims that:

68 For some objections to the deflationary perspective on indirect assertion, endorsing functions and generalizations see Gupta 1993, Soames 1999, Armour-Garb 2004.

- (OP) The belief that p is true if, and only if, with respect to the belief that p, things are as they are believed to be. (Lynch 2009: 8)

The problem with the deflationary theory is that many philosophers maintain that it cannot accommodate the correspondence intuition (Alston 1996, Fumerton 1995, Kirkham 1992). Anyway, some followers of the deflationary theory does not deny that the concept of truth is intuitively related to how things are. For example, Horwich claims that, from the one hand, deflationism does not explain what truth is and therefore it does not provide a positive answer as truth is correspondence between propositions and facts; but, from the other hand, it does not deny that truth corresponds, in some sense, to the facts. On the contrary, he does say that “statements *owe* their truth to the nature of reality” (Horwich 1990: 105, italics mine). And, additionally, his deflationary minimalism does not reject the idea that concepts such as truth, satisfaction, reference and so on are inter-defined. Horwich's minimalism is correspondence-friendly (or representation-friendly in Marino 2010: 222) as far as it tries to accommodate the correspondence intuition without appealing to the metaphysical essence of truth; in other words, we shall assume that the correspondence theory consists of two main claims: 1) that true statements correspond to portions of reality; 2) that such a correspondence is what truth essentially is. Correspondence-friendly delflationism endorses the first claim, but rejects the second and accounts for the concept of truth (as well as the concepts of correspondence, satisfaction, reference, representation) from a non-essentialist standpoint. Thus, it acknowledges that truth-values depend on how things are and that propositions are true or false because the world is in a certain way and, to that extent, propositions are true because they correspond to the world. But it denies that those relations are constitutive of truth and that they are what truth consists in. This is because the deflationary perspective explains the correspondence intuition basing on (ES), and therefore the instances of (ES) are taken to be more fundamental than the correspondence relation. But, I will come back to the explanatory role of the instances of (ES) in the next parts of this chapter.

So, it would be wrong to say that deflationism is not applicable to scientific knowledge because it is tied to a non-realist metaphysics: unlike the epistemic theories, it does not make the truth mind-dependent. Anti-realists accounts of truth usually tie truth to an epistemic or potentially epistemic state; for example some theories identify truth with what an ideal observer would know or warranted assertibility. Correspondence and deflationism agree in rejecting this approach to truth. Moreover, they are both consistent with cognition-independence, that is one of our most basic intuitions about truth: “nothing in the account of truth itself indicates that truth is, ever will be, or can be entertained by minds of roughly our capacity” (Vision 2004: 38). Of course, correspondence and deflationism are non-epistemic for different reasons: the correspondence theory because it claims that the essence of truth is not related to the way we know it; the deflationary theory because it claims that there is nothing much to be said about truth (other than the instances of ES), it does not make difference whether epistemic or non-epistemic. But, anyway, the point is that deflationism is consistent with (OP), whatever is its logical relation with (ES). And therefore that the intuitive argument against the epistemic theories (that truth depends on how things are and not on how we think or wish them to be) is not automatically an argument for the correspondence theory (Williams 1986: 224. See also Field 1994: 265). Finally, as far as weak correspondence theories are based on the idea that truth depends on non-linguistic facts, deflationism *is* a weak correspondence theory (Patterson 2003: 425), but, as I will explain later, this is far to week a notion of correspondence to be of interest. So, in the end, a correspondence-friendly deflationary theory of the kind suggested, for example, by Horwich or Quine is the view that the right theory of truth equals the correspondence theory minus metaphysical entities like propositions, facts and state of affairs and semantic relations such as representation and correspondence (David 1994: 53).

Finally, in this section, after roughly introducing the deflationary theory, I have explained why it can be applied to scientific knowledge and scientific sentences without violating our most basic intuitions about truth (and scientific truth in particular). In the next sections, I will focus on how correspondence and deflationism disagree about the explanatory power of (ES).

3. The explanatory role of the truth predicate

In the last section, I have said that inflationary and deflationary theories approach in different ways the problem of the analysis of truth: deflationism refuses to take this problem seriously, since it claims that there is nothing to say about the essence or the nature of truth. For this reason, it is not fruitful to compare deflationism and correspondence on what they say (or refuse to say) about what truth is. On the contrary, a more straightforward way to face the problem is to focus on what they claim about the explanatory role of the truth predicate. As I have said in section 2, there are basically two questions about truth. The former concerns the nature of truth: inflationary theories maintain that there is something common and peculiar to true statements, for example that a statement is true in virtue of some sort of correspondence with some parts of the world and are committed to a description of this relation in naturalistic terms (appealing to reference or satisfaction and so on); and I have just said that deflationary theories reject any such reductive explanation of truth, since truth has no nature that is susceptible of scientific analysis. Instead, the second question is concerned with the causal and explanatory role of truth.

Typically, realists attribute to the truth property some important explanatory role, for example, to explain the success of science or the fact that people who hold true beliefs are generally more successful in meeting their goals; or to explain meaning in terms of truth-conditions, where, obviously, meaning, in turn, plays an explanatory role in the explanation of human behaviors. On the contrary, deflationism usually rejects that the truth predicate can be used as explanation, even if, obviously, this does not mean that the deflationist cannot use the truth predicate in explanatory sentences. For example, the appeal to (ES) explains why a sentence “p because it is true that q” is equivalent to the sentence “p because q”, but this does not mean that the truth is the explanation of p. Or, in other words, consider the example “I am wet because it is raining”: following (ES), this sentence can be rewritten as “I am wet because it is true that it is raining”. In a very trivial way, the explanatory or causal role is played by the rain, not by the truth. In some cases, like those I have sketched in section 2 (blind ascriptions, generalizations), the linguistic role of the truth predicate is essential to express statements, but, anyway, this does not make the truth metaphysically explanatory. The deflationist must convince us to give up truth as explanation, because, if the property P is part of a significant and informative explanation of the phenomenon Q, this is sufficient for P to be a real and distinct property; and the main claim of the deflationary theory is that truth is not a real property. So the main champions of the deflationary theory adopt this perspective on the distinction between inflationary and deflationary properties (Horwich 1990: 38, Field 2001: 29): that this dichotomy is an argument over whether or not truth is a causal or explanatory property[69].

This seems to fit well with scientific inquiry, where it appears intuitively clear that the explanatory role is played by facts. For example, in some works, Hacking agrees with the deflationist on the lack of explanatory role of the truth predicate in these contexts[70]. He says that we would extend to truth what Kant said about existence: that it is not a predicate, in the sense that it does not add anything to the subject (Hacking 2002: 192). For example, I may believe that there was a solar eclipse this summer because there was one in the place where I was staying. Here, referring to my belief, the explanatory role is played by the solar eclipse, along my experience, my memory, my background knowledge and so on. But the truth of the proposition expressing the fact that there was a solar eclipse this summer is not part of the explanation; or if so, it is explanatory only in a secondary sense, as a consequence of the eclipse itself, since the sentence "there was a solar eclipse this summer" is equivalent to "it is true that there was a solar eclipse this summer" according to the deflationary interpretation of (ES). Consider another example (in Levin 1984). You can explain why airplanes stay up by appealing to

- "The pressure on the underside of a moving airfoil is greater than the pressure on the overside" is true

or to

- The pressure on the underside of a moving airfoil is greater than the pressure on the overside

Of course, what plays the effective explanatory role is the latter statement; better, the former sentence is also explanatory, but the explanatory work is done in a disquotational way, within the quotes. In other words, here explanation appealing to the truth is achieved by (implicit) disquotation of the statement mentioned and therefore, the

69 Actually, it is not entirely clear what an explanatory property is thought to be; anyway, a possible definition is that, assuming P as an explanatory property, P should figure in effective explanations, except those in which it is a mere logical device.

70 Hacking has developed several (and sometimes contradictory) ideas about truth, but he never really endorsed deflationism. As I have already said, in his last works he seems to adopt some kind of pluralist theory (Hacking 2012).

empirical success of the theory is explained by the theory itself, and no particular explanandum of the success of a theory is required.

Now I will focus on how the deflationist aims to explain these connections between facts and true sentences.

3.1 Horwich's Account

Therefore, I will introduce some perspectives on how the deflationist can account for the correspondence intuition underlying science appealing only to (ES) and excluding any metaphysical implication. I have already said that Horwich does not deny that the truth of a sentence depends on how things are. More precisely, he says that whenever a proposition is true, it is true *because* something in the world (something external to the proposition) is in a certain way (Horwich 1990: 105). For example "*p*'s being true is explained by snow's being white" is taken to be equivalent to "p is true *because* snow is white".

A preliminary problem with his account is that, if we take "p is true because the snow is white", where p is to be replaced by "the proposition that the snow is white", in conjunction with the deflationary theory, that is with the instances of (ES), it will follow something naturally false. In fact, from "p is true because the snow is white" and "p is true if and only if the snow is white", it follows that "the snow is white because the snow is white"; and this is undoubtedly false, since the explanatory relation expressed by "because" can obtain only referring to distinct *relata* and this is not the case. Of course, the problem is that (ES) is a biconditional, while causation is irreflexive, asymmetric and transitive. But the deflationist can reply by arranging the correspondence intuition in different ways, or by saying that the word "because" creates an opaque context, where we are not allowed to substitute co-referential expressions preserving truth (Stoljar, Damjanovic 2010). Anyway, since I think that there is something more radically wrong here, I will proceed with the core of Horwich's argument. Let us consider again that "the proposition that the snow is white is true because the snow is white". According to Horwich, when we analyze the explanation relation between phenomena, we naturally give explanatory priority to "the laws of physics and the initial conditions of the universe" (Horwich 1990: 105). Appealing to those ultimate things, we can figure out the phenomenon that we aim to explain, for example that the snow is white, and then, basing on (ES), we are allowed to conclude that the snow is white is true, since it is equivalent to “the snow is white”. Since “the snow is white” has been proven to be derivative from the laws of physics and the initial conditions of the universe, it is explanatory prior to “the snow is white is true” and this explains that the snow is white is true because the snow is white, without appealing to the truth property and any semantic notion. The argument tries to ground the correspondence intuition in the concept of causation and basically goes as follows:

p is made T by x = p is T because of x
= p is T because x exists
= x exists and there is an explanatory deduction from “x exists” to “p is T”

Thus, assuming that T is the property of being true and p is a well-formed sentence, it will follow that

p is made true by x = p is true because of x
= p is true because x exists
= x exists and there is an explanatory deduction from “x exists” to “p is true”

(Horwich 2009: 191)

There are several problems with this argument (McGrath 2003 and Wright 1992: 27). The most important is that it implicitly assumes that the concept of explanation is necessarily tied to the recognition of (and the derivation from) some laws of nature. At first, for the sake of the argument, we can leave aside the fact that in ordinary contexts the relation between two or more brute facts can constitute an acceptable explanation in itself; or that mathematical, philosophical and accidental truths cannot follow from laws of nature and the initial conditions of the universe. I am dealing with the context of scientific inquiry and therefore, we can provisionally assume a view based on laws and conditions. But the point is that this view does not fit even with the empirical sciences. Firstly, it is concerned with laws or small collection of laws, but we cannot imagine how to present biology or psychology as clusters of laws; and actually, it is not clear whether those sciences contain any effective natural laws. And moreover a reductionist view aiming to derive those sciences from physics or chemistry is problematic as well. In fact some concepts employed by biology cannot be defined in the terms of the sciences projected as reducing it: for example the concept of gene cannot actually be analyzed in physicochemical terms. And anyway, even though physics is usually based on natural laws, not all physical explanations appeal to natural laws and, in the end, scientific explanation is not always a matter of pointing to laws[71].

Thus, since my conclusion is that Horwich's account is not sufficient to explain the complex relations between theories and reality obtaining in scientific contexts, in the next section I will present a more plausible deflationary account of the role played by truth in the empirical field.

4. Leeds' account

4.1 Disquotationalism and the success argument

Stephen Leeds' account (Leeds 1978, Leeds 1995, Leeds 2007) of the deflationary applications of the truth predicate in scientific contexts is also based on the idea that the truth predicate does not play any explanatory role concerning the correspondence intuition. His starting point is the success argument, one of the most compelling arguments for the explanatory power of the correspondence relation and therefore for inflationary correspondence. The core of the argument is quite similar to the no-miracle argument for scientific realism and it claims that we cannot justify the success of our theorizing about the world without appealing the correspondence relation; explaining the success of our theorizing is an explanatory role and therefore we should accept the correspondence theory rather than deflationism.

Let us start with a trivial example. Assume that F is a football player, who succeeds in shooting a penalty kick in the best way for him to do this, given that his aim is to score a goal. How can we explain his success? A good explanation would be some like this:

- F wants to score a goal

71 For a perspective on scientific explanation that does not depend on the idea of law see Kitcher 2001.

- F believes that he will score a goal by shooting in the opposite corner after the goalkeeper dives
- F is able to figure out when the goalkeeper will dive

According to Field's interpretation of the success argument (Field 1986), this explanation assumes an implicit explanatory use of the truth predicate, concerning for example the truth-conditions of the explaining sentences. For instance, the sentence “F believes that he will score a goal by shooting in the opposite corner after the goalkeeper dives” has the truth-conditions that “he will score a goal by shooting in the opposite corner after the goalkeeper dives” and this expresses a connection between sentences and worldly conditions, just in the sense that the deflationist aims to rule out.

The deflationist would reply to this example saying that F, in his language, is able to express sentences like G, D and S, such that F aims to score a goal (G), he tends to believe something about how the goalkeeper will dive (D) and he tends to succeed in shooting in the corner when he intends to do this (S). In this way, what is needed to explain the success of F is that F holds a belief of the form G#(D#S), where # is an inference equivalent to “if and only if” (Leeds 1995: 17); and in such an explanation the world “truth” is not used at all.

So the main claim of the deflationary account is that the analysis of the causal connection between sentences and reality is a sufficient explanation of our trust in true sentences and our tendency to act following true beliefs; and that reference and truth are not relations between language and the domain of extra-linguistic things, but, rather, they obtain only in a merely formal or logical way. Now consider a more informative example, from the field of the empirical sciences:

> To take the case of viruses as an example, we can explain how the causal relations came to hold by tracing the history of research into viruses. Such a history – already available in the local library – will show us, among other things, how it came about that, at a certain point, the causal connections between “virus” and viruses were fairly firmly set up: so that from that point onward it was nearly guaranteed, given our theory of viruses, and given our inductive procedures – and given also how viruses actually work – that the new beliefs about viruses that won general acceptance would tent to be T-true [that is to say true under the interpretation that assigns “virus” to viruses and “dog” to dogs and so on].
> (Leeds 1995: 10)

In this way the deflationist can tell a story on how a naturalistic analysis of the words-world relations can account for the correspondence intuition and on how these causal chains are related to the success and the reliability of our theories; all these tasks are fulfilled without recurring to a general theory of truth and denotation. But this is not necessarily an argument against the correspondence theory, at least in the minimal or weak variant that I have defended; for example Kitcher's minimal correspondence theory does not appeal to anything like a general theory of truth or denotation (Kitcher 2002: 347). So I will go into the details of Leeds' account to spell out the core of his argument.

4.2 The map analogy

Coming back to the example of the penalty, we are in front of two possible explanations of the behavior of F. Speaking in terms of propositions, we can say either that the success of F is explained by the proposition that he believes is true or that his success is explained by the proposition expressed by F's belief. The former interpretation proposes an explanatory linkage between a belief's being true and something extra-linguistic; and this is an invitation for further questions concerning what makes this relation satisfied and for a theory of truth in which truth plays an explanatory role. On the contrary, the latter interpretation claims that there is no deep explanatory relation; rather, the explanatory relation is between the propositions that F believes (about how to shoot and how the goalkeeper will act) and the proposition that a certain kind of shot tends to work better than others. And this kind of relation has to be justified by appealing to the game of football, not to the semantic relations between the words and the world. The latter interpretation is adopted by the deflationist, and his explanation consists of two steps: 1) explaining why F believes in a given action-guiding sentence; 2) explaining why those action-guiding sentences are true.

So, the deflationist claim is that several questions where the correspondence theory sees robust semantic relations are actually problems of local epistemology. Where the correspondentist says that F scores a goal because many of his relevant beliefs are true (where this involves a referential relation between his beliefs and the world), the deflationist believes that he succeeds because he tends to belief that the goalkeeper will dive on his left when and only when the goalkeeper will dive on his left.

Leeds' example (in reply to Kitcher) of the map of Venice is a typical example of how deflationism aims to replace truth and reference with questions of local epistemology:

> [Kitcher] has no trouble arguing that their [deflationary] alternative explanation leave out the fact that our map of the world, being a largely correct one, is endlessly and systematically versatile. But I do not see why a deflationist should need to eliminate all use of "true" in explanations, any more than someone who thinks there is no general, systematic account of how people come to have good maps of Venice needs to show how we can replace every explanation of the form "I looked on the map, and I saw that if I followed this street I would land in a canal" with one that does not make implicit reference to the particular projection under which the map resembles Venice. (Leeds 2007: 11)

Basically, this example means that we can plausibly say that the map guides the tourists well because it is accurate, without affirming anything general about the representational relation between the map and the world, for example that the map guides us well because it corresponds to the world. Thus, Leeds poses a parallel between maps and scientific theories: like maps, theories allow us to make successful predictions and to fruitfully interact with the world, without saying anything general about the relationship between theory and reality.

In this way, the deflationist tends to reject the semantic concept of reference and use the concept of "indications relations" (Maddy 2007: 153-156; but also Field 1994); roughly, the "indications relations" of a sentence p are the worldly conditions associated with the speakers believing p. Wilson (1994) suggests to consider the example of a rainbow: there seems to be a tension between the fact that the sentence "there is a rainbow" is true if and only if the angle of refraction of light between my location and that spot is just so as to produce a prism effect and the deflationist claim that the truth-talk related to this sentence (including truth-conditions) is exhausted by the biconditional ""there is a rainbow over there" is true if and only if there is a rainbow over there". The indications relations relevant to the sentence "there is a rainbow" are that "there is a certain angle of refraction between water droplets and me"; from (ES), we know that "there is a rainbow over there" is true if and only if there is a rainbow over there; from optics, we know that "there is a rainbow over there" is true if and only if the angle of refraction of light between my location and that spot is just so as to produce a prism effect. And, putting these biconditionals together, we will come back to the intuitive explanation "there is a rainbow over there if and only if the angle of refraction is such-and-such".

This is how the deflationary (and, for Leeds, disquotationalist) strategy dismisses the general concept of truth and tries to account for the correspondence intuition in terms of problems of local epistemology. Here the complexities of the words-world relations do not threaten deflationism, since the indications relations approach concerns only the features of individual cases, not global problems on truth and reference.

5. Problems for the deflationary strategy

In the last section I have presented a plausible deflationary explanation of the success argument that appeals only to the instances of (ES) and denies that the truth predicate has any explanatory power. Now, I will make some objections against this kind of explanation.

5.1 Is truth really non-causal?

The first objection against the deflationary strategy has been developed in (Damjanovic 2005): his argument rests on the distinction between causal properties and efficacious properties. Suppose that A suddenly falls asleep because he ingested a sleeping pill; thus we may intuitively say that A is sleeping because he has ingested something that has the property of being a sleeping pill. But another point is which is the efficacious property causing A falling asleep; in fact, it seems that what is doing the causing is that A has ingested something that has a specific chemical composition (and therefore something that has the property of having a specific chemical composition). Being a sleeping pill is not an efficacious property, but it is an explanatory property, since we legitimately use it in explanations about why A is sleeping. Causal or explanatory properties are either efficacious or causally relevant and the deflationist has showed that the truth property is not efficacious. But the fact that the explanation appealing to the property T can be replaced by an explanation that does not appeal to T does not entail that T is not explanatory. Thus, the first objection is that, even though perhaps the truth property is not an efficacious property, it may well be a causal or explanatory property.

5.2 Truth or explanation?

The second argument concerns the relation between truth and explanation in the deflationary strategy. In its application to scientific theories, the deflationist would say that scientific research provides us with reasons for believing some theories and that these are good reasons; and, in the end, resting on (ES), we have good reasons for believing them true as well. So, roughly, the argument goes as follows: 1) we give reasons for believing A; 2) if they are good reasons, we can believe A; 3) therefore, we can believe that A is true. The problem with this argument is that it is not showing that A is true, but, rather that we have good reasons for believing A. All that it points out is that we can explain some sentences by means of scientific explanation and that (ES) can transfer that explanation into an explanation of why the relevant sentence is true. And, of course, this is not to explain why the snow is white in terms of snow's being white, but, rather, to explain why we are allowed to believe that the snow is white in terms of what scientific communities find persuasive and well-confirmed[72]. So, in the end, the objection is that this argument explains why we think that it is true that the snow is white, but it does not explain why we use to infer from “it is true that snow is white” that “the snow is white” without requiring any further explanation (see also Caputo 2010: 27). On the contrary, the task of the correspondence intuition is to provide an account of the explanatory relation ““snow is white” is true because snow is white” and this relation should obtain even if no physical explanation is available.

5.3 Imperfect maps

The third objection has been presented in (Marino 2010) and it is the most compelling one. Let us come back to Leeds' analogy between scientific theories and maps. The point is that a good map successfully guides our action and allows us to make predictions about the relevant portions of reality; but this does not mean that we should create a general theory of the relations between maps and reality, since the successful applications of the map can be justified at a local level. But Marino (2010: 226) suggests to consider a similar example. Imagine that you have a perfect map, that is a perfect representation of the field it aims to represent or that the map and the field

72 A similar argument in (Wright 1992: 27), referring to Horwich's argument that I have discussed in section 3.1 of this chapter.

are identical. The deflationist succeed in analyzing this case in terms of local relations, since he can characterize this perfect map as follows: for any locations x and y, x stands on the relation K to x on the map if and only if x stands in the relation K to y on the field. To express this relation we do not need anything like a general theory of how the map represents the world and its accuracy.

But now imagine to have an imperfect map, that is more accurate in some respects than in others; such a map uses different methods of projection in some parts and, referring to some others, it is merely wrong. In this example, the analysis of the accuracy of the map is far more complex and involves the complexities of the relation between words and the world that the deflationist aims to rule out. In fact, if the map is guiding us well, this constitutes evidence for thinking that the map "corresponds" to the field it is mapping (the global concept of truth rejected by the deflationist) and we can derive conclusions about the causal chains that made this possible. On the contrary, when the map misleads us, we should analyze the portions of reality and parts of the map, to find out mistakes that we had not noticed (and this task can be achieved by the local deflationary perspective).

The weak point of the deflationary argument is just that scientific theories (as well as languages) work like imperfect maps. Scientific theories are true strictly speaking referring to some applications; approximately true to some degree relatively to other applications; and merely false to some others. And in this context, the weak correspondence theory offers a better account of the deflationary one, since it claims that, in investigating the complexities of the world-words relations the map analogy is no longer accurate and sometimes we have to go beyond mere lists.

6. Global and local, upward and downward

In the last sections I have presented some objections against the deflationary explanation of the success argument and the correspondence intuition; in my view the decisive objection is the third, especially because it is the only one directly concerned with scientific knowledge. As I have stated in the last chapter, the phlogiston theory is a case of successful theory that does not give us a perfect representation of the world. In response to this case, the deflationist may say that the truth-conditions of "this air is dephlogisticated" are that "this air is dephlogisticated" and the indications relations that “this air is rich of oxygen” (Maddy 2007: 155); and this is supposed to explain why scientists were successful, even though they were substantially wrong. In the previous chapters, I have explained several times why this interpretation of the relation between theories is wrong. Now the point is that the weak correspondence theory entails a more plausible answer to the phlogiston challenge. Following the spirit of selective realism, the weak correspondence theory may say that, even if the sentences of the phlogiston theory are themselves false, the theory itself is approximately (and partly) true. We can admit that our map is imperfect, but, anyway, it corresponds to the world to some degree (even if I do not think so as regards the phlogiston theory, as I have claimed in Part 1).

So, in the end, there is no doubt that the direct argument from success to truth is not tenable: too many successful scientific theories were proven to be false[73]. But, unlike deflationism, weak correspondence allows us to recognize the fact that not only the truth of a theory, but also its partial truth can explain empirical success. The basic idea (the same idea underlying selective realism) is that, given a scientific theory T, some claims of T are true, other claims are false, but the explanation of the success of T is related to the truths it contains. And in the previous chapters I have explained how actually scientific realism deals with the parts of scientific theories we should consider true strictly speaking. But you can do this only adopting a (weak) correspondence theory, since the deflationary theory is not able to account for those parts of scientific theories that are considered essential for their empirical success (because it requires a global perspective). Finally, the argument for the relation between success and truth goes as follows:

- *S* plays a crucial role in a systematic practice of fine-grained prediction and intervention
- *S* is approximately true

(Kitcher 2002)

But this is not a direct argument since we can know whether S plays some crucial role in empirically successful practice only basing on the preservation relation between successive theories.

In his classic (Laudan 1981), Laudan introduces a distinction between “Downward

73 See the debate about the No Miracle Argument, Pessimistic Induction and Inference to the Best Explanation.

Path” (inference from truth to empirical success) and “Upward Path” (inference from empirical success to truth). Of course, none of these strategies is viable, since they both assume a necessary connection between success and truth. But the weak correspondence theory, in conjunction with the adoption of a proper form of selective realism[74], can overcome this distinction posing an indirect relation between truth and success, based on a global perspective which allows us to deal with scientific theories as a whole and to select the most empirically interesting parts.

7. Conclusions

In this chapter I have discussed how the deflationist aims to analyze the correspondence intuition in the terms of his theory of truth, and especially how she faces the relation between truth and empirical success, where truth is usually supposed to play an explanatory role, which is not consistent with the deflationary thesis. I have introduced Horwich's (minimalism) and Leeds' (disquotationalism) accounts of the correspondence intuition and I have concluded that, even though, at first sight, they seem to apply to the local level of individual scientific sentences, they have several problems. The most important is that the deflationist cannot explain why scientific theories are partly true, or, more precisely, they are true to some applications, approximately true to some others and false referring to others yet. Therefore we should not replace the concept of (approximate) truth with the concept of accuracy combined with (ES). We need a correspondence theory to explain how the relations between scientific theories and portions of reality obtain at a global level; but, at the same time, we need a *weak* correspondence theory, which is not committed to a one-to-one relation between theories and facts and therefore to a direct inference from truth to success (or the other way round). The relation between truth and success obtains only by means of the selection of the parts of scientific theories that are responsible for their empirical success.

74 This is not thought to be an argument for scientific realism, but only for weak correspondence, since it already assumes a realist commitment and therefore, it would be a circular argument.

Section 4 and Conclusion: Weak Correspondence. Obections, Replies and Open Questions

1. Introduction

In this last chapter, it is time to take the stock of what I have said about correspondence truth (and its interrelated concepts) and to reply to some objections concerning the theory of truth I have presented and some relevant problems. I will start summarizing the main requirements and claims of weak correspondence; then, I will provide a practical application about how weak correspondence applies to scientific knowledge as a consequence of Tarki's semantic theory; and, finally, I will reply to some objections and present some open questions that I have not dealt with in this work.

2. Summarizing weak correspondence

Roughly, a tenable correspondence theory should fulfill four requirements (Marino 2006):

1. Correspondence Platitude: true sentences, statements or beliefs correspond to the way things are in the world.
2. Cleavage: there is a gap between a sentence and the fact it expresses.
3. Propertyhood: truth is a property, i.e. there is something that all true propositions have in common (at least in the empirical field[75]).
4. Content-Implication: true sentences, if not true in virtue of their logical form, are true in virtue of something.

Unlike traditional correspondence theories, minimal correspondence is not committed to another classic *desiteratum* of correspondence truth:

- Independence-Congruence: true sentences mirror bits of raw, unconceptualized, mind-independent reality.

75 My discussion is restricted to how truth works in scientific contexts; I have not analyzed or taken a stand on what is moral truth or logical truth.

I have discussed in Part 2 – Section 4 – why the correspondence theory is not necessarily related to the notion of comparison-mirroring. Even if truth is correspondence between content and the world (for content-implication), often our theories work in such a way the correspondence relation is not direct, but, rather, indirect (Horgan, Timmons 2002) (as in referential failure); there is no one-to-one correspondence between names and objects or predicates and properties. For example the sentence "the snow is white" does not entail that there is some genuine object answering to the name "the snow" and a genuine property expressed by the predicate "is white".

But, at the same time, I have specified that this does not mean that the referential relations between scientific theories and the world are empty and can be satisfied by any kind of things. I have given the example of ontic structural realism (Part 3 – Section 2), according to which reference obtains referring to uninterpreted structures, and I have concluded that this is not sufficient for a proper correspondence theory, since truth-value attributions require interpreted languages and interpreted structures. On the other hand, since Part 1 – Section 2, I have assumed that purely structural homogeneity is not a sufficient condition for the cumulative nature of scientific progress, since it implies the instantiation of properties of entities, individuated by theoretical descriptions. Minimal correspondence meets those conditions since it does not aim to dismiss reference and claims that (Kitcher 2002):

- names range over and refer to real entities
- variables range over and refer to real entities
- predicates denote sets of these entities
- truth-values can be determined in a "Tarskian" style

According to weak correspondence, there are no specific entities and facts to which the theory refers and, above all, no unitary account of the notions of reference, denotation and even truth is required.

3. A concrete application

In this section, following (Wójcicki 1995), I will present a practical application that will show how weak correspondence can be applied to scientific knowledge, resting on the semantic theory of truth. He suggests to consider a sentence, taken from the *Encyclopedia Britannica*, concerning the measure, the position and the shape of the Sun:

- The Sun is a sphere of luminous gas 1,392 × 10^6 m in diameter. It is the star nearest the Earth, lying at an average distance of 149,6 × 10^8 m. The Sun converts five million tons of matter into energy every second. It is not expected to undergo any dramatic change for the next 5 billion years, when it will expand into a red giant star.

This elementary and informal description appeals to notions to be defined by advanced theories, like Euclidean geometry (sphere, diameter, distance) or modern physics (gas, luminous gas, transformation of matter into energy). This entry is composed of four sentences, and each of them expresses some content related to worldly conditions. According to minimal correspondence, if the expressed state of affairs obtains, the relevant sentence is true; otherwise, it is false. Every astronomy student knows very well to what those sentences refer and which states of affairs should obtain to make them true. But the philosophical point is whether this knowledge can be expressed in the terms of a proper theory of truth; or, in other words, in the terms of a minimal correspondentist interpretation of Tarski's semantic theory. Let us consider the first sentence of the description:

- The Sun is a sphere of luminous gas 1,392 × 10^6 m in diameter.

According to the semantic theory, we shall conclude that:

- The sentence “the Sun is a sphere of 1,392 × 10^6 m in diameter” is true if and only if the Sun is a sphere of 1,392 × 10^6 m in diameter.

Which is a speciman of Tarski's equivalence schema (ES). But, in order to use a Tarskian truth predicate, we should specify the translation of this sentence into any language that is appropriate for Tarski's requirements; that is to say, we should express it in (at least) semi-formal terms. This language will work as a meta-theory, whose aim is to provide an interpretation of the language of experiments and measurements related to it. Since this statement expresses the shape of the Sun and its location in space, we should appeal to a meta-theory that allows us to think about physical objects as located in space-time. Since this is a local application, Einstein's theory is not necessary and a Euclidean three dimensional space combined with one dimensional time will suffice.

The minimal theory we need will consists of the following rough statements:

1. Physical space is a three dimensional Euclidean space E^3. Consequently, after selecting a coordinate system and a unit of length, each point of E^3 can be identified with a triple (x, y, z) of real numbers.
2. Physical time T is a one dimensional continuum, which after selecting a unit of time can be identified with the set of all real numbers.
3. Every physical body b is an entity localized in both space and time; the localization of b, L(b) is a finite Euclidean subspace $E^3 \times T$. If $(x, y, z, t) \in L(b)$ we shall say that b occupies (x, y, z, t) or that it occupies (x, y, z) at the time t. The geometrical figure of the localization of a body is the shape of that body.
4. For every physical body b there is a set of physical bodies called parts of b. If b' is a part of b, then at any time t the localization of b' is a subset of the localization of b. If all the localizations in time of a part of b' of b are single points, then the part b' is called physical point of b.
5. A reference system is a set of physical points (of some bodies) which define a coordinate system for E^3; such physical points cannot move (change their localization with time) and cannot be all located on the same plane.
6. Every two physical points whose distance does not change with time, define a unit of length.
7. Every cyclic movement of a physical point defines a unit of time.
8. The standard unit of length is one meter.
9. The standard unit of time is one second.
10. The set of physical bodies includes the Sun and the Earth.

(Wójcicki 1995: 503-504)

In this semi-formal way, we have defined the notion of "localization of a body" and fixed a relation between physical bodies and regions of physical space; actually, given a time *t*, we can identify the shape of a body with the shape of the region of space it occupies at *t*. And we can set up experiments to analyze the region of space the Sun occupies. For example, we can measure its diameter by measuring the distance between the Earth and the Sun and the angles under which we see the ends of the diameter of the Sun from the Earth.

Thus, the statements that I have sketched describe the mathematical content of the sentence describing the location, the shape and the diameter of the Sun. They consist of logical symbols, mathematical terms of Euclidean geometry and descriptive terms (physical body, parts of a physical body, localization of a physical body and so on). Now, given a mathematical structure and a full list of descriptive terms, we can define a language to which Tarski's definition of truth undoubtedly applies.

Of course, starting with this meta-language, you can choose a set of additional postulates that may restrict the scope of the theory; some additional postulates could not be empirically testable in themselves (because they are purely mathematical conditions with no factual counterpart), but, in conjunction with the empirically testable assumptions, they contribute to derive anything that can be experimentally tested. As I have assumed in Part 1 – sections 1-2 – and Part 2 – sections 1-2 – the most important postulates and physical laws fix the fundamental properties of the domain of the theory (both detection properties and properties that contribute to the empirical application of the mathematical formalism). And, as in Part 3 – section 2 – the most basic step for truth-value attributions is to determine the class of physical bodies to which we are applying the theory. And the identification of the domain of application (and the relevant laws) is very important especially because the relation between mathematical structure and physical interpretation cannot be determined in a general way, but only referring to concrete and specific applications (see part 1, especially sections 3-4 for the role played by concrete applications referring to the respective mathematical formalism).

Another important step to achieve the results of the measurements is to assure that the physical quantities postulated by the theory (time, position of a physical body and so on) can be measured; of course, to do this, you have to introduce other additional postulates, for example, that the path of light in the cosmic vacuum is a straight line[76]. Assuming that the relation between mathematical structure and physical interpretation is clear enough referring to the hypothesis we are testing, now we can propose experiments and models to experimentally test the hypothesis, in this case to measure the diameter of the Sun. Note that the selection of additional postulates concerning measurement does not imply that the theory explicitly defines how measurements should be executed (establishing, for example, explicit definitions or correspondence rules), since the relevant conditions are usually part of the tacit knowledge of the researchers, as a part of normal science work and a consequence of scientific training analyzed in Part 2 – section 2. Of course, no scientist does this kind of analysis in the terms of Tarski's semantic theory, but what is important is that the theory is consistent with intuitive and concrete scientific methodology and that the methodological conclusions to which researchers arrive can have their counterparts in terms of theories of truth.

Moreover, this definition poses that, following the theory of the meaning of scientific terms that I have presented in Part 1 – sections 3-4 – the referential interpretation of a term can be established in a procedural way. Or, in other words, we should not explain what physical terms denote, but, rather, how they are used: literally speaking, descriptive terms do not denote anything. Obviously, you can do this again in the terms of a meta-theory to define the applications of the hypothesis we are testing; but the truth-content of a hypothesis is not determined by a single procedure or application and therefore we have to consider all the procedures and theories relevant to the hypothesis in question. And I have discussed in Part 1 – section 4 – how the structural combination of different applications is essential to the physical interpretation of a mathematical structure.

Thus, in this section, I have analyzed how minimal correspondence is grounded in the semantic theory and how it works in concrete scientific contexts. Additionally, I have showed how this theory of truth is related to the selection of the parts of scientific theories that we consider essential for the empirical success of the theory (see especially Part 3 – section 3, but also Part 1 – sections 1-2 and Part 2 – sections 1-2) and to the

76 Here I am calling postulates some factual claims, concerning for example how light moves. This may be unfair, but they are postulates as far as they behave as interpretations of mathematical structures; this does not exclude that, in other contexts, they may be empirically testable (See Part 1 – Section 1).

theory of the meaning of scientific terms that I have defended (especially in Part 1 – sections 3-4). And this version of the correspondence theory is immune to the objections that I have analyzed in the previous sections: comparison objection (Part 2 – sections 3-4), no unitary account of the correspondence relation (Part 3 – section 1) and rejection of the success argument (Part 3 – section 3).

4. Questions, objections and replies

After summarizing and applying the theory of truth that I am defending, now in this section I will reply to some questions and objections concerning it and other interrelated concepts. In fact those objections do not concern only truth, but also reference, meaning and other concepts I have dealt with in the previous chapters.

Q1. Your discussion of the correspondence theory and its applicability to scientific knowledge rests on the assumption that scientific knowledge is intrinsically related to the concept of truth. But you have not justified this assumption. In particular, you have not discussed the empiricist claim that science can give up truth and replace it with the concept of empirical adequacy (see in particular Van Fraassen 1980).

The use of the concept of empirical adequacy instead of the concept of truth relies on the rejection of the Inference to the Best Explanation (IBE). IBE works both at a basic level and at a meta-level: 1) at the basic level it claims that, if the theory T is the best explanation of some relevant phenomena, we can infer that T is approximately true; 2) at the meta-level, it claims that scientific realism is approximately true, since it is the best explanation of the empirical success of science. One of the problems with IBE is that it treats empirical success as a natural phenomena, something that can be explained in the same way of the natural facts that constitute the domain of empirical science. Scientific realism usually answers positively to this question and conclude that IBE can be applied to the relation between empirical success and scientific realism. On the contrary, several philosophers (Van Fraassen, but also Fine 1996) reply that the best explanation of empirical success should involve only instrumental reliability or empirical adequacy (and not truth). The weak point of this argument is that, at the meta-level, it is circular (Da Costa, French 2003: 176): in what sense is instrumental reliability empirically adequate to the phenomenon we are explaining (empirical success)? At the basic-level of individual theories, it is quite clear how to explain their success in terms of empirical adequacy; but, at the meta-level, how the empirical success of science can be embedded into instrumentalism? Perhaps the empiricist would appeal to the concept of evolutionary fitness, but is not clear in which terms.

Q2. Your description of the weak correspondence theory is really minimal. Unlike traditional correspondence theories, it does not speak about facts and isomorphic relations between propositions and facts. Furthermore, I am not really convinced by your appeal to the notion of approximate truth to defend correspondence; I do not think that truth is a matter of degree. Can you provide another argument to distinguish between correspondence-friendly deflationism and weak correspondence?

My second argument for the distinction between weak correspondence and correspondence-friendly deflationism is related to the same problem of the argument concerning approximate truth: the fact that we need theoretical descriptions to select the parts of scientific theories to which we are ontologically committed (see also Part 1 – section 2); and that, therefore, weak correspondence fits well with selective realism. In its application to scientific theories, deflationism aims to substitute similarity for correspondence. Since it is based on the semantic view of scientific theories, it claims that the correspondence theory is wrong because it looks for a semantic link between theoretical descriptions of models and the facts, while, according to deflationism, models represent the world in terms of similarity relations (Giere 1988). Here, the deflationary theory is supposed to suffice, since the semantic relation is replaced by the similarity between model and the world, while theoretical hypotheses expressing the similarity relation are treated as deflationary.

The problem is that scientific knowledge is not merely interested in generating reliable predictions, but in whether the entities, properties and relations associated with the relevant models have real counterparts in the world (at least if the empiricist viewpoint has been ruled out). And the deflationary theory cannot satisfy this requirement, even if it is not necessarily tied to an anti-realist metaphysics. More precisely, there are at least four ways in which models can represent the world (Chakravartty 2001: 336):

1. correspondence between linguistic formulations and the respective model (conventional, or by definition);
2. correspondence between models and classes of phenomena (determined by the domain of the theory);
3. correspondence between similarity relations claims and actual similarity relations (empirical, since the theoretical hypotheses should be true);
4. correspondence between linguistic descriptions and the world (empirical, since the relation of correspondence obtains if and only if these descriptions are true).

The empiricist deflationist would say that a combination of (1) and (3) will suffice to gain significant knowledge about the world; (1) is not problematic, while (3) can be expressed in deflationary terms saying that, given a relation of similarity S, it is true that S obtains if and only if S obtains. But, if I am right in maintaining that we need physically interpreted structures to express true-or-false claims and select the parts of the theories we are testing, this will not be sufficient for these purposes. In order to understand what a similarity claim is saying about the world, we have to appeal to theoretical descriptions (of the model and the world) to disambiguate the claim and make it true (or false) in a proper way. In other words, we cannot do without (4): we need laws and interpretations to specify how similarities obtain; and, of course, this cannot be done in a deflationary way.

Q3. Your version of the correspondence theory rules out the concept of fact. But, not only traditional theories, but also ordinary linguistic behaviors of competent speakers seem to postulate that true statements correspond to the facts and that a statement is true if and only if there is a fact to which it corresponds. How can you accommodate those phenomena into your theory?

My view is that the statement “it is a fact that p” has been used (and it is currently used) in a quite imprecise way, both by followers of the correspondence theory and by ordinary speakers. Probably there are several connotations concerning the use of this kind of sentence, but it seems to me that the most important one is something along the lines of ““that p” has nothing to to with what we know about it and how we know it. It is just a fact”. A first conclusion that we can draw is that someone who says “it is a fact that p” means that there is a gap between “p” and what makes it true; in other words, that the fact that we express “p” and the relevant fact with the same words does not mean that they are the same thing. Moreover, perhaps the appeal to the facts entails that a statement either corresponds to the facts or it does not; this means that, even though it is not clear how the correspondence relation obtains, anyway there is only one truth, and not several truths. Finally, I would take “it is a fact that p” as an assertion of objectivity. That is to say, people aim to distinguish between sentences such as “the snow is white” and statements concerning moral values and subjective taste, which seem to be more controversial. So, in the end, I think that the most basic idea of the correspondence theory is “how things are” and not “the facts” and that the facts are not necessarily associated with the correspondence theory (Marino 2006).

Q.4 You have said that, by dismissing the concept of fact, weak correspondence can overcome the comparison objection. But, how the comparison objection (and your answer to it) is related to the problem of matching (Rasmussen 2014: 66), that is the idea that a proposition cannot describe the real world because they are structurally different?

Like the comparison objection, the problem of matching concerns how the truth-bearers connect to the entities they wish to describe. Basically, the problem of matching goes as follows:

1. in order to correspond to portions of reality, propositions should be structurally similar to them;
2. propositions are not structurally similar to anything they are supposed to correspond to;
3. propositions do not correspond to the world;

The main difference between comparison objection and problem of matching is that the former attacks the very idea of correspondence from an epistemological viewpoint, without going into the details of the correspondence relation. The point is that we cannot verify whether propositions and facts are similar (to some respect) because we do not have independent access to the facts. On the contrary, the scope of the problem of matching is more narrow, since it attacks only a specific kind of correspondence theory, which postulates an isomorphic relation between propositions and facts. For example, Russell poses that true atomic sentences and facts have the same logical structure. The atomic sentence F(a) is true if and only if it mirrors the fact characterized by the following components: the object denoted by "a" and the property expressed by F (Russell 1907). Of course, this is problematic because, even though it were clear how to individuate the components of a proposition, definitely it is not clear what he means by components of facts. Anyway, the theory of truth I have defended is not committed to the concept of facts as well as to the concept of isomorphic relation, and therefore the problem of matching does not apply to it.

Q5. In your work you have not paid much attention to the coherence theory of truth. But, by eliminating the concept of fact from the correspondence theory and making reference language-dependent, perhaps you are running the risk of moving from a weak correspondence theory to a coherence theory. Can you specify the difference between them?

The coherence theory of truth holds that, given a belief p, p is true if and only if it is part of a coherent collection of beliefs (see Bradley 1914 and Walker 1989). This theory shares some advantages with weak correspondence, for example they both can address the comparison objection. After all, for the coherence theory, there is nothing to compare with our beliefs and therefore the problem of the independent access to an unconceptualized reality no longer arises. But the main difference is that the coherence theory denies what I have taken to be a fundamental intuition about truth: the cleavage intuition, that is the idea that there is a gap between a sentence and what it expresses (Marino 2006). The coherence theory does so because, if the truth of a sentence consists in a relation with other sentences, it will be a relation among linguistic things. And, since there is no place for the actual conditions related to true sentences, there is no gap between sentences and what makes them true as well. And, even worse, this may create problems related to the descriptive content of true sentences, since there is no role for the things described by true sentences. Anyway, the cleavage intuition in itself is particularly important and intuitive for scientific knowledge, since scientific practice is entirely devoted to find non-trivial explanations of how hypotheses are related to phenomena and empirical situations that make them true; and therefore, if there were no cleavage, scientific practice would be pointless.

Q6. Sometimes scientific models are taken to represent their intended domain. How does weak correspondence account for the notion of representation in scientific contexts? The most important point is whether it is related to the concepts of truth and approximate truth that you have presented and if it can be arranged by a deflationary perspective.

Even though models are a fundamental part of scientific theories, I do not think that the concept of truth directly applies to models; and, therefore, I do not think that it immediately applies to the concept of representation related to scientific models. Perhaps, the representation relation they involve is the most easy to account for from a deflationary perspective, appealing to the concept of similarity. For example, claiming that the behavior of the gas atoms can be represented by the behavior of billiard balls means that they are similar. Where, from a structural realist viewpoint, this similarity can be presented as a partial structural isomorphism between the two cases (Da Costa, French 2003: 49). In other words, there are elements of the relevant families of relations that are the same referring to both the behavior of billiard balls and gas atoms. So far so good for the deflationary perspective; but, as I have argued, this is not sufficient for a realist theory, which aims to know the real entities, properties and relations the world is made of (unless you hold, like ontic structural realism, that all there is, is structure, but I have already discussed what kind of problems concerning the concept of truth it entails). To that extent, models are true-or-false strictly speaking only in conjunction with theoretical descriptions and laws and so I think that it is inappropriate to talk about truth referring to models and the relevant concept of representation.

Q7. Weak correspondence assumes that the essence of truth is a relation between linguistic and non-linguistic things, but it denies the existence of a universal correspondence relation, since it claims that it can obtain in several ways. This may be a problem, since, speaking about correspondence, we mean that it is the way sentences are true, not only a formal relation.

This point is quite similar to the problem of the plurality of the truth predicates-properties that I have discussed referring to alethic pluralism and the styles of reasoning. It is quite trivial that sentences can be correlated to the world in several ways, corresponding to different referential functions. And, of course, this does not imply that we should give up the unitary concept of truth and be interested only in the restricted predicates (for language pluralism). But this argument does not take into account the fact that we are referring to interpreted languages. The recognition of different languages and the creation of new ones are valuable questions, but here the point is that weak correspondence can be specified referring to each language. So, as Niiniluoto says "the existence of many truth-relations is not a problem for Tarski, but a natural feature of his model-theoretic approach to formal languages" (Niiniluoto 1999: 61). It is a consequence of the interpretation process, by which, for example, the words "dog" and "chien" refer to the same animal in English and in French and can be justified in the minimal terms of primitive denotation. There is no doubt that the interpretation of a given language may change with the time, but, anyway, as far as the interpretation is provisionally fixed, the correspondence relation can obtain referring to the specific language (or fragment of language) in which we have defined it.

Q8. Actually it is widely acknowledged that scientific communities are not interested in trivial truths, but in significant truths according to the current interests and wonders of scientific inquiry (Kitcher 2001). How can you arrange the objectivity of the correspondence theory to be consistent with the methodologically-oriented nature of scientific truth?

Recent accounts of scientific explanation stress the fact that there is no context-independent account of scientific explanation and that scientific practice is guided by moral and social values, individual or group interests, contingent historical situations, availability of relevant technologies and other pragmatic and epistemic concerns. This seems to be a problem, since weak correspondence strongly maintains the objectivity of truth. But, anyway, it is also a minimal theory, which can deal with the fact that the correspondence relation can obtain in many different ways. Assume, for example, that we should check whether Parish is the capital of France. To do so, we need to transform the sentence "Paris is the capital of France" into an open formula "x is the capital of

France”; then, we are allowed to ask whether Paris satisfies this formula. Of course, there are several ways in which this sentence can gain scientific significance referring to the current moral and social values of scientific communities and society in general, and the current aims, wonders and methods of scientific investigation. But, anyway, we do not need a strong correspondence relation aiming to compare “Paris” with Paris or “Paris is the capital of France” with the fact that Paris is the capital of France. The basic point is to check whether Paris satisfies the formula “x is the capital of France”. The semantic theory (as well as weak correspondence) is not expected to give advice about how to investigate scientific phenomena and how to provide an answer about whether the satisfaction relation between objects and relevant conditions obtain (see Woleński 1999: 62).

Q9. Some philosophers have objected that the semantic theory cannot be applied to scientific knowledge. One of the reasons they say this is that the acceptance of some instances of the equivalence schema leads to ontological relativism, since they force us to commit ourselves to non-existing theoretical entities (for example Jennings 1987).

The argument that tries to show that the equivalence schema (and its instances) entails ontological relativism is merely wrong. It argues that, since any competent speaker is ready to accept that “phlogiston is given off during combustion” is true if and only if phlogiston is given off during combustion, the acceptance of this biconditional implies that we should believe that phlogiston exists and this is certainly false. The argument is wrong since the equivalence schema does not entail that we should believe in the existence of the entities cited in the sentence taken as an instance of it. The biconditional ““p” is true if and only if p” will hold even if the sentence by which we replace “p” is trivially false, since an equivalence statement is true even if both sides are false. Therefore, for example, we will undoubtedly accept that “the snow is green” is true if and only if the snow is green (see also Niiniluoto 1999: 60-61).

Q10. All inflationary theories of truth (like correspondence) acknowledge that the truth property has some explanatory role, for example they use truth to explain meaning in terms of truth-conditions. You have said that, for weak correspondence, sentences are related to worldly conditions, but your theory of meaning seems to go in another *direction.*

A deflationary theory is expected to explain the concept of meaning independently of the concept of truth (see Patterson 2003), for instance appealing to “indications relations”. Now, even though I have not accepted the deflationist interpretation of the equivalence between “there is a rainbow” is true if and only if the angle of refraction is such-and-such and ““there is a rainbow over there” is true if and only if there is a rainbow over there”, I have made some deflationary claims concerning meaning: 1) that the concepts of meaning, primitive denotation and translation are prior to the truth predicate; 2) that the meaning of scientific sentences consists in their use; 3) that there is no global account of meaning and reference. This may seem contradictory, but my point against deflationary truth-conditions is just there is a gap between the equivalence schema and the worldly conditions that scientific sentences point out; otherwise scientific practice would be pointless. Unlike the deflationist, I do not think that the equivalence schema is exhaustive of everything related to the truth, but, at the same time, in my view truth-conditions are not necessary as an explanation of meaning. My viewpoint is that the language in which we are defining the truth predicate should be already interpreted and the meaning fixed by a pragmatic process involving the expertise of scientific communities. But it is plausible to think that the use of some sentences involves knowledge of the circumstances in which they can express something true-or-false. This may be problematic, since knowledge of truth-conditions is propositional, while knowledge of use is not, but: 1) competent speakers often use sentences without being aware of their truth-conditions; 2) it is often difficult to determine truth-conditions, since applications depend on their context of utterance as well (see Récanati 2004). So, in the end, meaning is use, but this does not mean that the equivalence schema is sufficient.

Q11. Your theory of truth is related to the idea that meaning is context-dependent and that we had better give up meaning and use the concept of semantic potential. But Kitcher (1978) has developed the concept of reference potential, which accounts for the plurality of uses of scientific terms in the history of science without having to leave context-independence.

Kitcher's theory of reference potentials rests on the idea that, even if terms were used in different ways, we can specify when the types are co-referential, by substituting tokens of rival theories for those of the theory we wish to replace them. According to Kitcher, this is context-insensitive, since each type is associated with two elements: 1) a description that identifies the referent; 2) the specification of a causal agent in presence of which the term is individuated (something like "indications relations"). In this way, different tokens are equivalent, since, for example, we can establish (without referring to the context) when sentences describing dephlogisticated air are successfully referring to oxygen. There are several problems with this theory (see Psillos 1997). Basically, it is at the same time: too strong, since it attributes to the speakers intentions when they did not have any; too weak, since perhaps all rejected types have some referential tokens. So, for example, since the dominant intention of Aristotle when he was working on his theory of natural places was to analyze moving bodies, one can conclude that some tokens of "natural places" and "motion along geodesics" are co-referential (see also Stanford 2003b). Moreover, historical reconstructions aiming to work out the intentions of modern and ancient scientists in the light of contemporary physics are likely not to be reliable. Anyway, the main differences between Kitcher's model and mine are that: 1) I am not concerned with tokens, but with applications (which are fixed, not subjective); 2) the ambiguity of scientific terms does not depend on the fact that the same object may be dubbed in different ways at different times, but on the existence of different coexistent applications at the same time; 3) I am not concerned with the circumstances under which a term has been firstly introduced.

Q12. You maintain the context-dependence of meaning (and the process of meaning fixing). But this may arise problems related to the cases where there are difficulties in determining the boundaries of some natural kind terms. Since you cannot appeal to theoretical identities (like water = H_2O), how do you deal with borderline cases?

I do not think that H_2O is both necessary and sufficient of water (or that it is the essence of water). On the contrary, replying to Putnam's thought experiment of the Twin Earth, we might conclude that XYZ is water, or better, that it is not clear whether it is in or out of the extension of "water". At the same time, if we have a look at some H_2O with special features, we might be inclined to conclude that some H_2O is not water (see

LaPorte 2004). As I have said, the main point is that there is no metaphysical distinction between essential and superficial properties on which we can safely rely to discover the essence of natural kinds: sometimes the structure is the same, but superficial properties differ, sometimes two substances are superficially similar, but their structures are different. Perhaps the most famous borderline case is the concept of heavy water: is it really water? This is problematic since this case involves other kinds, like isotopes, which overlap each others and apply to most cases. In these situation, I would simply appeal to the fact that, according to the viewpoint about meaning that I have presented, linguistic revision is ubiquitous and that, actually, no linguistic community would be right or wrong in saying that heavy water is water, since they both agree on the relevant facts about the world.

5. Open questions

In the last section I have replied to some questions and objections related to problems that I have faced throughout my work. But I shall admit that there are several important points that, for reasons of space and time, I have not directly challenged. Now, I will briefly introduce those problems that deserve much attention and referring to which more work has to be done.

Deflationism and the concept of reference. According to the deflationist, reference and truth work in the same way: like truth, reference is merely a logical device and therefore truth and reference should be defined independently of each other. Just like the schema "“p” is true if and only if p" is exhaustive of the notion of truth, the schema "if a exists, “a” refers to a", is taken to be exhaustive of the notion of reference. But this assumes that we already know (in a quite clear and precise way) what the name (or predicate) refers to. For example, the sentence "“rabbit” refers to rabbits" is trivially true, but it requires that we are aware of what rabbits are. In other words, the inhabitants of the Earth would accept a sentence such as "“water” refers to water" and the same is true for the inhabitants of the Twin Earth. But, taking for granted the circumstances of Putnam's experiment, they would not agree about the truth-value of such a sentence and about the worldly conditions to which it is associated. The same applies to Quine's example of "gavagai": if we try to understand what "gavagai" stands for, the equivalence (or disquotation) schema will not help us to do this. So, roughly, the open question is whether the equivalence schema plays some metaphysical role in the determination of reference. Ideally, from the perspective I have developed, the problem concerns meaning and translation and not reference as explanation: “gavagai” should be translatable independently of the object it stands for.

The relation between meaning, truth and translation. Several philosophers, for example Hartry Field, have pointed out that the kind of theory of truth I am defending assumes (implicitly or explicitly) a specific relation between meaning and translation: that a theory of translation should be available without appealing to the theory of truth. This is a consequence of the fact that Tarski's partial definitions of truth define it only for those sentences that we grasp. Introducing propositions as metaphysical entities (for example, state of affairs to which sentences refer) would arise several problems and complexities that I have not dealt with assuming (like Tarski) that sentences are the most basic truth-bearers. The point is that, appealing to the equivalence schema, we may say that propositions can be simply defined saying that ““S” expresses the proposition that s”, where s is a sentence of a language we understand; in other words, we need both the sentence s and its translation, to extend the definition to other languages (through the concept of translation). This perspective might succeed in making translation independent from the truth property, but, as far as the concept of translation is not fairly defined, it is not much more informative than saying that a proposition is a class of sentences expressing the same meaning, which is undoubtedly problematic.

Correspondentist and deflationary interpretations of the equivalence schema. Tarski has repeated several times that his definition of the concept of truth is intended as a defense of the ancient concept of correspondence, firstly defended by Aristotle, even though the equivalence schema is metaphysically neutral. But actually this point has been questioned at several levels and many scholars would maintain that the equivalence schema can be better accommodated in deflationary terms. I have not taken a stand on whether the semantic theory is in itself a correspondence or a deflationary theory. Now, my point is that the correspondence theory can be grounded in the semantic theory (and, in my view, it should be grounded in it); and, at that point, you can pick out your own correspondence theory by specifying your metaphysical commitment to the correspondence relation and the truth-makers. My weak correspondence theory is entirely consistent with this project and I have tried to show this by means of the example of the measurement of the diameter of the Sun. Anyway, there are some arguments for the correspondentist interpretation of Tarski's theory that should be analyzed in more details: 1) the objectual quantification used by Tarski and the metaphysical implications it might entail; 2) the role played by the interpretation of the language in which we are defining the truth predicate and the way meanings are fixed, which may provide a connection between language and worldly conditions.

Scientific Realism. Throughout my work I have tried to postulate that we can develop a proper theory of truth for scientific knowledge without specifying whether we endorse scientific realism; and, in this case, which specific kind of scientific realism (entity realism, structural realism, semirealism and so on) should we adopt for this purpose. I have not remained as consistent as I would have liked to be on this purpose: one of my conclusions has been that the choice of the weak correspondence theory (over deflationism) is tied to the concept of selective confirmation and its relation with selective realism. Moreover, I have maintained an overall realist attitude that should be analyzed in depth elsewhere. Anyway, I can briefly try to justify my naive realist commitment by three points, which are not related to a specific theory of scientific realism, but, rather, with the personal “optimistic” attitude I have toward these questions: I am aware that these argument may seem subjective. The first point is common sense: it is intuitively clear that science provides us with a quite accurate description of the world. Secondly, a strong trust in science which may appear a kind of scientism (see Ladyman, Ross 2007). And, finally, naturalism, that is the idea that we should base our philosophy and metaphysics on what scientific theories say about the world. Of course, all these points are very questionable, but, at this stage, by scientific realism I merely mean the idea that we should be optimistic about what science tells us.

Detection and auxiliary properties. In this work I have tried to present a model that, given a theoretical change, is able to distinguish between cases of belief revision (cumulative, i.e. we modify the set of beliefs associated with a given entity) and cases in which we are substantially wrong (referential change, i.e. the entity we were supposed to refer to does not exists or it is radically different from how we thought it would). This is based on the distinction between those properties that cannot be rejected without turning into a referential change (detection properties) and those we can do without (auxiliary properties). So, roughly, given two terms *a* and *b*, they are co-referential if and only if the set of properties and structures associated with *a* is co-extensive with the set of properties and structures associated with *b* (Chakravartty 2007). So, for example, the chemical revolution does not match this condition, because the structure and properties associated with "dephlogisticated air" is not co-extensive with the ones associated with "oxygen". On the contrary, other theoretical changes are cumulative even if rival theories map different entities. But, the problem that should be scrutinized is: how can we distinguish detection and auxiliary properties? Of, course, now with hindsight, we can know which properties were essential, but it would be better to know in principle which properties cannot be rejected without rejecting the whole theory. Moreover, it is not clear how many properties we should recognize to identify an entity with good approximation. And this is a particularly compelling problem if, as I have suggested, the distinction between detection and auxiliary properties is epistemic and not metaphysical.

References

Ainsworth, P.M., (2010), “What is Ontic Structural Realism” Studies in History and Philosophy of Modern Physics 41, pp. 50-57.

Alston, W.P., (1996), *A Realist Conception of Truth*, Cornell University Press, Ithaca.

Andjelković, M. and Williamson, T., (2000), “Truth, Falsity and Borderline Cases”, Philosophical Topics 28, pp. 211-244.

Andersen, H., (2000a), “Kuhn’s Account of Family Resemblance: A Solution to the Problem of Wide-Open Texture”, Erkenntnis 52, pp. 313 – 337.

Andersen, H., (2000b), “Learning by Ostension: Thomas Kuhn on Science Education”, Science and Education 9, pp. 91 – 106.

Andersen, H., Barker, P., Chen, X., (2006), *The Cognitive Structure of Scientific Revolutions*, Cambridge University Press, Cambridge.

Armour-Garb, B.P., (2004), “Minimalism, the Generalization Problem and the Liar”, Synthese 39, pp. 491-512.

Armour-Garb, B.P. and Beall, JC, (2005), “Deflationism: The Basics”, in B.P. Armour-Garb, JC Beall, (eds.), *Deflationary Truth*, Open Court, Chicago 2005, pp. 1-29.

Armstrong, D., (1983), *What is a Law of Nature?*, Cambridge University Press, Cambridge.

Baltas, A., (2004), “On the Grammar Aspects of Radical Scientific Discovery”, Philosophia Scientiae 8, pp. 169-201.

Baker, G.P., Hacker, P.M.S., (2005), *Wittgenstein: Understanding and Meaning*. Part I: Essays. Volume 1 of An Analytical Commentary on the *Philosophical Investigations*. Second extensively revisited edition by P.M.S. Hacker, Blackwell Publishing, Oxford.

Barnes, B.S., (1982), *T.S. Kuhn and Social Science*, Macmillan, London.

Båve, A., (2009), “A Deflationary Theory of Reference”, Synthese 169, pp. 51-73.

Beall, JC, (2013), “Deflated Alethic Pluralism”, in N. Pedersen and C. Wright, (eds.), *Truth and Pluralism: Current Debates*, Oxford University Press, Oxford 2013, pp. 223-238.

Bird, A., (2000), *Thomas Kuhn*, Acumen Publishing, Chesham.

Bird, A., (2004), “Kuhn on Reference and Essence”, Philosophia Scientiae 8, pp. 59 -92.

Bird, A., (2011) “Thomas Kuhn’s Relativistic Legacy”, in S. Hales (ed.), *A Companion to Relativism*, Wiley – Blackwell, Oxford 2011, pp. 475-488.

Bradley, F., (1914), *Essays on Truth and Reality*, Clarendon Press, Oxford.

Brandom, R., (1994), *Making it Explicit*, Harvard University Press, Cambridge, Massachusetts.

Brown, H.I., (1983), “”Incommensurability”, Inquiry 26, pp. 3-29.

Buzzoni, M., (1986), *Semantica, ontologia ed ermeneutica della conoscenza scientifica. Saggio su T.S. Kuhn*, Franco Angeli, Milano.

Caputo, S., (2011), “La verità nel XXI secolo”, in M. Carrara, V. Morato, (eds.), *Verità*, Mimesis, Milano 2010, pp. 3-57.

Cavell, S., (1979), *The Claim of Reason. Wittgenstein, Skepticism, Morality and Tragedy*, Oxford University Press, Oxford.

Cederbaum, D.G., (1983), “Paradigms”, Studies in History and Philosophy of Science, 14, pp. 173-213.

Chakravartty, A., (1998), “Semirealism”, Studies in History and Philosophy of Science 29, pp. 391-408.

Chakravartty, A., (2001), “The Semantic or Model-Theoretic View of Theories and Scientific Realism”, Synthese 127, pp. 325-345.

Chakravartty, A., (2004), “Structuralism as a Form of Scientific Realism”, International Studies in the Philosophy of Science 18, pp. 151-171.

Chakravartty, A., (2007), *A Metaphysics for Scientific Realism: Knowing the Unobservable*, Cambridge University Press, Cambridge.

Chakravartty, A., (forthcoming), “Truth and the Sciences”, in M. Glanzberg (ed.), *The Oxford Handbook of Truth*, Oxford University Press, New York (forthcoming).

Chang, H., (2012), *Is Water H_20? Evidence, Realism and Pluralism*, Springer, Dordrecht.

Chen, X., (1997), "Thomas Kuhn's Latest Notion of Incommensurability", Journal for General Philosophy of Science 28, pp. 257-273.

Cruse, P., Papineau, D., (2002), “Scientific Realism Without Reference”, in M. Marsonet (ed.), *The Problem of Realism*, Ashgate, Aldershot, pp. 174-189.

Cumminskey, D., (1992), “Reference Failure and Scientific Realism. A Response to the Meta-Induction”, The British Journal for the Philosophy of Science 43, pp. 21-40.

D'Amico, R., (1993), *The Problem of the Criterion*, Rowman and Littlefield Publishers, Lanham.

Da Costa, N., French, S., (2003), *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*, Oxford University Press, Oxford.

Damjanovic, N., (2005), "Deflationism and the Success Argument"; The Philosophical Quarterly 55, pp. 53-67.

David, M., (1994), *Correspondence and Disquotation. An Essay on the Nature of Truth*, Oxford University Press, New York.

David, M., (1996), "Analyticity, Carnap, Quine, and Truth", In J. E. Tomberlin (ed.), *Philosophical Perspectives 10: Metaphysics*, Basil Blackwell, Oxford, pp. 281-96.

David, M. (2008). Tarski's Convention T and the Concept of Truth. In D. Patterson (ed.), *New Essays on Tarski and Philosophy* (pp. 133-156). New York: Oxford University Press.

Davidson, D., (1990), "The Structure and the Content of Truth", Journal of Philosophy 67, pp. 279-328.

Davidson, D., (1996), "The Folly of Trying to Define Truth", Journal of Philosophy 93, pp. 263-278.

Demopoulos, W., Friedman, M., (1985), "Critical Notice: Bertrand Russell's *The Analysis of Matter*: Its Historical Context and Contemporary Interest", Philosophy of Science 52, pp. 621-639.

Devitt, M., (1997), *Realism and Truth*, Princeton University Press, Princeton.

Devitt, M., (2005), "Rigid Application", Philosophical Studies 125, pp. 139-165.

Devitt, M., Sterelny, K., (1999), *Language and Reality. An Introduction to the Philosophy of Language*, MIT Press, Cambridge.

Doppelt, G., (1978), "Kuhn's Epistemological Relativism. An Interpretation and Defense", Inquiry, 21, pp. 33 – 86; reprinted in M. Krausz, J.W. Meiland (eds.), *Relativism: Cognitive and Moral*, University of Notre Dame Press, Notre Dame 1983, pp. 113-146.

Dulith Novaes, C., (2008), "Tarski's Hidden Theory of Meaning", in S. Rahman, T. Tulenheimo and E. Genot (eds.), *Unity, Truth and the Liar – The Modern Relevance of Medieval Solutions to Semantic Paradoxes*, Springer, Berlin 2008, pp. 41-63.

Eco, U., (1997), *Kant e l'ornitorinco*, Bompiani, Milano; translated by A. McEwen, *Kant and the Platypus*, Harcourt Brace & Co, New York 1999.

Ellis, B., (2011), *Scientific Essentialism*, Cambridge University Press, New York.

Eddington, A.S., (1929), *The Nature of the Physical World*, The MacMillan Company, New York.

Falk, R., (2000), "The Gene – A Concept in Tension", in P. Beurton, R. Falk and H.J. Rheinberger (eds.), *The Concept of Gene in Development and Evolution. Historical and Epistemological Perpectives*, Cambridge University Press, Cambridge, pp. 137-348.

Fernandez Moreno, L., (2001), "Tarskian Truth and the Correspondence Theory", Synthese 126, pp. 123-147.

Feyerabend, P., (1975), *Against Method. Outline of an Anarchist Theory of Knowledge*, New Left Books, London 1975.

Field, H., (1972), "Tarski's Theory of Truth", Journal of Philosophy 69, pp. 347-375.

Field, H., (1986), "The Deflationary Conception of Truth", in G. Macdonald, C. Wright, (eds.), *Fact, Science and Morality*, Blackwell Publishing, Oxford, 1986, pp. 55-117.

Field, H., (1994), ""Deflationist Views of Meaning and Content", Mind 103, pp. 249-285.

Field, H., (2001), *Truth and the Absence of Facts*, Clarendon Press, Oxford.

Fine, A., (1996), *The Shaky Game*, University of Chicago Press, Chicago.

Forster, M.N., (2004), *Wittgenstein on the Arbitrariness of Grammar*, Princeton University Press, Princeton.

French, S., Krause, D., (2003), "Quantum Vagueness", Erkenntnis 59, pp. 97-124.

French, S., Krause, D., (2006), *Identity in Physics. A Historical, Philosophical and Formal Analysis*, Oxford University Press, Oxford.

French, S., Ladyman, J., (2003), "Remodeling Structural Realism: Quantum Mechanics and the Metaphysics of Structures", Synthese 136, pp. 31-56.

Friedman, M., (1983), *Foundations of Space-Time Theories*, Princeton University Press, Princeton.

Fumerton, R., (1995). *Metaepistemology and Skepticism*. Rowman & Littlefield, London.

García-Belmar, A., Bertomeu-Sánchez, J.R., Bensaude-Vincent, B., (2005), "The Power of Didactic Writings: French Chemistry Textbooks in Nineteen Century", in A. Kaiser, ed., *Pedagogy and the Practice of Science*, The MIT Press, Cambridge 2005, pp. 219-252.

Gelfert, A., (2003), "Manipulative Success and the Unreal", International Studies in the Philosophy of Science 17, pp. 245-263.

Giere, R.N, (1988), *Explaining Science: A Cognitive Approach*, Chicago University Press, Chicago.

Glock, H.J., (ed.), (1996a), *A Wittgenstein Dictionary*, Blackwell Publishing, Oxford.

Glock, H.J., (1996b), "Necessity and Normativity", in H. Sluga, D.G. Stern, (eds.), *The Cambridge Companion to Wittgenstein*, Cambridge University Press, Cambridge 1996, pp. 198-225.

Griffiths, P., Stotz, K., (2006), "Genes in the Postgenomic Era", Theoretical Medicine and Bioethics 27, pp. 499-521.

Gupta, A., (1993), "A Critique of Deflationism", Philosophical Topics 21, pp. 57-81.

Haack, S., (1978), *Philosophy of Logic*, Cambridge University Press, Cambridge.

Hacking, I., (1979), "Imre Lakatos's Philosophy of Science", The British Journal for the Philosophy of Science 30, pp. 381-402.

Hacking, I., (1982), "Language, Truth and Reason", in M. Hollis, S. Lukes, (eds.), *Rationality and Relativism*, MIT Press, Cambridge, Massachusetts 1982, pp. 48-66.

Hacking, I., (1983), *Representing and Intervening. Introductory Topics in the Philosophy of Natural Sciences*, Cambridge University Press, Cambrigde.

Hacking, I., (1984), "Experimentation and Scientific Realism", in J. Leplin, (ed.), *Scientific Realism,* University of California Press, Berkeley 1984.

Hacking, I., (1989), "Extragalactic Reality: the Case of Gravitational Lensing", Philosophy of Science 56, pp. 555-581.

Hacking, I., (1992), "Statistical language, statistical truth and statistical reason: The self-authentication of a style of reasoning", in Ernan McMullin (ed.), Social Dimensions of Science, University of Notre Dame Press, Notre Dame 1992, pp. 130-157.

Hacking, I., (1993), "Working in a New World: The Taxonomic Solution", in P. Horwich, (ed.), *World Changes. Thomas Kuhn and the Nature of Science*, MIT Press, Cambridge, Massachusetts 1993, pp. 275-310.

Hacking, I., (1996), "The Disunities of the Science", in P. Galison and D. Stump (eds.), *The Disunity of Science. Boundaries, Contexts and Power*, Stanford University Press, Stanford, pp. 37-74.

Hacking, I., (2002), *Historical Ontology*, Harvard University Press, Cambridge.

Hacking, I., (2012), "Language, Truth and Reason 30 years later", Studies in History and Philosophy of Science 43, pp. 599-609.

Hardin, C.L., Rosenberg, A., (1982), "In Defense of Convergent Realism", Philosophy of Science 49, pp. 604-615.

Hesse, M., (1966), *Models and Analogies in Science*, Notre Dame University Press, Indiana.

Horgan, T., (2001), "Contextual Semantics and Metaphysical Realism: Truth as Indirect Correspondence," in Michael Lynch (ed.), *The Nature of Truth: Classical and Contemporary Readings*, MIT Press, Cambridge, pp. 67–95.

Horgan, T, Timmons, M., (2002), "Conceptual Relativity and Metaphysical Realism", Nous 36, pp. 74-96.

Horwich, P., (1990), *Truth*, Basil Blackwell, Oxford.

Horwich, P., (2001), "A Defense of Minimalism", in M. Lynch (ed.), *The Nature of Truth. Classic and Contemporary Perspectives*, MIT Press, Cambridge 2001, pp. 559-577.

Horwich, P., (2005), *From a Deflationary Point of View*, Oxford University Press, New York.

Horwich, P., (2009), "Being and Truth", in E.J. Lowe, A. Rami, (eds.), *Truth and Truth-Making*, Acumen Publishing, Stocksfield 2009, pp. 185-200.

Hoyningen-Huene, P., (1993), *Reconstructing Scientific Revolutions. Thomas S. Kuhn's Philosophy of Science*, translated by A.T. Levine, with a "Foreword" by T.S. Kuhn, The University of Chicago Press, Chicago and London.

Hoyningen-Huene, P., Oberheim, E., Andersen, H., (1996), "Essay Review: On Incommensurability", Studies in History and Philosophy of Science 27, pp. 131-141.

Irzik, G., Grünberg, T., (1998), "Whorfian Variations on Kantian Themes: Kuhn's Linguistic Turn", Studies in History and Philosophy of Science 29, pp. 207-221.

Jammer, M., (1954), *Concepts of Space: The History of Theories of Space in Physics*, Harvard University Press, Cambridge.

Jennings, R.C., (1987), "Tarski – a Dilemma", Inquiry 30, pp. 155-172.

Johannsen, Wilhelm, (1909), *Elemente der exakten Erblichkeitslehre*, Gustav Fischer, Jena.

Kindi, V., (2005), "The Relation of History of Science to Philosophy of Science in *The Structure of Scientific Revolutions* and Kuhn's Later Philosophical Work", Perspectives on Science 13, pp. 495- 430.

Kindi, V., (2010), "Novelty and Revolution in Arts and Sciences: The Connection Between Kuhn and Cavell", Perspectives on Science, 18, pp. 284-310.

Kindi, V., (2012), “Concept as Vessel and Concept as Use”, in Feest and Steinle (eds.), *Scientific Concepts and Investigative Practice*, De Gruyter, Berlin, pp. 3-23.

Kirkham, R., (1992), *Theories of Truth: A Critical Introduction*, The MIT Press, Cambridge.

Kitcher, P., (1978), “Theories, Theorists and Theoretical Change”, The Philosophical Review 87, pp. 519-547.

Kitcher, P., (2001), *Science, Truth and Democracy*, Oxford University Press, Oxford.

Kitcher, P., (2002), “On the Explanatory Role of Correspondence Truth”, Philosophy and Phenomenological Research 64, pp. 346-364.

Kölbel, M., 2002, *Truth Without Objectivity*, Routledge, London.

Kripke, S., (1980), *Naming and Necessity*, Basil Bleckwell, Oxford.

Kuhn, T., (1963), “The Function of Dogma in Scientific Research”, in A. Crombie (ed.), *Scientific Change. Historical studies in the intellectual, social and technical invention, from antiquity to the present*, Heinemann Educational Books, London 1963, pp. 347 – 369.

Kuhn, T., (1970), *The Structure of Scientific Revolutions*, 2nd revisited edition, International Encyclopedia of Unified Sciences, vol. 2, no. 2, University of Chicago Press, Chicago and London.

Kuhn, T., (1977), *The Essential Tension. Selected Studies in Scientific Tradition and Change*, The University of Chicago Press, Chicago and London 1977.

Kuhn, T., (1990), “Dubbing and Redubbing: The Vulnerability of Rigid Designation”, in C.W. Savage (ed.), *Scientific Theories*, University of Minnesota Press, Minneapolis 1990, pp. 298 – 318.

Kuhn, T., (1999), “Remarks on Incommensurability and Translation”, in R. Rossini Favretti, G. Sandri, R. Scazzieri (eds.), *Incommensurability and Translation. Kuhnian Perspectives on Scientific Communication and Theory Change*, Edward Elgar Pub, Cheltenham 1999, pp. 33-37.

Kuhn, T., (2000), *The Road Since Structure*, edited by J. Conant, J. Haugeland, The University of Chicago Press, Chicago and London.

Kunne, W., (2003), *Conceptions of Truth*, Clarendon Press, Oxford.

Kuukkanen, J., (2007), “Kuhn, the Correspondence Theory of Truth and Coherentist Epistemology”, Studies in History and Philosophy of Science 38, pp. 555-566.

Kuukkanen, J., (2010), “Kuhn on Essentialism and the Causal Theory of Reference”, Philosophy of Science 77, pp. 544-564.

Ladyman, J., (1998), “What is Structural Realism?”, Studies in History and Philosophy of Science 29, pp. 409-424.

Ladyman, J., (2011), “Structural Realism versus Standard Scientific Realism: the Case of Phlogiston and Dephilogisticated Air”, Synthese 180, pp. 87-101.

Ladyman, J., Ross, D., (2007), *Every Thing Must Go: Metaphysics Naturalized*, Oxford University Press, Oxford.

Lakatos, I., (1970), “Falsification and the Methodology of Scientific Research Programmes”, in I. Lakatos, A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge 1970.

Lakatos, I., Musgrave, A. (eds.), (1970), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge.

Lakatos, I., (1978), *Philosophical Papers vol. 1*, Cambridge University Press, Cambridge.

Lanfredini, R., (1988), *Oggetti e paradigmi. Per una concezione interattiva della conoscenza scientifica*, Theoria, Roma – Napoli.

Laporte, J., (2004), *Natural Kinds and Conceptual Change*, Cambridge University Press, Cambridge.

Laudan, L., (1981), “A Confutation of Convergent Realism”, Philosophy of Science 28, pp. 19-49.

Leeds, S. (1978), “Theories of Reference and Truth”, Erkenntnis 13, pp. 111-129.

Leeds, S., (1995), “Truth, Correspondence and Success”, Philosophical Studies 79, pp. 1-36.

Leeds., S., (2007), “Correspondence Truth and Scientific Realism”, Synthese 159, pp. 1-21.

Leplin, J., (1984), “Introduction”, in J. Leplin (ed.), *Scientific Realism*, University of California Press, London, pp. 1-7.

Levin, M., (1984), “What Kind of Explanation is Truth?”, in J. Leplin (ed.), *Scientific Realism*, University of California Press, London, pp. 124-139.

Lynch, M., (2004), “Truth and Multiple Realizability”, Australasian Journal of Philosophy 82, pp. 384-408.

Lynch, M., (2009), *Truth as One and Many*, Oxford University Press, New York.

MacFarlane, J., (2003), “Future Contingents and Relative Truth”, The Philosophical Quarterly 53, pp. 321-336.

Maddy, P., (2007), *Second Philosophy: A Naturalistic Method*, Oxford University Press, New York.

Malone, M.E., (1993), "Kuhn Reconstructed: Incommensurability Without Relativism", Studies in History and Philosophy of Science 24, pp. 63-93.

Marino, P., (2006), "What Should a Correspondence Theory Be and Do?", Philosophical Studies 127, pp. 415-457.

Marino, P. (2008), "Toward a Modest Correspondence Theory of Truth: Predicates and Properties", Dialogue 47, pp. 81-102.

Marino, P., (2010), "Representation-Friendly Deflationism vs. Modest Correspondence" in C. Wright and N. Pedersen (eds.), *New Waves in Truth,* Palgrave McMillan, New York 2010, pp. 218-231.

Massimi, M., (2010), "Structural Realism: A Neo-Kantian Perspective", in A. Bokulich, P. Bokulich (eds.), *Scientific Structuralism*, Springer, Dordrecht 2010.

Massimi, M., (2015), "Working in a New World: Kuhn, Constructivism and Mind-dependence" Studies in History and Philosophy of Science 50, pp. 83-89.

Masterman, M., (1970), "The Nature of a Paradigm", in Lakatos, Musgrave (1970), pp. 59-89.

McDermid, D., (1998), "Pragmatism and Truth: the Comparison Objection to Correspondence", The Review of Metaphysics 51, pp. 775-811.

McDermid, D., (2006), *The Varieties of Pragmatism. Truth, Realism and Knowledge from James to Rorty*, Continuum, London.

McGrath, M., (2003), "What the Deflationist May Say about Truth-Making", Philosophy and Phenomenological Resarch 66, pp. 666-688.

Medina, J., (2002), *The Unity of Wittgenstein's Philosophy. Necessity, Intelligibility and Normativity*, State University of New York Press, Albany.

Morrison, M. (2000), *Unifying Scientific Theories.* Cambridge: Cambridge University Press.

Newman, M.H.A., (1928), "Mr. Russell's 'Causal Theory of Perception'", Mind 37, pp. 137-148.

Niiniluoto, I, (1987), *Truthlikeness*, Reidel, Dordrecht.

Niiniluoto, I., (1999), *Critical Scientific Realism*, Oxford University Press, Oxford.

Newton-Smith, W.H., (1981), *The Rationality of Science*, Routlege and Kegan Paul, London.

Nola, R., Sankey, H. (eds.), (2000a), *After Popper, Kuhn and Feyerabend. Recent Issues in Theories of Scientific Method*, Kluwer Academic Publishers, Dordrecht.

Nola, R., Sankey, H. (2000b), "A Selective Survey of Theories of Scientific Method", in Nola, Sankey (2000a), pp. 1-65.

Oddie, G., (1986), *Likeness to Truth*, Reidel, Dordrecht.

Patterson, D., (2003), "What is a Correspondence Theory of Truth", Synthese 137, pp. 421-444.

Patterson, D., (2008), "Introduction", in in D. Patterson (ed.), *New Essays on Tarski and Philosophy*, Oxford University Press, New York 2008, pp. 1-20.

Pedersen, N., (2010), "Stabilizing Alethic Pluralism", Philosophical Quarterly 60, pp. 92-108.

Popper, K.R., (1959), *The Logic of Scientific Discovery*, Hutchinson & Co., London.

Popper, K.R., (1970), "Normal Science and Its Dangers", in I. Laktos, A. Musgrave (1970), pp. 51-58

Psillos, S., (1995), "Is Structural Realism the Best of Both Worlds?", Dialectica 49, pp. 15-46.

Psillos, S., (1997), "Kitcher on Reference", International Studies in the Philosophy of Science 11, pp. 259-272.

Psillos, S., (1999), *Scientific Realism: How Science Tracks Truth*, Routledge, London.

Psillos, S., (2000), "The Present State of the Scientific Realism Debate", British Journal for the Philosophy of Science 51, pp. 705-728.

Psillos, S., (2006), "The Structure, the Whole Structure and Nothing but the Structure?", Philosophy of Science 73, pp. 560-570.

Putnam, H., (1975), *Mind, Language and Reality*, Cambridge University Press, Cambridge.

Putnam, H., (1981), *Reason, Truth and History*, Cambridge University Press, Cambridge.

Putnam, H., (1994), "Sense, Nonsense and the Senses: an Inquiry into the Powers of the Human Mind", Journal of Philosophy 91, pp.445-517.

Quine, W.O., (1953), *From a Logical Point of View*, Harvard University Press, New York.

Raatikainen, P., (2008), "Truth, Meaning and Translation", in D. Patterson (ed.), *New*

Essays on Tarski and Philosophy, Oxford University Press, New York 2008, pp. 247-262.

Rasmussen, J., (2014), *Defending the Correspondence Theory of Truth*, Cambridge University Press, Cambridge.

Read, R., (2003), "Kuhn: Le Wittgensteinien des sciences?", Archives de philosophie 66, pp. 463-479.

Recanati, F., (2004), *Literal Meaning*, Cambridge University Press, Cambridge.

Redhead, M.L.G., (2001a), "The Intelligibility of the Universe", Philosophy 48, pp. 73-90.

Redhead, M.L.G., (2001b), "Quest of a Realist", Metascience 10, pp. 341-347.

Resnik, D.B., (1994), "Hacking's Experimental Realism", Canadian Journal of Philosophy 24, pp. 395-412.

Ruphy, S. (2011), "From Hacking's Plurality of Styles of Scientific Reasoning to 'Foliated' Pluralism: A Philosophically Robust Form of Ontologico-Methodological Pluralism", Philosophy of Science 78, pp. 1212-1222.

Russell, B., (1907), "On the Nature of Truth"; Proceedings of the Aristotelian Society 7, pp. 228-249.

Russell, B., (1948), *Human Knowledge: Its Scope and Limits*, George Allen and Unwin, London.

Sankey, H., 1993, "Kuhn's Changing Concept of Incommensurability"; British Journal for the Philosophy of Science 44, pp. 759-774.

Sankey, H., (1994), *The Incommensurability Thesis*, Avebury, Aldershot.

Sankey, H., (1997a), "Induction and Natural Kinds", Principia 1, pp. 235-254.

Sankey, H., (1997b), "Kuhn's Ontological Relativism", in D. Ginev, R.S. Cohen (eds.), *Issues and Images in the Philosophy of Science*, Kluwer Academic Publishers, Dordrecht, 1997, pp. 305-320.

Sankey, H. (1998), "Taxonomic Incommensurability", International Studies in the Philosophy of Science 12, pp. 7-16.

Sankey, H., (2000), "Methodological Pluralism, Normative Naturalism and the Realist Aim of Science", in R. Nola and H. Sankey (eds.), *After Popper, Kuhn and Feyerabend. Recent Issues in Theories of Scientific Method*, Kluwer Acadermic, Dordrecht, pp. 211-230.

Sankey, H., (2011), "Epistemic Relativism and the Problem of the Criterion", Studies in

History and Philosophy of Science 42, pp. 562-570.

Sankey, H., (2012), “Reference, Success and Entity Realism”, Kairos 5, pp. 31-42.

Scheffler, I., (1967), *Science and Subjectivity*, Hackett, Indianapolis.

Shapere, D., (1966), “Meaning and Scientific Change”, in R.G. Colodny (ed.), *Mind and Cosmos. Essays in Contemporary Science and Philosophy*, University of Pittsburgh Press, Pittsburgh 1966, pp. 41-85.

Shapere, D., (1993), “Discussion: Astronomy and Antirealism”, Philosophy of Science 60, pp. 134-150.

Shapiro, S., (2006), *Vagueness in Context*, Oxford University Press, Oxford.

Sharrock, W., Read, R., (2002), *Kuhn. Philosopher of Scientific Revolutions*, Polity Press, Cambridge.

Sher, G., (2005), “Functional Pluralism”, Philosophical Books 46, pp. 311-330.

Sher, G., (2013), “Forms of Correspondence: the Intricate Route From Thought to Reality,” in Pedersen & Wright (eds.) 2013, pp. 157–179.

Siegel, H., (1987), *Relativism Refuted. A Critique of Contemporary Epistemological Relativism*, Reidel, Dordrecht.

Soames, S., (1999), *Understanding Truth*, Oxford University Press, Oxford.

Soames, S., (2002), *Beyond Rigidity*, Oxford University Press, New York.

Stachel, J., (2005), “Structural Realism and Contextual Individuality”, in Y. Ben-Manahem, (ed.), *Hilary Putnam*, Cambridge University Press, Cambridge 2005, pp. 203-219.

Stanford, K., (2003a), “No Refuge for Realism: Selective Confirmation and the History of Science”, Philosophy of Science 70, pp. 913-925.

Stanford, K., (2003b), “Pyrrhic Victories for Scientific Realism” Journal of Philosophy 100, pp. 553-572.

Stegmüller, W., (1976), *The Structure and Dynamics of Theories*, Springer, New York.

Stoljar, D. and Damjanovic, N., (2010), “The Deflationary Theory of Truth”, Stanford Encyclopedia of Philosophy, http://plato.stanford.edu/entries/truth-deflationary.

Tarski, A., (1935), “Der Wahrheitsbegriff in den formalisierten Sprachen”, Studia Philosophica I, pp. 261-405.

Tarski, A., (1944), “The Semantic Conception of Truth and the Foundation of

Semantics”, Philosophy and Phenomenological Research 4, pp. 341-376.

Tarski, A., (1969), “Truth and Proof”, Scientific American 220, pp. 63-77.

Tarski, A., (1983), *Logic, Semantics, Metamathematics, papers from 1923 to 1938*, ed. John Corcoran, Hackett Publishing Company, Indianapolis.

Tobin, E., (2010), “Crosscutting Natural Kinds and the Hierarchy Thesis”, in H. Beebee and N. Sabbarton-Leary, *The Semantic and Metaphysics of Natural Kinds*, Routledge, New York, pp. 179-191.

Tversky, A, (1977), “Features of Similarity”, Psychological Review 84, pp. 327-352.

Van Fraassen, B., (1980), *The Scientific Image*, Oxford University Press, Oxford.

Vanzo, A., (2010), “Kant, Skepticism and the Comparison Argument”, in P. Muchnick (ed.), *Rethinking Kant*, vol. 2, Cambridge Scholars Publishers, Newcastle 2010.

Vision, G., (2004), *Veritas: The Correspondence Theory and Its Enemies*, The MIT Press, Cambridge.

Waismann, F. (1968), “Verfiability”, in Flew (ed.), *Logic and Language*, Basil Blackwell, Oxford pp. 117-144.

Walker, R.C.S., (1989), *The Coherence Theory of Truth: Realism, Anti-realism, Idealism*, Routledge, New York.

Wang, X., (2002), “Taxonomy, Truth – value Gaps and Incommensurability: a Reconstruction of Kuhn’s Taxonomic Interpretation of Incommensurability”, Studies in History and Philosophy of Science 33, pp. 465-485.

Warwick, A., Kaiser, D., (2005), “Kuhn, Foucault and the Power of Pedagogy”, in A. Kaiser, (ed.), *Pedagogy and the Practice of Science*, The MIT Press, Cambridge 2005, pp. 393-409.

Wiggins, D., (2001), *Sameness and Substance*, Cambridge University Press, Cambridge.

Williams, M., (1986), “Do we (Epistemologists) Need a Theory of Truth”, Philosophical Topics 14, pp. 223-242.

Williams, M., (1999), *Wittgenstein, Mind and Meaning. Towards a social conception of mind*, Routledge, London.

Wilson, M., (1994), “Can we Trust Logical Form?”, Journal of Philosophy 91, pp. 519-544.

Wittgenstein, L., (1922), *Tractatus Logico – Philosophicus*, translated by D.F. Pears and F. McGuinness, Routledge & Kegan Paul, London and New York 1961.

Wittgenstein, L., (1958), *Philosophical Investigations*, translated by G.E.M. Anscombe, Basil Blackwell, Oxford.

Wittgenstein, L., (1969), *On Certainty*, translated by D. Paul and G.E.M. Anscombe, Basil Blackwell, Oxford.

Wittgenstein, L., (1974), *Philosophical Grammar*, Basil Blackwell, Oxford.

Wittgenstein, L., (1976), *Wittgenstein's Lectures on the Foundations of Mathematics*, edited by C. Diamond, The Harvester Press, Hassocks.

Wittgenstein, L., (1978), *Remarks on the Foundations of Mathematics*, translated by G.E.M. Anscombe, Basil Blackwell, Oxford.

Woleński, J., (1999), "Semantic Conception of Truth as a Philosophical Theory", in J. Peregrin, (ed.), *Truth and its Nature (if any)*, Kluwer Academic Publishers, Dordrecht 1999, pp. 51-66.

Wright, C., (1992), *Truth and Objectivity*, Harvard University Press, Cambridge.

Worrall, J., (1988), "The Value of a Fixed Methodology", British Journal for the Philosophy of Science 40, pp. 376-388.

Worrall, J., (1989), "Structural Realism: The Best of Both Worlds?", Dialectica 43, pp. 99-124.

Worral, J., (2003), "Normal Science and Dogmatism, Paradigms and Progress: Kuhn 'versus' Popper and Lakatos", in T. Nickles, ed., *Thomas Kuhn*, Cambridge University Press, Cambridge 1993, pp. 65-100.

Wójcicki, J., (1994), "Realism vs. Relativism in Philosophy of Science (Some Comments on Tarski's Theory of Truth", in J. Wolenski (ed.), *Philosophical Logic in Poland*, Kluwer Publishing, Dordrecht, pp. 337-361.

Wójcicki, R., (1995), "Theories, Theoretical Models, Truth Part II: Tarski's Theory of Truth and its Relevance for the Philosophy of Science", Foundations of Science 1, pp. 471-516.

Wray, B., (2011), *Kuhn's Evolutionary Social Epistemology*, Cambridge University Press, New York.