Philosophia Scientiæ
Travaux d'histoire et de philosophie des sciences

8-1 | 2004
Le problème de l'incommensurabilité, un demi-siècle après

Incommensurability and laboratory science

Emiliano Trizio

OpenEdition Journals

Electronic version
URL: http://journals.openedition.org/philosophiascientiae/603
DOI: 10.4000/philosophiascientiae.603
ISSN: 1775-4283

Publisher
Éditions Kimé

Printed version
Date of publication: 1 May 2004
Number of pages: 235-266
ISBN: 2-84174-338-1
ISSN: 1281-2463

Electronic reference
Emiliano Trizio, « Incommensurability and laboratory science », *Philosophia Scientiæ* [Online], 8-1 | 2004, Online since 23 June 2011, connection on 21 April 2019. URL : http://journals.openedition.org/philosophiascientiae/603 ; DOI : 10.4000/philosophiascientiae.603

Incommensurability and laboratory science*

Emiliano Trizio
Universities of Paris-X Nanterre et Venezia Ca' Foscari

Résumé : Le but de l'article est d'établir des relations entre, d'une part, la caractérisation générale kuhnienne de l'incommensurabilité comme impossibilité de traduire l'une dans l'autre les taxinomies de théories scientifiques rivales et, d'autre part, la version plus spécifique de l'incommensurabilité proposée par Hacking, laquelle porte sur des théories concurrentes s'étant stabilisées en relation à des équipements de laboratoire et des techniques de mesure différents. Sur la base d'une analyse, inspirée des travaux de Duhem, de la nature des taxinomies scientifiques, on soutiendra que l'approche linguistique kuhnienne est inadéquate pour rendre compte de la manière dont les termes scientifiques s'appliquent à la nature dans le domaine des sciences de laboratoire, au sein desquelles le rôle des opérations de mesure est essentiel. L'analyse introduira la notion de *taxinomie duale* et celle de *caractère ostensif* d'une théorie. Il apparaîtra que, une fois ces deux notions prises en compte, il devient possible de poser les bases d'une version taxinomique élargie de l'incommensurabilité, susceptible de fournir un cadre commun pour la discussion des exemples introduits par Kuhn et Hacking.

Abstract: The aim of the article is to establish relations between Kuhn's general characterization of incommensurability as the impossibility to translate the taxonomies pertaining to rival scientific theories into one another

*I wish to express my deep gratitude to Léna Soler, whose many comments and suggestions have compelled me to rethink various aspects of this article and to modify it in a substantial way. It is also a pleasure to thank Matteo Bevilacqua, Emiliano Boccardi, Fantina Madricardo, Ayesha Mian, Anna Sartori and the anonymous referees of *Philosophia Scientiæ* for helping me to improve its form and content.

Philosophia Scientiæ, 8 (1), 2004, 235–266.

and Hacking's more specific version of incommensurability affecting competing theories that have stabilized relatively to different laboratory equipments and measurement techniques. On the basis of an analysis of the nature of scientific taxonomies that takes its inspiration from the works of Duhem, it will be argued that Kuhn's language-based approach is inadequate to provide an account of the way scientific terms apply to nature in the domain of physical laboratory science, in which the role of measurement procedures is essential. The analysis will be carried out by introducing the notion of *dual taxonomy* and the notion of *ostensive character of a theory*. It will result that, once these two notions are taken into account, it becomes possible to lay the foundations of an enlarged taxonomic version of incommensurability which can provide a common framework for the discussion of the examples introduced by Kuhn and Hacking.

1. Introduction: Kuhn's linguistic version and Hacking's "literal" version of incommensurability

It is well known that Kuhn has increasingly stressed over the years the role of language, meaning and translation in his account of revolutionary changes and incommensurability[1]. He has also explained that this evolution was coherent with the problems that originally motivated the introduction of this notion: namely those faced by the historian when trying to interpret old scientific texts, which, to a modern reader, seem often more obscure and nonsensical than simply wrong or outdated. Kuhn came to believe that this interpretative work requires, in a non-trivial sense, the understanding of the language in which those texts were written [Kuhn 1983a, 56-57][2] for, "the appearance of nonsense could be removed by recovering older meanings for some of the terms involved, meanings different from those subsequently current" [Kuhn 1990c, 91]. Following this line of reasoning, he compared the task of the historian with that of the linguist and became deeply interested in the works of Quine on meaning and translation [Kuhn 1970b, 165].

It must be stressed that although (contrary to Hacking[3]) Kuhn deems the theory of meaning necessary to any satisfactory account of scientific knowledge [Kuhn 1993, 229], he restricts its role in the discussion of incommensurability to the treatment of "taxonomic terms" or "kind terms"

[1]On the evolution of the concept of incommensurability in Kuhn's writings after its introduction in 1962 see [Hoyningen-Huene 1989, 206-222] (the pages mentionned are the ones of the english edition).

[2]For all the papers contained in [Kuhn 2000], the pages mentionned are the ones of this last book.

[3]See, for instance, [Hacking 1983, 43-45].

[Kuhn 1990c, 92]. Different theories have a different lexical taxonomy, (which Kuhn calls also *lexicon* [Kuhn 1990c, 93]). When translation between such lexical taxonomies is (at least partially) impossible, the two theories are said to be *incommensurable*: there is no common measure in the sense that "there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss" [Kuhn 1983a, 36].

Kuhn's linguistic-taxonomic account of incommensurability is far from being the only one. Since its introduction in 1962, the concept of incommensurability has given rise to several attempts to grasp the main idea underlying the purported "lack of common measure" that the word by itself suggests. Kuhn's departure from a literal interpretation of the concept, as we have seen, comes along with his stress on language and translatability (incommensurability = no common language). Hacking and Pickering have instead maintained that incommensurability can also be interpreted in a non-metaphorical way, when the essential role played by measurement instruments and procedures in laboratory science are taken into consideration. While Kuhn, as it will repeatedly be stressed, develops a highly general account of incommensurability, which applies to common sense language as well as to scientific theories of any degree of complexity, Hacking and Pickering have devoted special attention to the relation between rival theories whose development is deeply intertwined with the stabilization of different and incompatible laboratory practices and equipments.

According to Hacking, "Stable laboratory science arises when theories and laboratory equipment evolve in such a way that they match each other and are mutually self-vindicating" [Hacking 1992, 56]. As self-vindication does not exclude the possibility that more refined instruments produce new sets of data incompatible with the available theories, laboratory sciences can undergo deep crises that call for the development of new theories. This development takes place as a complex, mutual maturation of theory and experiment leading to a new stabilized stage of laboratory science. Yet the old theory is still compatible with the old sets of instruments and data. This fact is essential to Hacking's views on incommensurability.

> Kuhn (1961) noticed almost all of this with characteristic precision. Fetishistic measurement sometimes hints at anomaly that can only be tackled by devising new categories of instruments that generate new data that can be interpreted only by new sort of theory: not puzzle solving but revolution. This is the overriding theme of his study of black-body radiation (Kuhn 1978). He omitted only the fact that the old theory and its instruments

> remain pretty much in place, in their data domain. Hence new and old theory are incommensurable in an entirely straightforward sense. They have no common measure because the instruments providing the measurements for the one are inapt for the other. This is a scientific fact that has nothing to do with "meaning change" and other semantic notions that have been associated with incommensurability. [Hacking 1983, 43-45][4].

Hacking distinguishes this kind of incommensurability from the one that concerns the translation between kind terms that he discusses in another article [Hacking 1993, 275-310]. His two separate treatments have a lot in common, for they are both based on Hacking's fundamental unwillingness to interpret science (and knowledge in general) solely in terms of notions such as *language* and *meaning* at the expenses of its *practical* and *material* dimension.

Although I would not subscribe to the view that a theory of meaning is altogether unnecessary in the philosophy of science, the general point of view that I will adopt in this article is close to that of Hacking. The fact that theories can be considered up to a certain extent as sets of sentences or, at any rate, as abstract meaning structures, has led many philosophers (*in primis* the logical empiricists) to neglect the crucial role played by practice and manipulation in science. Science was thus reduced to the pair language/observation and little effort was made to analyse in detail what the notion "observation" really amounts to in science and what goes with it. As it is well known, Kuhn, along with others, has contributed to the overthrow of logical empiricism, precisely by blurring the distinction theory/observation; but, I will argue, he has done it in such a way that language has been granted a role, which is, if possible, even more prominent than it used to be. Kuhn's analyses do not focus on the different status of the operations that allow different types of scientific theories and taxonomies to apply to the world. His study of exemplars, which I will discuss later in some detail, underwrites this interpretation. I will try to give an account of "how scientific terms attach to nature" that takes into consideration these differences, with special attention to the distinction between *observation* and *measurement*. The aim of this analysis will be to show that the two kinds of incommensurability just referred to can be accommodated in an enlarged taxonomic version, which takes into account the distinctive roles of observation, experimental practices and measurement procedures.

One terminological clarification concerning the meaning of the term "taxonomy" is indeed necessary. In Kuhn's works terms like "vocabulary",

[4]See also [Pickering 1984, 407-414], and [Pickering 1995, 186-192].

"structured vocabulary" [Kuhn 1989, 61], "conceptual vocabulary" [Kuhn 1991, 220], "lexicon", "lexical taxonomy", "cluster of terms" [Kuhn 1989, 65] are employed more or less as synonyms. One has the impression that he tends to prefer "taxonomy" or "lexical taxonomy" when he refers to theories (like ancient astronomy) whose concepts can be ordered in "trees" on the basis of the relation between *genera* et *species* [Kuhn 1991, 218], while he is more inclined to speak simply of "lexicon" or "cluster of terms" when he discusses the conceptual structures of physico-mathematical theories such as Newtonian mechanics [Kuhn 1989, 65-66]. Yet it seems to me that he intends to analyse a theory's conceptual vocabulary without presupposing that its structure is that of a classical "aristotelian" taxonomy. Hacking, on the contrary, discusses kuhnian incommensurability on the basis of a precise definition of "conceptual taxonomy" in terms of the relation *genus/species* and of the existence of a *summum genus* or *category* at the top of the tree [Hacking 1993, 286], [Hacking 2003].

In what follows, I will use the word "taxonomy" in what I take to be Kuhn's loose sense, without assuming that the conditions defining tree-like taxonomies are always fulfilled and I will employ the aforementioned companion expressions more or less according to the kuhnian usage. This choice is justified by the fact that many of Kuhn's analyses concern conceptual clusters whose structure cannot be reduced to that of a taxonomic tree[5]. In general, we can say that the problem of characterizing the nature of revolutionary scientific development demands that an account of scientific *conceptualization* be given, and the tree-like taxonomic pattern based on the genus/species relation does not seem to be apt to accommodate the huge variety of conceptual structures that we encounter in science. This becomes more apparent when physico-mathematical theories[6] are taken into account[7].

The plan of the article is the following:

- in *section 2* a comparison developed by Nancy Cartwright between Kuhn's and Duhem's accounts of the relation between theory and observation will be discussed. It will result that a discussion of the

[5]Sometimes what Kuhn calls a "lexicon" even includes laws of nature and consequently cannot be thought of as a mere structured set of kind terms, "Once mass and *the second law have been added to the Newtonian lexicon* in this way, the law of gravity can be introduced as an empirical regularity" [Kuhn 1989, 70] (emphasis added). I will avoid this admittedly deviant usage.

[6]Throughout the article I will refer to *physico-mathematical theories, terms or taxonomies* as those specifically pertaining to modern *mathematized physics*.

[7]As it was shown already by [Cassirer 1910], especially chapters I and IV.

way scientific terms attach to nature requires, in the case of mathematical physics, a careful analysis of laboratory practices. Such an analysis appears to be missing in Kuhn's writings, which focus mainly on the use of scientific language in teaching, learning and theorizing, without an acknowledgement of the ultimately essential role of experiments.

- In *section 3* some of Duhem's penetrating insights on the subject are recalled and,

- in *section 4*, they are taken as the starting point for a characterization of the nature of conceptualization in laboratory science. It is claimed that what Duhem's analysis shows is that physical idealizations differ from everyday observational concepts not so much because they are more general and abstract, but because, in order to apply them to the world, one must simultaneously yield a theoretical interpretation of whatever contributes to define the experimental context that renders their application possible. Taxonomies belonging to mathematical physics will thus be described as *dual*, in the sense that they subsume natural entities and processes only if they *co-subsume* practical contexts and laboratory items.

- These results will be used in *section 5* to criticize Kuhn's language-centred account of the way scientific terms attach to nature. It will be argued that his language-relative developmental characterization of the theory/observation distinction is unable to capture the essential difference existing between taxonomies that can be applied perceptually and taxonomies that require acts of measurements (and that are therefore dual).

- In *section 6* Kuhn's failure to recognize the importance and specificity of laboratory practices will be shown to be a consequence of the fact that his analysis conflates learning and understanding scientific terms with their authentic application to nature.

- The notion of dual taxonomy will be further clarified and illustrated with the aid of an example in *section 7*.

- In *section 8* the application of scientific theories outside the laboratory will be briefly discussed. For this purpose the notion of *ostensive character of a theory* will be introduced. Theories can be classified according to the degree of perceptual accessibility of the objects and processes they are about (the ostensive character).

Much of contemporary laboratory science corresponds to a stage at which theories have very weak or no ostensive character at all and whose taxonomies can be applied to the world only via the mediation of measurement instruments.

- Finally, in *section 9*, on the grounds of the preceding analyses, the main ideas for an enlarged taxonomic version of incommensurability will be introduced.

2. Cartwright on Duhem and Kuhn

Nancy Cartwright has drawn attention on some essential features common to Duhem's and Kuhn's accounts of the relation between theory and observation. Her starting point is that observation is theory-laden and that Kuhn has helped us to see it. Nevertheless, she goes on, "(...) to admit that observation is theory-laden is a long way from denying that there is a theory/observation distinction". She rightly points out that terms used by scientists can be and often are more *recondite* than those used in every-day life. Disagreement begins as soon as one wonders how the dividing line has to be drawn. Cartwright quotes a long passage taken from section 3 of the Postscript of *The Structure of Scientific Revolutions* to the effect that Kuhn too sees a gap between the abstract and symbolic generalizations of physical theory and the concrete situations to which they apply. I will start my analysis from that passage just as Cartwright does.

The central notion of the passage is that of *paradigm as shared example*[8]. To illustrate it Kuhn considers Newton's second principle (or law) of dynamics $f = ma$. According to him the meaning and cognitive content of the principle is not located at a purely theoretical level. The kuhnian classical move to understand the real cognitive content (and, as we shall see, not an innocent one) is to resort to an analysis of the way the members of the community have learned to apply the expression to some paradigmatic concrete situations. According to Kuhn $f = ma$ is, rather than a simple law, a law-sketch or a law-schema. Students learn how to use different versions of it by being exposed to paradigmatic cases of classical dynamical systems such as a free falling body, a simple pendulum, a pair of harmonic oscillators, etc. In each case they are taught

[8]It is worth recalling that this notion constitutes the fourth components of the disciplinary matrix, which in turn represents what the members of a scientific community share during a period of normal science (see [Kuhn 1970a, 181-187]).

how to recognize masses, forces and accelerations and how to pick up the suitable variants of Newton's law. Kuhn concludes:

> The resultant ability to see a variety of situations as like each other, as subject for $f = ma$ or some other symbolic generalisation, is, I think, the main thing a student acquires by doing exemplary problems, *whether with pencil and paper or in a well-designed laboratory*[9].

That is, Kuhn can say, "At the start and for some time after, doing problems is learning consequential things about nature". There is no real understanding of a symbolic generalization without simultaneous acquaintance with particular examples that is without a given interpretation of some phenomenal situations.

According to Cartwright, Duhem's analysis has a lot in common with that of Kuhn. I will now sketch her views on the subject and subsequently argue that there is actually a crucial difference between the accounts of the two historians and philosophers of science. Such difference will in turn result highly interesting in the discussion of Kuhn's views about incommensurability.

It is well known that Duhem draws a sharp line between the language of common sense and the language of theory. A common sense generalization such as "all men are mortal" is postulated inductively on the grounds of direct observations of facts accessible in everyday life. This is the case because terms such as "man" and "mortal" apply directly to particular cases. Everyday concepts are *abstract* and *general* but they can be, according to Duhem, attached unproblematically to individual objects in the real world. A law of physics is, on the contrary, an abstract and *symbolic* generalization, whose terms cannot be applied directly to individual objects: we cannot see temperature instantiated in a concrete object, just as we see its shape or colour. In order to judge that an object has a certain temperature we must resort to an act of measurement. It is measurement, as Duhem famously said, that makes possible the translation[10] between the abstract and symbolic expressions of physical theory and the concrete circumstances that also the "layman ignorant of physics" would witness while an experiment is being performed.

---

[9] [Kuhn 1970a, 189], emphasis added. The fact that Kuhn considers irrelevant whether a concept is acquired "with pencil and paper or in a well-designed laboratory" will play an important role in the next sections.

[10] It order to avoid confusion, it must be stressed that Duhem uses the term "translation" somehow metaphorically, and not in the more technical sense implied by Kuhn while discussing the notion of incommensurability.

According to Cartwright, Duhem's and Kuhn's claims can be both accounted for by pointing out that the relation between a symbolic expression and the different cases to which it applies is that of the *abstract* to the *concrete*. Just like the abstract moral of a fable is expressed and made intelligible thanks to the description of a concrete situation, an abstract generalization such as $f = ma$ needs more concrete models in order to be understood by students and practitioners of physics. Clearly, there are many different possible models embodying concrete situations that fall under a single abstract description.

The question that should be now asked, I believe, is just: how concrete are the models Cartwright (and Kuhn) refers to? Certainly, not as concrete as Duhem's common sense facts and descriptions. The following example is sufficient to show this:

> I maintain that to say of mass m that it is distance r from another mass M is a concrete description to which corresponds the familiar more abstract description "Mass m is subject to a force of size GmM/r^2". [Cartwright 1993, 268].

What is considered concrete in this case is actually expressed in mathematical terms and involves a physical concept such as that of *mass*. Clearly then, the models that render abstract and symbolic generalizations intelligible can still be quite apart from the world of everyday experience. Cartwright is fully aware of this:

> Of course, the more concrete descriptions may themselves be abstract when compared to yet another level of discourse in terms of which they can be more concretely fitted out in turn. (...) Models are a long way from the world. [Cartwright 1993, 270].

How then is the actual connection with the world eventually obtained in the case of mathematical physics? How do models relate to reality? That can happen only when an experiment is performed[11]. The problem of the relationship between the recondite concepts of physical theory and those of everyday experience cannot therefore be resolved simply by distinguishing physical theory from models. A schematic account based on at least three stages must be developed, where the final link with the world is realized by means of experimental practices. Cartwright (who also pays attention to this third experimental level) has excessively stressed, I believe, the analogy between Duhem's and Kuhn's views on

[11] There are indeed other highly theoretical sciences whose hypotheses are not related to the world only with the aid of experiments.

the subject. In the following section I will briefly recall some of Duhem's interesting insights on the relation between theory and experiment. Subsequently, I will try to show, on the grounds of a qualified "duhemian" position, that Kuhn has never really put this relation properly into focus when dealing with the problem of *how scientific terms attach to nature*.

3. Back to Duhem

The problem of relating the symbolic language of physics to nature can be rephrased, borrowing from Duhem, in terms of the possibility to translate *theoretical facts* into *practical facts* and vice versa. It is true that Duhem draws a distinction between common sense observation and physical experiment; but let us not lose sight of the fact that he also attributes to the results of the experiments belonging to descriptive sciences such as physiology or botany the status of mere reports of concrete facts that need no interpretation in theoretical terms[12]. The peculiar character of the experiments performed in physics (and up to a certain extent, in chemistry) is due to their being aimed at applying mathematical language to nature. A *theoretical fact* is for Duhem, an idealized system fully defined in terms of physical properties, which are in turn expressed in *exact* mathematical terms, whereas a *practical fact* is what corre-

[12] Duhem's insistence that only physico-mathematical language is responsible for the theory-ladenness of observation is, as it will result clear in the following sections, one of the aspects of his thought that can no longer be accepted. Some of Duhem's statements about common sense knowledge sound to today's readers, to say the least, baffling, "The laws that ordinary non-scientific experience allows us to formulate are general judgements whose meaning is immediate. In the presence of one of these judgments we may ask, 'Is it true?' Often the answer is easy; in any case the answer is a definite yes or no. The law recognized as true is so for all time and for all men; it is fixed and absolute." [Duhem 1991, 178] (the pages mentionned are the ones of the english edition). The great critic of inductive method doesn't even question the absolute validity of general common sense judgements, besides that of particular ones! Theories, however, do not exist, according to Duhem, only within mathematical physics, although the term "theoretical fact" is forged by him specifically for the latter. Also physiologists develop theories, but the experiments they perform are not infected by them and rest on the direct observation of "a recital of concrete and obvious facts" [Duhem 1991, 147], whose status is very much alike to that of common sense experience, "When many philosophers talk about experimental sciences, they think only of sciences still close to their origins, e.g., physiology or certain branches of chemistry where the experimenter reasons directly on the facts by a method which is only common sense brought to greater attentiveness but where mathematical theory has not yet introduced its symbolic representations." [Duhem 1991, 180]. Experiments pertaining to those sciences "still close to the origin" involve theoretical presuppositions only when (again) physical instruments are used [Duhem 1991, 183], but clearly this is an exogenous source of theory-ladenness.

sponds to it in the real world, which lies before our eyes, the world in which there is no perfect geometrical shape, no point-like particle, no physical magnitude defined at each instant and at each point and, most of all, no exactly quantifiable property; in short, the world accessible to our senses and describable without any knowledge of physics[13].

It is tempting to think that Duhem is trying to relate objects, as they are directly perceived, with the physico-mathematical modelizations that replace them in the calculation of the theoretician. The layman sees a block of ice (the practical fact), while the physicist "sees" a thermodynamic system subject to certain pressure etc. (the theoretical fact). The famous title of the section opening the chapter of *La Théorie Physique* in which Duhem analyses the role of experiments in physics, seems to underwrite this interpretation: "*An Experiment in Physics Is not Simply the Observation of a Phenomenon; It Is, Besides, the Theoretical Interpretation of This Phenomenon*" [Duhem 1991, 144]. The title suggests that on one side there are the phenomena, the practical facts (i.e. nature) as they are directly observed, while on the other side there is their interpretation in the language of physical theory. Nevertheless, the examples described in the section show that the real state of affairs is more complicated. It is worth quoting at length the description of one of them:

> Regnault is studying the compressibility of gases; he takes a certain quantity of gas, encloses it in a glass tube, keeps the temperature constant, and measures the pressure the gas supports and the volume it occupies.
>
> There you have, it will be said, the minute and exact observation of certain phenomena and certain facts. Certainly, *in the hands and under the eyes of Regnault, (. . .) concrete facts were produced*; was the recording of these facts that Regnault reported his intended contribution to the advancement of physics? No. In a sighting device Regnault saw the image of a certain

[13] Duhem's theoretical facts are very much at the same level of Cartwright's models and Kuhn's text-book problems: they are expressed in the language of physics, but they are not as general and abstract as either physical theories or physical laws. They differ from models or exercises in that the latter consist of the description of a whole physical situation, whereas the expression "theoretical fact" can be used also to refer just to a part or a single aspect of it, or to temporal stages of its evolution. An oscillating circuit with given characteristics can be the object of textbook exercises and can be a model that helps us to relate electromagnetic theory to the world. A theoretical fact, on the other hand, can consist of a description of the kind "there is here an oscillating circuit of such and such characteristics", but it can be also "there is an electrical current of three ampere" and even "the current is on". Finally, theoretical facts must be distinguished from laws and generalizations that Duhem sees as complex summaries of theoretical facts and that, in turn, are classified and summarized by physical theories [Duhem 1991, 22-23].

> surface of mercury become level with a certain line; is that what he recorded in the report of his experiment? No, he recorded that a gas occupied a volume having such and such a value. An assistant raised and lowered the lens of a cathetometer until the image of another height of mercury became level with the hairline of the lens; he then observed the disposition of certain lines on the scale and on the vernier of the cathetometer; is that what we find in Regnault's memoir? No, we read there that the pressure supported by the gas had such and such a value. (...) [Those values are] *abstract symbols* which only physical theory connects to the *facts really observed*. [Duhem 1991, 145-146].

As this passage clearly shows, what the concrete facts really observed amount to is the *history of the experimental setting* while the experiment is been performed, as it would appear to a layman ignorant of physics. This history is not only that of the objects under investigation, the fragments of external reality the experimenter strives to describe in physical terms (in this case, the gas enclosed in a glass tube); it is also that of the *actions, instruments and tools* that have been deployed in order to bring about the required conditions (Duhem says: "concrete facts were produced") and to carry out the relevant measurement procedures. It is this "the recital of concrete facts" that must be interpreted with the aid of physical theory, not a chunk of the ready-made phenomenal world, for clearly, a thermometer and its use have nothing do to, by themselves, with the sample of gas. This recital is translated in the report, which in turn provides the basis for stating the results of the experiment. Both the report and the results are stated in an abstract and symbolic language.

Duhem's analysis of the nature of this reformulation in theoretical terms can be schematized in the following way:

1. The sentences in common sense language describing the facts really observed must be replaced by abstract and symbolic judgements. This, in turn, implies that:
   - the instruments used must be mentally substituted by idealizations endowed with physical properties known in an approximate way[14].
   - the actual spatio-temporal region in which the phenomenon under investigation occurs must be replaced in the mind of

[14] It is while discussing the role of instruments in laboratories that Duhem introduces the important notion of *type schématique d'instrument* (see original version, [Duhem 1906, 235], unfortunately translated into English as "schematic model" [Duhem 1991, 156], thus somehow hiding the possible link with the classificatory nature of physical concepts and laboratory activities (see next section).

the experimenter by a physical system fully defined by all physical properties that, as far as it is known, may turn out to affect in some way the result.

2. The data obtained must be analysed according to the theoretical interpretation of the experiment in order to formulate the abstract and symbolic judgement constituting the result of the experiment.

The theoretical replacement concerning the laboratory as a spatiotemporal region implies, we could say, that the entire world is omitted from the theoretical interpretation of a phenomenon, except for what our theories assert to be relevant, once a certain degree of approximation in the physical description has been set as a target.

Having briefly recalled Duhem's reconstruction of the interpretative moment of an experiment, we can now try to see whether it helps us to better appreciate the nature of the relation between theoretical facts and practical facts and, consequently, that of physical theory to the world.

4. Conceptualization in laboratory science: the dual taxonomy

As we have seen, Duhem refers to this relation as a *translation*. However his own account invites us to describe it as one of *conceptualization* or *subsumption*: physical theory provides a network of concepts that through experimental practices are applied to the world. Talk of concepts and their application will help us to highlight the role of practice in laboratory science and its relevance for the doctrine of incommensurability.

What is essential to this analysis is that it shows us how to recognize the peculiar character of the conceptualization provided by physical theory *vis-à-vis* the ordinary application of observational concepts. Concepts of any kind serve the purpose of grouping individuals into classes. They provide a taxonomy of things, events and processes that renders the world intelligible to us. Although it would seem natural to think that physical knowledge develops on the grounds of commonsensical, pre-existing taxonomies, by correcting and refining them in virtue of a rich and highly sophisticated network of concepts which, so to speak, superimposes on the previous one, the preceding analysis shows that this is not exactly the case. I refer here to the fact that physical concepts attach to nature only if a whole *experimental context* is theoretically interpreted. Let us see why this fact makes physical concepts and descriptions so different from ordinary ones, by comparing the following two statements:

(1) when the sun rises, the air becomes warmer;

(2) if the pressure of a gas battery increases by so many atmospheres, its electromotive force increases by so many volts.

Statement (2) is reported by Duhem as an example of an experiment's conclusion [Duhem 1991, 148]. Of course (1) can be understood by any competent speaker of English, whereas understanding (2) requires some knowledge of physics; yet their cognitive content remains essentially different also for the trained experimenter. Both statements can be tested under the circumstances they describe, but the (practical) facts referred to by (1) are recognized on the grounds of everyday perceptual similarity, while the situation described by (2) can be tested in infinitely different concrete situations that have hardly anything in common in terms of perceptual properties[15]. Perceptual similarities are simply sidestepped by physical conceptualizations. The experimenter in order to control the validity of (2) may adopt several alternative measurement techniques, which require different instruments and procedures. The experimental setting which would be perceptible in the laboratory and the series of manipulations it would undergo would change completely from case to case. The interpretation of a physical experiment I previously mentioned requires different actions depending on the choice of samples, instruments and tools and even of the place in which the laboratory is situated. Without a careful check of these conditions, without a simultaneous subsumption of these items under appropriate theoretical types, the required physical description won't apply at all to what is actually seen and touched. However:

> (...) all these diverse manipulations, among which the uninitiated would fail to see any analogy, are not really different experiments; they are only different forms of the same experiment; the facts which have been really produced have been as dissimilar as possible, yet the perception of these facts is expressed by a single proposition: The electromotive force of a certain battery increases by so many volts when the pressure is increased by so many atmospheres. [Duhem 1991, 149].

[15] This is not only true of physico-mathematical descriptions, for there exist other types of concepts that do not apply to the world in virtue of perceptual similarities standards. What is however at issue here is a comparison between the language of mathematical physics and the language used in basic observational reports.

Sentence (1) describes a particular set of facts involving situations which are identifiable on the basis of perceptual similarity and, moreover, identifiable independently one from the other. It is not because you see that the sun is rising that you feel warmer and vice versa. Objects and processes enter sentence (1) one by one, without mutual definition. On the contrary, the experimenter cannot say that the pressure has increased by so many atmospheres without holding some beliefs about the manometer (and about many other things) and, conversely, the failure of observing expected phenomena might cast doubts on the reliability of the instruments implemented. New checks may be called for (new practical facts would take place in the laboratory), new systematic errors may be taken into account, and the very fact that an instrument was acting as a manometer, that is, its being an instantiation of an abstract type of instrument, may be questioned. This example clearly illustrates a situation that we could describe with the term *co-subsumption*: the gas cannot be subsumed under the predicate "having a pressure of x atmospheres" if at the same time the instrument used to determine that the value of its pressure is x is not subsumed under the predicate "reliable manometer".

A concept's extension can be said to consist of an equivalence class of individuals. What we have just seen is that physico-mathematical concepts cannot divide up the world in different classes without a simultaneous definition of equivalent classes of experiments allowing their applications to the world; thus, physical theory does not provide a static taxonomy of the world, it yields rather an interrelated *dual taxonomy of entities and actions in experimental contexts*. In a laboratory physical entities cannot be classified if a suitable *co-classification* of the practical context and of the material items involved does not take place[16]. Only in this way can we understand in what sense physical experiments may be said to be repeatable. As a series of concrete facts each experiment actually performed is unique and different from any other, but physical theory provides a complex conceptual unification of these series of facts under a single abstract and symbolic description. Only at the level of these abstract descriptions can the methodological condition of repeatability be fulfilled: observable regularities play little if any role at all in laboratory science[17].

---

[16]Popper famously said that in the exploration of reality our theories play the role of new *sense organs*. I believe this should be said rather of experimental contexts under a theoretical description. An equivalent class of seemingly different experiments conceptually unified by a theoretical description constitutes an extension of the cognitive possibilities of our body.

[17]In an article on the nature of laboratory science, Ian Hacking has maintained

5. Perceptual taxonomies and physico-mathematical taxonomies

We have seen that Duhem introduces a distinction between physics and chemistry on the one hand and more descriptive sciences such as anatomy and physiology on the other. His holism applies only to the former, while the latter relate to the world somehow unproblematically, in virtue of the direct mirroring of external reality provided by naked-eye observation. There is hardly the need to say that we can no longer accept such a sharp cleavage. There is no such thing as a purely descriptive common sense language and the concepts used in ordinary contexts to classify the objects of perception are always theory-laden. Just as a physicist cannot leave his theories outside the laboratory when performing an experiment, a layman cannot utter any judgement about the world without interpreting what he sees in the light of a given conceptual network. Where Duhem saw a sharp discontinuity, more recent philosophers of science have seen, at best, a continuous range of different degrees of theory-ladenness. This fact is clearly highlighted by Kuhn's treatment of meaning change and incommensurable taxonomies, which is supported by examples taken from episodes scattered throughout the history of science, regardless of the level of abstraction of the theories under discussion and of the degree of sophistication of the experimental techniques involved. Kuhn mentions the shift from Ptolemy's to Copernicus' astronomy on a par with the controversy about phlogiston or the conceptual changes brought about by Planck's solution to the black-body problem. He treats all these historical episodes as examples of how the way *language* organizes experience can undergo more or less radical changes. It really seems that language and experience are no less central to Kuhn's history-oriented account of knowledge than to the log-

---

that Duhem's analysis stresses the role of intellectual elements on the grounds that it focuses on the interplay of theories and auxiliary hypotheses, thus neglecting the role of actions and material items [Hacking 1992, 54]. This interpretation is probably motivated by Duhem's strong emphasis on the theoretical character of the interpretations necessary for the use of instruments. My reconstruction should show, on the contrary, that Duhem, although not very explicitly, has taken into account the role of action and material items in experimenting and testing. Clearly, whatever is perceived or done can in principle be described and, *under this description*, becomes part of the host of hypotheses surrounding experimental activities; yet, it remains true that what Duhem's hypotheses are also about is actions and material items, and that he certainly does not treat laboratory devices as "(...) black boxes, as established devices that generate data which are literally given" [Hacking 1992, 53]. Hacking rightly says that "all action, all doing, all working is under a description" [Hacking 1993, 277], I would add that there is no better way to summarize Duhem's ideas on the matter.

ical reconstructions of scientific activities developed by his predecessors. Certainly, letting Duhem's crude distinctions go by the board allows us to achieve a more comprehensive perspective on knowledge and to discuss interesting phenomena such as holism and incommensurability also outside the domain of mathematical physics. But does this mean that within a satisfactory account of the way concepts deploy their descriptive power, laboratory activities shouldn't nevertheless be granted a different status with respect to naked-eye observations? Can't we look instead for a more fine-grained analysis of the different ways in which scientific concepts apply to the world? It is precisely on these grounds that, I believe, Duhem can still teach something to Kuhn.

As we have seen, Cartwright has urged that a distinction between observation and theory is needed. It is worth noticing that, in his answer to Cartwright, Kuhn accepts that distinction but he adds some important qualifications:

> I agree that the distinction is needed, but it cannot be just that between the "peculiarly recondite terms [of modern science and] those we are more used to in our day-to-day life." Rather, the concepts of theoretical terms must be relativized to one or another particular theory. Terms are theoretical with respect to a particular theory if they can only be acquired with that theory's aid. They are observation terms if they must first have been acquired elsewhere before the theory can be learned. 'Force' is thus theoretical with respect to Newtonian dynamics but observational with respect to electromagnetic theory. [Kuhn 1993, 246].

This passage renders even more apparent how Kuhn's point of view is language-centred. A term is theoretical with respect to the theory in which it is embedded and observational with respect to a (presumably) more complicated one, in which the term is nevertheless applied as an antecedently understood one. Clearly what Kuhn has in mind here is, once more, the language/theory learning process. In that process a previously understood vocabulary is necessary for learning new scientific concepts. A term is therefore observational, for Kuhn, if it belongs to a language, which provides the ground for further theoretical developments; the main advantage of this account being that it yields a *developmental* notion of the theory/observation distinction. At a certain stage, an individual or a community is endowed with some available dictionary, which of course is theory-laden for it makes sense in the context of the given language/theory in which it had been previously learned. This dictionary *happens* to be the contingent starting point for "further extension of both vocabulary and knowledge" [Kuhn 1993, 247], but, once the process of enlarging theories and dictionary get started, it becomes observational with respect to some new sets of terms. In short, there is no static, a-historical distinction between observational and theoretical terms, no original linguistic starting point, which lays stable foundations for the edifice of knowledge.

I believe that this solution can be accepted if what we are after is to understand how language is learned and how terms belonging to successive theories relate to each other, but it fails to provide an account of how different types of terms attach to nature. Terms such as "red" or "wood" on one side and "force" or "electrical resistance" on the other, in spite of their being all theory-laden in virtue of their embeddedness in a language, do have an intrinsically different status, for the former can and normally are applied on the grounds of simple perception, whereas the latter are concepts whose application requires acts of measurement. No developmental process, no paradigm shift, no new acquisition of knowledge and vocabulary can efface that difference or swap the roles of the two families of terms, for that difference is not grounded on language or theory, but in the different activities needed to apply them to the world. Indeed perceiving and measuring are essentially different performances of the knowing subject. To accept this does not imply a vindication of the duhemian cleavage between the *uncontroversial* but *vague* common sense facts and the *precise* but *hypothetical* theoretical facts, nor to resort to a kind of carnapian divide between the class of theory-free observational terms and the class of theoretical terms logically connected to the former via appropriate bridge principles[18]. I agree that all language is theory-laden and that also observational reports are fallible and relative to beliefs held at a certain developmental stage; nevertheless, I insist that there is a sharp difference in the way in which those terms or descriptions are attached to nature. A term like "quantity of heat" will never be observational in the way a term like "tree" is and this is the case for any term or taxonomy that, although not quantitative in itself, requires acts of measurement for its application[19]. At a particular historical stage, we are given a certain contingent conceptual network and we would be wrong in trying to split its vocabulary into an observational and a the-

[18]A succinct exposition on Carnap's ideas on the subject, which also pays attention to its evolution, can be found in Carnap's intellectual autobiography (see especially § 13), published in [Schilpp 1963]. A comprehensive reconstruction and a critical appraisal of the debate concerning the theory/observation dichotomy within logical empiricism can be found in [Parrini 2002].

[19]Venturing a long way outside the domain of physics, we find that a science like palaeontology is a source of examples of this kind (that are frequent also in physics, see section 7).

oretical part on the basis of a *linguistic* analysis, whether one (like that of Carnap) that purports to identify an intrinsic, unchanging distinction or one (like that of Kuhn) that aims at drawing theory-dependent and shifting dividing lines. What is needed instead, is an investigation of the various activities underlying the application of the conceptual terms (note that application does not reduce to learning but see further on this point) and an analysis of the intrinsic characteristics of these activities. The comparison between the sets of terms that must be applied in order to test sentences (1) and (2) of the previous section respectively should be interpreted in this way. The common sense taxonomy (or conceptual cluster):

(a) {sun, sunrise, air, warmth}

is different from the physico-mathematical taxonomy (or conceptual cluster):

(b) {pressure; electromotive force; increase in pressure by so many atmospheres; gas battery; increase in electromotive force by so many volts; relevant instruments; tools; conditions of the laboratory; actions of the experimenters; etc.}

because the activities needed to apply the concepts belonging to (a) amount simply to acts of perception, whereas those involved in the application of concepts such as "pressure" and "electromotive force" are, besides perceptions, complicated theory-laden instrumented actions aimed at measuring physical quantities. Those actions are therefore co-interpreted by the physico-mathematical taxonomy, which, for this reason, deserved to be called *dual*[20]. This explains why the concepts belonging to (a) do not refer to the acts of perceptions, nor to the sense organs needed to apply them, while in taxonomy (b) the actions and material items of the experimenter are taken together with the description of the physical properties and events. Physical theory, as Duhem says, unifies conceptually experiments that have nothing visible in common beyond what belongs necessarily to any structured perception of objects and properties, which implies that *the entities physical theory describes are classified if and only if the experimental contexts that reveal them are classified in turn.*

It might be objected that also the application of observational terms requires the fulfilment of some conditions concerning the context of the

[20] All physico-mathematical taxonomies are dual, but the converse needs not be true.

application. In order to decide whether what I see is a cat or a small dog, I need to get sufficiently close to the animal, I must have a well functioning perceptive apparatus, I must not be hallucinating or under the effect of drugs etc., the objection would then continue that those conditions are omitted from everyday observational reports, even though they must be fulfilled for the reports to be reliable and to count as valid applications of terms and descriptions to reality. Does this objection show that every taxonomy deserves to be called dual? Let us see. The reason why concepts referring to the act that allows the application of ordinary observational terms, are normally omitted is precisely that they would be in most cases identically the same. Observation terms are defined precisely by their being immediately applicable on the basis of that particular kind of act, which is unaided, normal perception. Our ready-for-use dictionary of objects, properties and states of affairs is not split in different sets in turn linked to specific conditions of application. In order to recognize everyday objects and their properties some conditions defining normal, reliable perception must be fulfilled and those conditions somehow contribute to the very meaning of the word "observation". Observation terms are, for a given speaker or community, those which can be readily used on the basis of simple perception. It is not the case that to the various taxonomies of cutlery, pets, home tools or road signals there correspond specific sets of concepts defining the conditions of their applications. What is peculiar to each of these taxonomies and makes it different from the others consists only in "objective terms" referring to reality, not in interrelated "act terms" that define appropriate conditions of application of the former. As we have seen, the contrary holds for physico-mathematical taxonomies. They contain "objective parts" composed by descriptive terms purporting to refer to the world and "experimental parts" which are highly interrelated to the former via the intermediary of physical theory. Each objective physico-mathematical conceptual cluster is a *semi*-dual taxonomy that demands a specific "experimental counterpart" for its proper application. It requires, therefore, the definition of a particular set of those "instrumented cognitive activities" that we call "experiments". This is not the case for observational taxonomies that hence cannot be called dual in a non-trivial sense.

Let us underline again, in conclusion, that the distinction between (a) and (b) is non-developmental, just as much as the difference between bare perceiving through the sense organs and performing instrumented manipulations in order to apply the language of mathematics to the external reality. It is true that measurement practices can develop only on the grounds of a pre-existing perceptual activity; but this is not the kind of "developmental character" that Kuhn refers to, and which is at issue here. According to Kuhn, a term like "electrical resistance" can be theoretical at a certain developmental stage and become observational (just like "tree") at a later one. I argue instead that this is impossible because of the physico-mathematical character of the concept "electrical resistance".

As we shall see, Kuhn's failure to notice this kind of distinction and its non-developmental character has repercussions on his description of the role of paradigmatic examples in the way scientific terms apply to the world.

6. Textbook problems and experiments

Can we really say that by solving paradigmatic textbook problems students learn to relate physical symbolic expressions to reality? In the light of the preceding considerations, we can say that an affirmative answer would be rash. A physics problem is still a purely theoretical exercise in which physical quantities appear as given from the outset. Even when concrete objects (such as a pendulum) are mentioned, their property are fully defined in mathematical terms, moreover each problem consists of the description of a "physical micro-drama" (such as a collision or the trajectory of a ball) which takes place in a world purified by all disturbing factors, a world reduced to a suitable physical description (see section 3). As Cartwright says (see section 2), "models are a long way from the world" and we need to step into a laboratory to actually see how to connect an abstract model (and, indirectly, the physical theory behind it) to reality. What was immediately given in the textbook suddenly becomes the result of painstaking investigations. The physical quantities are not waiting for us to use them in our calculations; they are to be found with the aid of some instruments, instruments that are normally omitted from the description of the problem. Moreover the elimination of disturbing factors cannot be simply postulated, it must be achieved. This is why students can often be very talented for solving problems, and yet feel completely lost in a well-equipped laboratory where they have a hard time recognizing that what happens before their eyes *is* the situation described in the textbook. Kuhn's account does not focus on this point. In various occasions he equates learning through problems with learning in the laboratory, for in both cases, according to him, it suffices to notice that students are exposed to some exemplars and that this allows the application of the theory to the real world.

Sometimes the exemplars discussed by Kuhn include standard measurement instruments such as galvanometers and manometers, but the analysis of their role does not lead to an acknowledgment of their privileged function with respect to theoretical learning in the application of physical language to reality, nor does Kuhn introduce the idea of a hierarchy of increasingly concrete and quantitatively defined exemplars. Had he done so, he would have probably paid more attention to the fact that during an experiment, physico-mathematical terms must be applied not only to the world, but also, at the same time, to the instrumented activities of the experimenters. In this way he would have appreciated the peculiar character of dual taxonomies with respect to perceptual ones. Similarly, in the *Postscript* some effort is made to distinguish the literal sense of the word "seeing" that is involved, for example, in ancient astronomy, from the "metaphoric" one that Kuhn adopts to account for complex experimental activities necessitating the use of instruments; but no satisfactory characterisation of the difference is actually provided. We can even say that Kuhn's later insistence of the language-dependence of theoreticity marks in some way a step backward with respect to some programmatic statements that can be found in the *Postscript* and in the article *Second Thoughts on Paradigms* [Kuhn 1974, 459-482], [Kuhn 1977, 293-319].

Why is Kuhn so reluctant to focus on the specificity of laboratory activities? I believe it is because he conflates two different processes: *learning or understanding a scientific term (or theory)* and *attaching it to nature*. Let us try to clarify this point. Certainly, as Kuhn says, in order to learn classical mechanics, we need to have in mind some examples of concrete mechanical systems. Nevertheless, various degrees of acquaintance with such examples must be distinguished. As Cartwright has noted (see section 2), we may find a series of increasingly concrete descriptions of an abstract, physical situation. Eventually, the desire for concreteness will inevitably lead us into a laboratory, where all the problems that have been mentioned will be waiting for us. Yet, if what we are after is only to learn and understand the theory, we can be content with a description of the physical situation, which occupies an intermediate position on the concreteness scale. It is at this level that most of the teaching, learning and communication in physics takes place. Classical mechanics can be understood without any laboratory session just with the aid of some more or less defined concrete situations that the teacher needs only to describe to the students, often in qualitative terms. The teacher, for example, can easily introduce the main ideas of the theory of collisions by referring to the well-known example of the billiard. He or she can then torment the students with all kinds of textbook problems, where mathematics will have the predominant role. Lecturing in front of a real billiard may have some pedagogic effect, but it won't change the nature of what is being done, that is, teaching and understanding. If on the contrary, we wished to test the mathematical predictions of the theory of collisions with a high degree of accuracy on the very billiard before us, we would immediately find ourselves lost in a duhemian jungle of instruments, careful checks and auxiliary assumptions. *It is only at this further stage that the mechanical concepts would authentically be applied to the world and not in the bare linguistic activities in which they are taught and learned*[21].

Theoretical physics provides us with other interesting examples. The Higgs boson is a particle postulated by the standard model of particle physics, which has not yet been detected. Therefore the concept "Higgs boson" has never been really applied to nature. Nevertheless, even if that particle happens to be found, the very event of its identification won't add much to the understanding theoretical physicists have of the standard model.

Could one really maintain that, as long as physicists are not trained experimenters as well as skilful problem-solvers, they do not really understand the theory? I suspect that today's theoreticians who, in the laboratories where their theory are being tested, often feel very much like a duhemian layman "ignorant of physics", would be rather upset by an affirmative answer.

7. An example of dual taxonomy

In this section I will illustrate the notion of dual taxonomy by means of a relatively simple example. The conceptual structure of modern physical theories is often far too complicated to be visualised with the aid of simple diagrams. As the degree of complexity cannot but increase if the experimental side is also taken into account, in my example I will only consider a small fragment of an already fairly robust taxonomy, namely that of crystalline lattice structures.

Within the class of lattices whose unit cell has a cubic shape, we find the subclasses of the *simple cubic* lattices (S.C.), the *face-centred cubic* lattices (F.C.C.) and the *body-centred cubic* lattices (B.C.C.). As it is well known, there are experimental procedures involving the diffraction

[21] Authentic application does not imply the *adequacy* to a mind-independent reality, but simply the presence of conditions that make possible the application of a term.

of X-rays that allow the determination of the lattice structure of various substances as well as the corresponding unit cell dimensions. In general, a suitably prepared specimen of the crystal under investigation is placed in a beam of monochromatic X-rays. The resulting diffracted beams produce a diffraction pattern, which is recorded on a strip of film. The study of the diffraction pattern provides the required information about the lattice structure. In the diagram given at the end of the present paper, the boxes in the vertical column represent the types of cubic lattices. For simplicity the subsumption of only one element (copper) under the corresponding type of lattice structured is represented.

The boxes representing objective properties and entities (the objective parts of the dual taxonomy) are drawn with continuous lines. The big arrow represents the subsumption of the element copper under the predicate "having a face-centred cubic lattice". The subsumption occurs if and only if an experiment is performed, which belongs to the class of equivalence of all possible experiments leading to the same conclusion (conceptually unified by physical theory). This class of equivalence can in turn be divided in types of experiments, thus giving rise to the experimental part of the dual taxonomy. A box drawn with dashed lines within the arrow represents each different type of experiment. We can imagine that in order to "reach" the box of F.C.C crystals, the element copper would have to "jump" into one of the boxes contained in the arrow. That is: for copper to be subsumed under the predicate "having a F.C.C. lattice structure", a sample of copper must be analysed with an experimental procedure in turn co-subsumed under a type of theoretically acceptable experiments.

It is certainly true that the specification of the experimental part of a dual taxonomy is highly problematic. For our purposes an experiment is defined as the set of all practical and intellectual operations necessary to achieve the final result from the choice, preparation and use of the experimental setting to the analysis of the data. There are clearly several ways in which the different types of experiments can be defined. In our example, the choice of the source of X-rays would be considered as an important discriminating factor and so would be the procedure used to render the radiation monochromatic and to measure its wavelength. But, would the kind of film or the method of development adopted be considered as defining different types experiments or would they simply determine minor distinctions between subtypes[22]? As a matter of

[22] In general such a classification will of course admit a complex branching of types of experiments into more and more precisely defined subtypes. In the diagram this hierarchical structure has been omitted for simplicity.

fact, no experiment can ever be completely described and in general, no experiment can be the exact repetition of another one: a great deal of tacit knowledge, of the kind students acquire only by imitating their teachers, contributes to determine the way an experiment is done and interpreted. Tacit *ceteris paribus* clauses intervene to wipe out the unmanageable number of little details that we deem without effect on the result. In short, the experimental part of a dual taxonomy is doomed to remain, up to a certain extent, sketchy and incomplete and some vagueness will inevitably affect the boundaries between types and subtypes of experiments. Difficult though it may be to fully specify taxonomies of experiments, it remains true that the description of the experiment is logically necessary for the application of physico-mathematical concepts to reality.

It must be stressed that, although a predicate such as "having a F.C.C. lattice structure" is not by itself the result of an act of measurement, it can only be applied if several physical quantities are determined (the wavelength of the monochromatic beam of X-rays, the exact position of the specimen in the camera, etc.). Moreover, as I have already said, experiments of the kind just described are normally made in order to determine also the unit cell dimension of the lattices, which can be derived by the exact position of the reflections constituting the interference pattern. It would be possible to take into account these further determinations in the diagram of the dual taxonomy, but at the price of increasing its complication. Each box in the vertical column would be split in an enormous number of little boxes corresponding to predicates of the kind "having a F.C.C. lattice structure with unit cell dimension 4.049 Å", "having a F.C.C. lattice structure with unit cell dimension 4.056 Å" etc. The number of such predicates would depend on the degree of precision that our experiments allow us to attain. Aluminium, for instance, would be connected to the little box corresponding to the first of the just mentioned predicates by an "experimental arrow" containing the class of equivalence of all experiments whose result is compatible with the correct value. The reason why, normally, predicates of this form are not considered "taxonomic" (in the strict sense of the word) is easy to understand. From a logical point of view there is nothing wrong in considering a crystal "having a F.C.C. lattice structure with unit cell dimension 4.049 Å" as *a kind of* crystal "having a F.C.C. lattice structure". However, it is clear that most of the classes corresponding to such predicates would be empty, for they far outnumber the existing elements and substances. As taxonomic kinds are normally introduced to describe in a simple and compact way a multiplicity of individuals, the ordinary quantitative determinations we encounter in physics do not have, in general, a classificatory function in the traditional sense. Yet it remains true that, in principle, also physico-mathematical values belong to the taxonomic lexicon of a theory: a force of 3 newton is a kind of force[23]. This fact is of crucial importance for understanding the common features shared by Kuhn's and Hacking's versions of incommensurability.

8. The ostensive character of theories

Yet one may think that something has been lost on the way. It may seem hard to deny that after all, physical concepts can and often are applied to the world also outside the laboratory. While holding a mug full of boiling hot coffee, we may say that some quantity of heat is being transmitted to our hand, and while seeing some sparks on a conductor, we may speak of electrical currents, electrostatic potentials and of the air's dielectric coefficient. Indeed physical theory is often used to explain a huge variety of phenomena that take place before our eyes. However, different types of application should be distinguished. Just as learning can proceed with the aid of some more or less concrete descriptions, the application of physical theory to actual situations admits degrees of *fulfilment*[24]. A sketchy explanation of an electrical phenomenon in qualitative terms cannot be equated with the precise quantitative determination of it that can take place in a laboratory. Physics can be taught in classrooms and used to formulate sketchy explanations or qualitative predictions in the "open air"; but it can be properly tested only through careful experiments.

Having said this, it must be added that the analysis of this problem can be satisfactorily carried out only by taking into consideration the role played by *ostension* in physics.

Especially in his later works, Kuhn's analyses of the function of exemplars in theory learning have heavily relied on the role of ostension. He takes ostension to be a procedure by which a particular object or situation is exhibited in order to explain the domain of applicability of a concept, without intending it as a radically pre-linguistic activity that would enable us to point at objects before any conceptual vocabulary is available (an antecedently understood vocabulary is, according to him, a necessary condition of theory-learning). The exhibits or exemplars

[23] From a logical point of view quantitative determinations are taxonomic in character in the strict sense defined by the "kind of" relation. Hence they belong *a fortiori* to the more loosely defined taxonomies that are at issue in this article.

[24] The term is borrowed, with some licence, from husserlian phenomenology.

have precisely the function of defining the similarity standards the students must acquire. This explains why a single ostensive episode never suffices to make out the meaning of a concept[25] and why ostension is always in some sense theory-laden: Ptolemy would point at the sun and at Jupiter in various occasions, he would remark their changing positions in the sky and would thus illustrate *his* concept of planet; Volta would draw or build a battery and then point at want he took to be an electric cell, etc.[26]. Kuhn's analysis is again both interesting and convincing but it has the already mentioned shortcoming of treating on an equal footing examples of conceptual changes occurred from the time of ancient astronomy to contemporary mathematical physics. Actually, he intends to give a language-based account of the meaning of kind terms in general where the conceptual couple cat/dog is mentioned along with conductor/insulator and planet/satellite. On the contrary, if the role of ostension is to be clarified, some attention must be paid to the different categories of objects and properties a given theory is about.

Ancient astronomy for instance, is a theory of a very peculiar character for it refers to the behaviour of a particular set of visible objects: the celestial bodies. From the point of view of modern science, Ptolemy didn't really develop a general theory, but an *ad hoc* model of a single complex system whose elements are identifiable through naked-eye observations (however complex they might be).

The decisive turning point didn't come with Copernicus but with Newton. It is only in the framework of classical mechanics that the solar system itself is replaced by an ensemble of mutually interacting masses whose behaviour is described by abstract laws. The resulting taxonomy is already physico-mathematical and, hence, dual. From the point of view of the domain of the theory, the change could not be more radical. The theory of gravitation is about the behaviour of masses, whether in the sky or elsewhere, and the very existence of the solar system (and even that of the fixed stars) is irrelevant to its validity. As long as we can identify objects endowed with mass, Newton's theory will be applicable. Clearly then, the role of ostension must have been consequently modified and the duality of the taxonomy will play an important role in the identification of relevant empirical situations. A concept that applies to direct visible objects, like "planet" for Ptolemy, is certainly more easily definable by means of ostensive procedures than

[25] Actually no finite set of ostensive episodes can suffice for the purpose. Meanings are always, in a sense, *unfinished*.

[26] Kuhn also mentions the importance of examples taken from the contrast set of concepts.

the abstract and mathematical notion of "mass". That is why complex exemplars are needed involving measurement instruments [Kuhn 1989, 66-74].

The theories developed along the history of science have generally become more and more abstract and mathematical, thus determining a growing role for dual experimental taxonomies with decreasing ostensive character. Newton's theory, or the theory of heat have still a phenomenal side that we could call in a loose way ostensive, for they refer to classes of objects or facts that belong also to common sense knowledge. With electromagnetism, this ostensive character has become even weaker. Finally, Twentieth Century particle physics has produced theories that do not refer directly in any way to something that can be observed. Their dictionary does not contain any term that belongs to common sense knowledge nor that is easily related to it. With respect to them, even Newtonian talk about masses seems after all to be referring to something open to our gaze. We cannot point at an object or event and be sure that what we are seeing has something to do with the predictions of the standard model of particle physics, for it is only background knowledge that can tell us if and when a pattern of observable facts embodies a model of the theory. The price to be paid for enriching our knowledge with extremely abstract and general theories lies in their remoteness from every-day observation.

Let us go back to the problem of the application of theoretical descriptions outside the laboratory. The idea is the following: *the more a theory is endowed with ostensive character, the more it can be applied, although often in a loose and qualitative way, outside an experimental context.* As science progresses, the role of ostension decreases and the gap between learning/understanding/theorizing on one side and experimenting on the other becomes bigger. Kuhn's exemplars usually refer to stages of the history of science during which that gap is not yet so dramatic as to render this fact evident.

Kuhn himself, in an article on the function of measurement in physics, situates a second scientific revolution between 1800 and 1850, at the time in which physics became fully mathematical [Kuhn 1961, 220]. Duhem writes after that revolution and has clearly understood some of his most relevant epistemological consequences.

9. Conclusion: Incommensurability and laboratory science

Let us reconsider the notion of incommensurability in the light of the preceding considerations. A passage of Kuhn describing the changes that taxonomies undergo during scientific revolutions will serve as the starting point of the discussion:

> When such a redistribution of objects among similarity sets occurs, two men whose discourse had proceeded for some time with apparently full understanding *may suddenly find themselves responding to the same stimulus with incompatible descriptions or generalisations*. Just because neither can then say, "I use the word 'element' (or 'mixture', or 'planet', or 'unconstrained motion') in ways governed by such and such criteria," the source of the breakdown in their communication may be extraordinarily difficult to isolate and bypass. [Kuhn 1970b, 173] (emphasis added).

Here again we find examples taken from different theories, regardless of the stage of the history of science to which they belong. This is not, however, what I intend to stress. Rather, I would like to focus on the fact that Kuhn treats incommensurable taxonomies as conceptual networks that cut up differently *a common fund* of sensory stimulations. The analysis carried out in this paper shows that this account of incommensurability can be applied to the shift from Ptolemaic to Copernican astronomy, or to other episodes involving theories with a strong ostensive character (section 8), but it need not be the correct account of incommensurability in laboratory science. Certainly, also in sophisticated mathematized sciences, it may happen that scientists belonging to rival schools are led to describe the same observable situation with different concepts, but this will rather be the exception than the rule. As we have seen, in a highly developed laboratory science, what is really observed during an experiment is replaceable in virtue of physical theory with items that have hardly anything visible in common (section 3). Different experimental settings and procedures are conceptually unified at a theoretical level by a dual taxonomy (section 4). Therefore two rival schools of physicists may accept different dual taxonomies that define distinct classes of equivalence of experimental settings and procedures. In this case the experimenters belonging to the two schools would not be exposed to the same stimulations, for their experiments would look different also to the layman "ignorant of physics". Incommensurability would take a different and more radical form.

It might be objected that after all, the adherents of one school could in principle step into the laboratories belonging to the other and try to understand and describe what happens there in the language of their theory and vice versa. In this case, it might seem that the situation would again be that envisaged by Kuhn: a common fund of sensory stimulations (in this case, those relative to the two sets of laboratory items) are interpreted in a different way in the light of two rival paradigms. It would seem that we could therefore hold on to the kuhnian idea that radical scientific change can be characterised, also in the case of laboratory science, as a change in the way language organizes experience.

The answer to this objection can be given by taking into account the peculiar structure of dual taxonomies. As we have seen (sections 5 and 7), a dual taxonomy has an objective and an experimental part. Now, as the diagram of section 7 intuitively illustrates with the graphic differentiation between static and dynamic classificatory arrays, what is classified by these two different parts has a specific logical and ontological status. The experiments are not elements or parts of the *one world* that both schools of physicists seek to understand, in the way objects, properties and processes classified by the objective part of a dual taxonomy are purported to be. They are not natural phenomena "to be saved", belonging to the *explanandum* of all rival theories whose adherents strive to render complete. Certainly, any experiment has a material or phenomenal side that demands an explanation of some kind, but, as we have seen, its cognitive relevance for a given theory is dependent upon the theoretical interpretation that the experimenter attributes to it. After observing the experiments that are performed by a rival school of physicists, one could simply be led to consider them as theoretically irrelevant for the actual controversy or even to disqualify them as valid experiments. For instance, disagreement over the shielding of the laboratory, the calibration of the instruments, the treatment of the noise in the analysis of data can determine different views about what counts as a valid experiment and what counts instead as a meaningless series of manipulations.

In his last works Kuhn does not seem to stress the role of sensory stimulations. Hopefully this is due to a gradual departure from the naturalism that lurks behind his epistemology. However, reference to stimulations is unnecessary for his account of incommensurability and for the objections to it that have just been presented. Two contemporary rival theories with a strong ostensive character certainly share a basic vocabulary that can play the epistemological role of "the same stimulus" mentioned in Kuhn's last quotation. This shared vocabulary can provide the ground for the comparative assessment of the two theories[27].

[27] See [Soler 2003] where it is underlined that, if the rival theories in question are not contemporary, the "common observational vocabulary" belongs to a meta-

As the ostensive character decreases, the basic vocabulary available for comparison becomes thinner or even empty and the sharing of measurement instruments and procedures will instead play the predominant role in the comparison.

The extreme situation in which two rival theories are supported by disjoint sets of measurement procedures is precisely that described by Hacking and named "literal incommensurability". Hacking, as we have seen (section 1), treats separately this kind of incommensurability from the one that concerns the relation between taxonomies pertaining to rival theories. The aim of my analysis has been to show that these two kinds of incommensurability can be accommodated in an enlarged taxonomic formulation that takes into account the competing roles of ostension and dual taxonomies in the way theories refer to the world. The central idea of this enlarged taxonomic version can now be introduced:

Incommensurability results, in general, from a deep transformation of the taxonomy pertaining to a theory, where the term "taxonomy" can refer both to dual taxonomies and to more descriptive ones. In the case of laboratory science, this transformation implies the replacement of a dual taxonomy with a new, incompatible one[28].

The classical example of lexical incommensurability provided by Ptolemy's and Copernicus' astronomies and the examples of literal incommensurability proposed by Hacking and Pickering refer to theories that are extreme, opposite cases within an ideal classification of theories with respect to the strength of their ostensive character and the inversely proportional duality of their taxonomies: the difference between the two apparently incompatible types of incommensurability is only one of degree. The experimental part of the taxonomy pertaining to descriptive sciences is often almost trivial, whereas that of contemporary physics is hypertrophic[29]. Once reconsidered in this way, therefore, also Hacking's literal incommensurability can be seen as a case of taxonomic incommensurability. The situation he envisages is one in which successive theories differ radically not only with respect to the objective part of their dual taxonomies, but also with respect to the experimental part. The objective part would consist of physico-mathematical predicates: ei-

language developed by the historian of science, a meta-language that is not theoretical with respect to the incompatible assumptions of the two theories. The common observational sentences thus introduced need not have actually been uttered by the adherents of the two paradigms.

[28] In what way the *degree* of incompatibility of two dual taxonomies should be evaluated is a problem that I will not discuss here.

[29] This is, I believe, the way in which Duhem's distinction between different types of sciences should be rephrased.

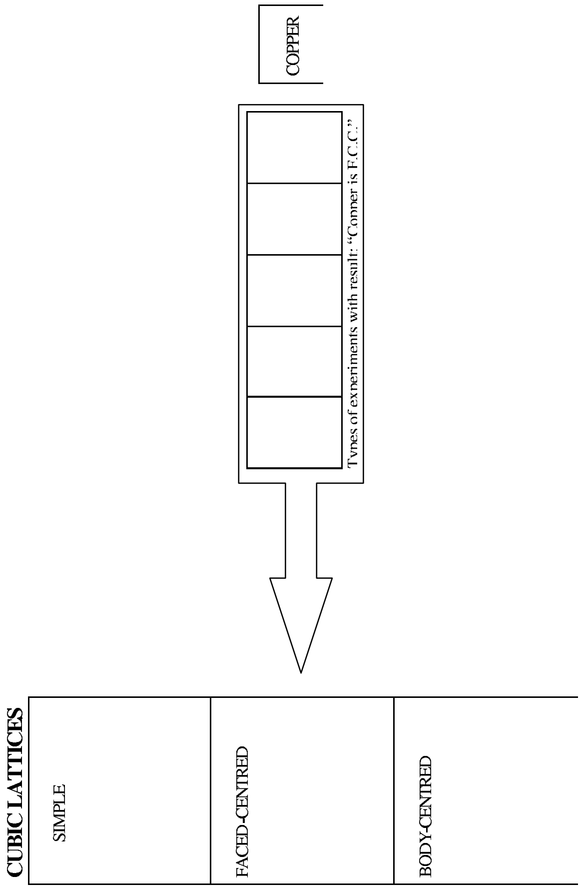
ther physico-mathematical values, or predicates applicable only on the basis of measurement acts. The experimental part would instead consist of types of laboratory practices and measurement procedures ordered in equivalent classes allowing the application to reality of the corresponding elements of the objective part. Note that considering the results of measurements as descriptive predicates belonging to the objective part of the dual taxonomy is essential to this taxonomic reformulation of literal incommensurability (see end of section 7): measurement procedures are conceptualizing procedures. Finally, let us not lose sight of the fact that Hacking has considered an extreme situation. Less dramatic cases, in which two competing theories share some elements of the objective and of the experimental part of their dual taxonomies are more likely to occur.

Kuhn has often urged that:

> (...) men who hold incommensurable viewpoints be thought of as members of different language communities and that their communication problems be analyzed as problems of translation. [Kuhn 1970b, 175].

In the light of the preceding analysis, it can be maintained, I believe, that this account of incommensurability is too narrow. A scientific community cannot be seen, in general, simply as a language community; it must be characterised also in terms of the kinds of experimental contexts and practices through which its theories are applied to the world. After a scientific revolution, scientists do not just "tell a different story" about the world, they also modify their experimental practices and develop new technical and conceptual means for interacting with reality: modern science is a cognitive enterprise to which both language and practice are essential[30]. It is with respect to the complex interplay of these two elements that a role for the problematic of translation in the discussion of radical scientific change still needs to be found.

[30]Sometimes Kuhn acknowledges the crucial role of technical means in the mediation between reality and theory (see, for instance, [Kuhn 1993, 246]) and Hacking underlines that he intended to give an account also of kinds of instruments and experiments [Hacking 2003, 390]. It is a pity that these insights should have never been consistently integrated into his account of the nature of incommensurability.


COPPER
Types of experiments with result: "Copper is F.C.C."
CUBIC LATTICES
SIMPLE
FACED-CENTRED
BODY-CENTRED