

Introduction

The Incommensurability Problem: Evolution, Current Approaches and Recent Issues

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The idea that competing scientific theories may be incommensurable was introduced in philosophy of science in 1962, simultaneously and independently by Thomas Kuhn and Paul Feyerabend¹. Thereafter, incommensurability promptly became one of the most central and controversial issues of 20th century philosophy of science. Half a century later, how has this issue evolved? The present volume of *Philosophia Scientiae* aims at providing the elements of an answer to this question.

I will begin by stating the problem as it can be formulated today, from a point of view that allows us to distance ourselves from the most significant misunderstandings involved in the earliest discussions on incommensurability² (and unfortunately still present in some recent works). This choice will allow us, at the same time, to profit by some useful distinctions that are nowadays available, but that were elaborated only at the price of careful and thorough analyses carried out over the past forty years.

¹[Feyerabend 1962]; [Kuhn 1962]. On Kuhn's conception of incommensurability and its evolution, see [Hoyningen 1989]; [Hoyningen 1990]; [Sankey 1993]; [Hoyningen 1998]. For recent books on Kuhn considering incommensurability as a particular aspect of Kuhn's thought, see [Bird 2000]; [Laugier 2003]; [Nickless 2003]; [Read & Sharrock 2002a]. For a precise comparison of Kuhn's and Feyerabend's conceptions of incommensurability, conducted both at the historical and at the analytical levels, see [Hoyningen 2004].

²For a list and an analysis of such misunderstandings, see [Hoyningen 1989, section 6.3].

Yet, before beginning to state the incommensurability problem from a contemporary perspective, a brief clarification seems desirable if not required. The term ‘incommensurability’ often operates in certain areas of philosophy of science, today just as much as in the recent past, as a *repulsive-term*, whose rhetorical function is to stigmatize some rejected relativist-antirealist-sociological combination. As a result, a simple willingness to talk about the ‘incommensurability problem’ and to manifest interest in it is often equated with the *acceptance* of a relativistic-antirealist-sociological position, in a sense that remains vague but carries nevertheless the precise accusation of denigrating science up to an intolerable point. This may encourage to talk of the incommensurability/*commensurability* problem instead of the incommensurability problem *tout court*, precisely in order to stress that *there is* here a philosophical *question* — the only commitment implied being the conviction that such question potentially offers something important and interesting to think about. Now, I will not systematically use the rather unwieldy locution ‘incommensurability/*commensurability* problem’, but it is in this spirit that I approach the problem.

The Incommensurability/Commensurability Problem

Let us now state the incommensurability/*commensurability* problem³.

At the most general level, it is the problem of characterizing a particular kind of deep transformations occurred in the history of science. The main related epistemological issues coincide with some central issues of 20th century general philosophy of science, namely, scientific realism, scientific rationality and scientific progress.

It is not by chance that such problem became increasingly important in the course of the last century. Indeed, the 20th century saw the development of new scientific theories showing unexpected characteristics that seemed to break in many central respects with earlier theories. This was notably the case in physics⁴, that is, in the field functioning at the time as the paradigmatic object of epistemology. These deep transformations have appeared to some philosophers as a refutation of scientific realism; have led them to weaker and weaker conceptions of scientific progress

³For panoramic presentations of the problem, see [Hoyningen & Sankey 2001, vii-xxx]; [Sankey 1994a]; [Sankey 1997b]; [Soler 2000a, 180-191]. These references are most of the time useful with respect to the different particular aspects that will be examined below, although I will not systematically repeat them each time.

⁴Especially with quantum physics. See for example [Bitbol 1996], [D’Espagnat 2002].

(so weak that their foes equate them with the claim that there is *no* scientific progress); and have correlatively led them to deny that there may be rational grounds for the judgement that one theory is objectively better than another.

To go a little bit further, **two kinds of scientific changes, and correlatively two kinds of incommensurabilities**, have been progressively recognized as different in principle, and are currently distinguished in contemporary philosophy of science: the so-called ‘semantic’ and ‘methodological’ ones.

1. Let us begin with **changes** (and possibly **incommensurability**) arising **at the level of theoretical contents** (or at the level of content *tout court*⁵). They are most of the time called semantical changes, and I will also label them ‘descriptive changes’⁶.

These changes concern what rival theories ‘say’ about the object under study. They concern ontological changes in the broader sense of

⁵I use the expression ‘content tout court’ because I think that, at the level of contents, a generic expression in which the adjective ‘theoretical’ does not appear is needed. This in order to include, within the category of ‘contents’, propositions that the philosopher of science supposes to be assumed by scientists *but that he is reluctant to label ‘theoretical’*. My concern, here, is to free as much as possible the characterization of incommensurability from the ambiguities and disagreements associated to the terms ‘theory’, ‘theoretical’, etc. Contemporary philosophy of science shows that the usages of the word ‘theory’ are not at all homogeneous, and that there are disagreements about what can be legitimately labeled ‘theoretical’. Such disagreements and shifts in usage can be understood as a undesirable effect of some (in themselves beneficial) insights of the last century philosophy of science. The now widely accepted thesis of the theory-ladenness of any observation, the recognition that measurement instruments are ‘materialized theories’ (according to Bachelard’s famous expression), and many other assumptions of the same kind, had together the effect that *any* aspect of scientific practices — experimental instrumented actions and manipulations included — can *in a sense* be said ‘theoretical’. But in that case, the label ‘theoretical’, being unable to discriminate anything, is useless. And if, taking the later conclusion into account, we accept that non-theoretical contents (or relatively less theoretical contents) are involved in scientific practices, we have to decide what we are ready to call non- or less- theoretical. Taking all that into account, I am looking for a conception of ‘contents’ sufficiently wide to cover, if required, propositions not extracted from systematic high level theories. Propositions such as, for example, more or less implicit — but up to a point articulable — assumptions about the working of material devices involved in laboratory life.

⁶Although ‘semantic changes’ and ‘semantic incommensurability’ are the standard expressions found nowadays in the specialized literature, I prefer to talk of ‘changes in (theoretical) contents’ or ‘changes in descriptive contents’, in order to avoid possible ambiguities resulting from the fact that the semantic lexicon is used with a peculiar technical meaning in particular areas of our philosophy of science (for example the structural approach of science) or in fields closely linked to this philosophy (for example logic).

the word, that is, changes in the kinds of beings and processes supposed to populate the world according to the theory.

Content changes are often analysed relying on a linguistic theory that distinguishes meaning and reference as well as intension and extension. At the level of meaning, we have the well known problem of ‘meaning change’, that comprises the question of understanding how such changes arise, how to delimitate the meaning units and legitimately conclude that they are sufficiently equivalent (synonymy) or significantly different (genuine meaning change), and with what kinds of epistemological consequences. And at the level of references, we find notably the question of determining whether references of key scientific terms have indeed changed in the actual history of science, as well as the question of elaborating theories of language according to which reference is sufficiently independent of meaning to allow reference invariance despite of drastic meaning change⁷.

2. Side by side with changes of contents, there may be **changes** (and possibly **incommensurability**) arising **at the level of the norms of research**. They are most of the time called methodological changes, and I will also label them ‘axiological changes’.

These are changes in the answers (often tacitly) given to questions of the kind: what is the aim of scientific research? What are the central features of a genuine investigation, of a genuine scientific theory, or of a genuine scientific explanation? What kinds of problems and solutions are authentically scientific, and what kinds are not? What are the features of a good proof, what is a convincing argument?

In short, the category ‘norms of research’ comprises the whole set of values underlying scientific investigation. Changes at the level of ‘norms of research’ covers all types of axiological changes⁸, from the most local and apparently anecdotal ones, to the most global and visibly consequential ones that culminate in massive transformations of the general

⁷See below for more details, and notes 15 et 35 for references.

⁸That is why I prefer to talk of ‘norms of research’ rather than of ‘methodology’ (or ‘methodological changes’, or ‘methodological incommensurability’), despite the fact that the later expressions, and not the former, are commonly employed in the specialized literature. It is of course always possible to demand that ‘methodology’ be understood in a broad sense, so as to be able to meet all kinds of axiological changes. However, ‘methodology’ is usually understood in a narrower and stronger sense (according to which one could be reluctant to include, for example, exemplars in the Kuhnian sense). Moreover, the ‘methodology’ lexicon has quite inevitable undesirable connotations, being associated to the (now famously refuted) idea of universal algorithmic rules and procedures allowing to prove scientific hypotheses and to choose between scientific theories.

idea of scientificity itself.

3. **The incommensurability of contents**, or descriptive incommensurability, is more currently named ‘semantic incommensurability’ or again — in a somewhat particular interpretation of what is at issue — taxonomic incommensurability. It designates important differences of a particular type between competing scientific theories, scientific paradigms or scientific traditions.

The problem of the incommensurability of contents⁹ has several facets.

First it consists, adopting a synchronic approach to compare two historical cross sections artificially extracted from the continuous flow of the history of science, in achieving a fine-grained characterization of the descriptive differences at stake. Roughly speaking, these differences manifest an incompatibility that is not reducible to a simple logical contradiction. This incompatibility is apparently related to differences in the linguistic (and more generally symbolic) resources themselves, so that intuitively, one is inclined to say that there is *no common measure* at the very level of what is thinkable, expressible or symbolizable.

Such an incompatibility has been characterized by the late Kuhn, and is currently described in recent works, as the impossibility of translating the words of one theory (or more exactly *some* key words of one theory) into the vocabulary or the symbols of the other. It is in such framework that the label ‘taxonomic incommensurability’ is used¹⁰.

Kuhn’s conception of taxonomic incommensurability is deeply grounded in a definite holistic conception of language. Roughly speaking, Kuhn’s idea — here largely re-expressed in my words — is that the set of signs used by scientists involves non-decomposable sub-systems. These sub-systems are non-decomposable, in the sense that they are clusters in which all terms *reciprocally delimit* their content, so that none of them can be determined *without recourse to the others*, nor, therefore, be taken apart without being deeply denatured. In other words, each single signifier is defined by its relations to other signifiers. Put differently, each signifier takes its sense (or — depending of the chosen framework, and with nuances that will be ignored here — each takes its meaning, or conceptual content, or intension, or linguistic value. . .) from the structure

⁹See references note 3.

¹⁰The expression ‘taxonomic incommensurability’ seems to have been introduced by H. Sankey in a paper headed, precisely, “taxonomic incommensurability” [Sankey 1998]. In addition to the references given in note 3, see [Bird 2000, chapter 5]; [Chen 1997]; [Hacking 2003]; [Kuhn 1983a]; [Kuhn 1991]; [Kuhn 1993]; [Sankey 1990]; [Sankey 1991b]; [Sankey 1992]; [Sankey 1998]; [Soler 2000b]; [Soler 2003]; [Soler 2004b].

of the vocabulary (possibly including observational terms¹¹) at the heart of which it lies. Such a fragment of taxonomy, also called by Kuhn a “lexicon”, encapsulates a network of similarity and dissimilarity relations that are applied to the world as a whole and determine what is of the same kind and what is of different kind. Kuhn talks about “local holism” [Kuhn 1983a]. In this framework, “taxonomic incommensurability” designates the non-homology — or the impossibility of superimposition — of the two lexicons underlying the two theories under comparison. Kuhn insists — and this is in my opinion a very important point not always integrated by people working on incommensurability — that this non homology, which defines incommensurability and is responsible for the untranslatability, does not at all mean the impossibility of mastering the incommensurable theory. One can become bilingual, even if it requires effort and even if it is more usually the task of historians of science than the one of scientists. And once bilingual, it is possible to understand the rival incommensurable theory although it is still impossible to translate the words of one into the words of the other¹².

Second, the problem of semantic incommensurability also consists in the discussion, again in a synchronic perspective, about *how deep* the differences of contents encountered in the actual history of science are and about whether invariants can be found.

Third, the problem of semantic incommensurability understood in the broader sense requires the examination, this time in a diachronic approach concerning transformations that arise along a temporal line, of the historical reasons that have led to major descriptive bifurcations. It requires the analysis of their nature and the evaluation of their cognitive value

The most obvious and most debated epistemological issue pertaining to the incommensurability of scientific contents is the problem of scientific realism¹³. The simple claim that there are scientific revolutions at the descriptive level, i.e. the claim that there are, in the actual history of science, massive and deep ruptures at the level of ‘what science tells

¹¹‘Observational’ has of course to be understood here in a contextually determined and pragmatic sense : in the sense of ‘taken to be observational for a given (individual or collective) subject at a given stage of knowledge’, that is, ‘taken to name unproblematically directly perceptible and given states of affairs’. Kuhn is ready to maintain the distinction observational/theoretical, provided that one understands the boundary as an historically-moving one (see [Kuhn 1993], and the analysis given by Emiliano Trizio in the present volume).

¹²[Kuhn 1983a]; [Sankey 1991c].

¹³[Sankey 1994a]; [Sankey 1997c]. For recent contributions especially centred on incommensurability and realism, see for example [Brown 2001] and [Devitt 2001].

us about the world', is, in itself, already a threat for any variety of correspondentist or convergent realism. Yet, the more precise claim that these descriptive scientific disruptions originate from deep differences rooted in the very resources of what is expressible, is far more subversive, since it incites to question the very formulation of the problem of the relation between human theories and their non-human referent. At least, it forces us to face seriously the idea of a constitutive power of language in science. And the positions adopted concerning such questions engage determined positions about scientific progress¹⁴.

4. **The incommensurability of research-norms**, or axiologic incommensurability, is more currently called 'methodological incommensurability'. It designates important differences, tensions and incompatibilities between the normative idea of scientificity underlying two rival research traditions: differences between the conceptions of what is a genuine science, of what is a sound scientific explanation, of what is a scientifically acceptable proof or convincing argument, and so on.

The problem of the incommensurability of research-norms¹⁵ consists, first, in the search for a fine-grained analytical characterization of such differences.

Second, it consists correlatively in the discussion — conducted from a synchronic perspective contrasting two frozen pictures isolated from our history of science — about *how deep* are the axiological transformations encountered in the practices that we are ready to categorize as 'science' — considering the actual human history and the evolutions of the normative idea of scientificity that this history manifests. An important aspect of such discussion corresponds to the search for invariant axiological features that human activities should possess to be legitimately labelled 'scientific'. Such a quest today no longer amounts to the question about the existence of a universal and quasi-automatic scientific method enabling to find the truth, but it is, no doubt, an heir of that question reshaped in a way that allows the integration of the teachings

¹⁴On incommensurability of contents and the intertwined questions of meaning change, reference change and realism, see for example: [Achinstein 1964]; [Andersen 2001]; [Andersen 2002]; [Baltas 1990]; [Bartels 1995]; [Bird 2000]; [Carrier 2000]; [Carrier 2001]; [Carrier 2002]; [Feyerabend 1965a]; [Feyerabend 1987a]; [Field 1973]; [Kitcher 1978]; [Kitcher 1983]; [Kordig 1971]; [Kroon 1985]; [Kroon 1987]; [Kuhn 1983a]; [Kuhn 1983b]; [Kuhn 1984]; [Kuhn 1987]; [Kuhn 1989]; [Kuhn 1990]; [Kuhn 1991]; [Kuhn 1993]; [Nercessian 2001]; [Nola 1980]; [Norris 1997]; [Papineau 1979]; [Papineau 1996]; [Putnam 1973]; [Putnam 1975a]; [Putnam 1975b]; [Rasmussen 1987]; [Sankey 1991d]; [Sankey 1994a]; [Sankey 1997b]; [Shapere 1966]; [Soler 2000b]; [Soler 2003]; [Soler 2004a]; [Soler 2004b].

¹⁵For an overview, see [Sankey 1997a].

of the post-positivist philosophy of science. In any case, we cannot completely avoid the question of the axiological features that are ‘essentially’ or ‘analytically’ associated with science — even if the later adjectives are most of the time understood today in a evolving and pragmatical sense referred to the scheme of family resemblances rather than to the one of necessary and sufficient conditions.

Third — and maybe more important with respect to epistemological consequences that have been, in the history of philosophy of science, intimately associated with incommensurability –, the problem of axiological incommensurability corresponds to the task of characterizing, on a diachronic axis, the historical reasons having led to major axiological bifurcations.

The central epistemological issue commonly associated with the incommensurability of scientific norms is the problem of relativism. Relativism is here understood as the problem of knowing whether human beings — and in particular human being categorized as ‘scientists’ — have at their disposal genuine justifications — or at least sufficiently good reasons — for comparing the merits of incompatible systems of cognitive norms, and especially in order to decide what is good or what is better at the level of validation procedures. The term ‘incommensurability’ refers here to the idea that no universal ultimate standard of that sort is available, so that there is ‘no common measure’ between different incompatible axiological options, in the sense that there is in principle no ultimate legitimate tribunal before which all different options should appear in order to be situated on a trans-historic scale of ‘cognitive satisfiability’. In such an epistemological configuration, widely perceived as dramatic by philosophers of science, one is quite irresistibly inclined to conclude, from the human impossibility of deciding in the absolute what is cognitively better, to the idea that all human practices and elaborations are on the same level and have the same value. In other words one is inclined to conclude — with despair or delectation according to one’s temperament — that relativism is inescapable. Following these lines, axiological incommensurability seems to imply relativism, and the former has often been equated with the later.

Philosophy of science at the end of the 20th century showed that such a conclusion is not at all necessary — although the terminology of incommensurability remains widely and quasi-automatically associated with the adoption of a relativistic stance. This conclusion looks necessary only against the background of a (more or less tacit) foundationalist framework. Yet, several non-foundationalist strategies have been proposed in response to radically sceptical conclusions of the kind explained above:

forms of naturalism, revised conceptions of transcendentalism, new experimentalism. . . These approaches led to non-foundationalist and non-realist – or realist in a shifted sense — accounts of scientific progress¹⁶.

5. It is worth rendering explicit that **the distinction between the two kinds of incommensurability**, although analytically very useful for it allows us to deal with the questions one by one, is in most real cases *not* a separation *in re*. Actual historical situations show complex interactions between the available scientific contents and the scientific cognitive values shaping the idea of scientificity in a given state of knowledge. It remains nevertheless possible, at least as a step in a philosophical research program labouring under the generic label of incommensurability, to isolate and explore separately circumscribed problems coming more particularly under one of the two headings of descriptive or normative incommensurability.

It has moreover to be said that the methodological versus semantic incommensurability distinction is not just an analytical distinction between two forms of incommensurability. It is also a distinction grounded in the social fact that there are two separate literatures on incommensurability. The main literature that normally goes under the head of incommensurability is about semantic issues like meaning variance, reference change, translation failure, etc. There is, however, an even larger literature on variation of methods, standards, norms, criteria, etc., which surrounds the topic of rational theory choice and epistemological relativism and which thus concerns what have been called above ‘methodological incommensurability’, but this literature does not currently use the term ‘incommensurability’ — although Kuhn himself started it with his talk of ‘incommensurability of standards’ in [Kuhn 1962]¹⁷.

6. Machinic-literal incommensurability.

Semantical and methodological incommensurability exhaust the kinds of incommensurability commonly mentioned in most contemporary systematic global presentations of the problem. However in the last decades,

¹⁶On the problem of axiological incommensurability and the intertwined questions of norm variance, rationality and relativism, see for example: Doppelt’s papers mentioned in the bibliography; [Feyerabend 1975]; [Feyerabend 1983]; [Feyerabend 1987b]; [Forster 2000]; [Hoyningen & Sankey 2001, 159-205]; [Kitcher 1983]; [Kitcher 1993]; [Kuhn 1962]; [Lakatos & Musgrave 1970]; Laudan’s papers mentioned in the bibliography; [Nola & Sankey 2001]; [Nola & Sankey 2000b]; [Sankey 1994b]; [Sankey 1995]; [Sankey 1996a]; [Sankey 1996b]; [Sankey 1997a]; [Shapere 1984]; [Shapere 2001]; Siegel’s papers mentioned in the bibliography; [Zheng 1988].

¹⁷Contributors to the latter literature are people like Laudan, Doppelt, Siegel, etc., whereas contributors to the former literature are for example Putnam, Devitt, Kitcher, etc. (see references in the bibliography).

authors like Andrew Pickering and Ian Hacking claimed to have discovered “a new and fundamental type of incommensurability”¹⁸ ignored by traditional philosophers of science.

Such an incommensurability concerns competing scientific practices having stabilized on the basis of different measurement processes, instruments, machines, and laboratory practices, so that at a point they became, in Hacking’s terms, “literally” incommensurable, in the sense that there is, properly speaking, *no shared physical measure* between them. Pickering calls the same kind of configuration “machinic incommensurability” [Pickering 1995, 189]. The relation between the 1960s and the 1970s particle physics offers for example, according to Pickering, a striking example of machinic incommensurability¹⁹.

The idea of literal or machinic incommensurability has been relatively recently introduced and has not been widely discussed — at least with reference to the traditional incommensurability problem and if we compare with the immense literature devoted to traditional forms of incommensurability. Pickering and Hacking presented it as new and introduced it in contrast with language-based characterizations and theory-dominated orientations — claiming for example that it “has nothing to do with ‘meaning change’ and other semantic notions that have been associated with incommensurability” [Hacking 1992, 56-57].

However, the novelty of what is at stake and, more important, its exact relations with the traditional forms of incommensurability, deserve in my opinion more discussion²⁰. As far as I can understand the situation on the basis of the fragmentary investigations already undertaken, my provisional conclusion is the following. ‘Machinic-literal incommensurability’ is not a new *kind* of incommensurability, in the sense that

¹⁸[Hacking 1992, 54]. Also quoted by [Pickering 1995, 187].

¹⁹For the detailed study of the historical case, see [Pickering 1984]. The relations between such historical case itself and incommensurability is first characterized in [Pickering 1984, section 14.3]. For later developments and re-qualification in the framework of Pickering’s conception of the “Mangle of practice”, see [Pickering 1995]. It is only in that last book that the expression “machinic incommensurability” appears. For studies not all especially centered on incommensurability but that provide elements connected with the idea of a ‘pragmatic incommensurability’ arising at the level of experimental practices, see also [Ackermann 1985], [Bensaude-Vincent 1993], [Bensaude-Vincent & Stenger 2001], [Buchwald 1995], [Collier 1984], [Galison 1987], [Gooding 1992], [Hacking 1983], [Hacking 1988], [Hacking 1999], [Latour 1997], [Pickering 1994].

²⁰I recently examined the question in three talks: a first one given in Montreal in September 2003 at a conference organized by the SOPHA (now submitted for publication); and in two later talks given in October and March 2004, in the context of my seminar devoted to the incommensurability problem at the Collège International de Philosophie, Paris.

what it refers to does not require the introduction of a third additional category side by side with those of incommensurability of contents and incommensurability of norms. However, the study of what is at stake may introduce some novelty in the classical characterization of the incommensurability problem, since ‘machinic-literal incommensurability’ involves a shift in the grammatical subject of incommensurability. Indeed, what is classically termed ‘incommensurable’ is ‘something’ at the level of *theoretical* practices, whereas here, it is ‘something’ at the level of *experimental* practices (the problem being then: what exactly? With what epistemological consequences? And: do such consequences legitimize the recourse to the incommensurable lexicon?).

7. Understood according to the presentation given above, the incommensurability problem covers a broad and complex network of questions. Thus choices had to be made concerning the **composition of the present volume**. The volume takes incommensurability of contents as the starting point, and it is led from there to touch on other dimensions or species of incommensurability. Two papers are especially devoted to machinic-literal incommensurability. However, axiological incommensurability is not directly tackled and remains more or less in the background²¹.

Such a choice follows, in a way, the historical movement of the incommensurability debate — hoping that this will serve the aim of understanding better the present state of the problem. Indeed the incommensurability problem, after its introduction and the early attempts of clarifications in the sixties, progressively shrank, in the actual discussions of philosophers of science, to the more restricted problem of semantic incommensurability. This arose notably under the influence of Kuhn’s later stress on language, taxonomic structures and translation, as a sort of retarded incidence of the so-called ‘linguistic turn’. Philosophers of science did not, in this movement, completely abandon the constellation of questions listed above under the heading of ‘axiological incommensurability’. But they did not establish a systematic and explicit association between these questions and the incommensurability label²². Now in recent decades, the previously circumscribed incommensurability problem has been re-enlarged afresh, with attempts to include in the picture the instrumental, experimental layer of scientific activities, this time in the broader context of the so-called ‘pragmatic turn’.

²¹For a recent book on incommensurability making room to methodological incommensurability side by side with semantic incommensurability, see [Hoyningen & Sankey 2001].

²²See [Sankey 1994a].

Current Approaches to Incommensurability

Before presenting one by one the different papers constituting the present book, let us have a brief overview of the main methodological options nowadays available and alive in studies on incommensurability. Without claiming that the contributions of the book offer an exhaustive sample of the existing options, I have been anxious to make room to different and often opposed ones.

Not surprisingly, the main trends constituting contemporary reflections on science are also found in special studies focused on the incommensurability problem. We will consider here, first the analytical versus historical ways of examining science, then the linguistic, pragmatic and cognitive approaches of incommensurability, and finally, more specific and sometimes overlapping orientations such as the Wittgenstein-inspired one.

1. The distinction between **the analytical and the historical approaches** of science has often been thought, especially in the anglo-saxon world before the sixties, as an opposition and a quite strong one. Even today, it is still conceived as an opposition in certain areas of philosophy of science. However, this general tendency became less and less important within the group of philosophers studying incommensurability. Within this group the analytical and the historical determinations widely appear today to be two complementary and required approaches rather than two mutually exclusive ones. Almost all authors working on incommensurability today are anxious to examine available historical case studies (if not to explore themselves particular historical scientific episodes), in order to achieve a characterization of incommensurability both inspired by and conformed to historical ‘data’ about scientific development. One of the ‘fathers of incommensurability’, namely Kuhn, himself contributed very much, as it is well-known, to the progressive recognition, in the anglo-saxon world, of the necessity of taking the historical dimension into account when tackling many central questions of philosophy of science and notably the incommensurability problem²³.

2. The **linguistic approach** has been adopted by Feyerabend in his early characterization of incommensurability²⁴, as well as by the late Kuhn²⁵, and it is through it that incommensurability has been most widely and deeply explored. In the present volume, this approach is typically represented by the works of Alexander Bird, Martin Carrier

²³See especially [Kuhn 1962] and [Kuhn 1992].

²⁴See reference note 1.

²⁵See references note 10.

and Léna Soler. The papers of Hanne Andersen, Soazig Le Bihan and Emiliano Trizio, although they are less prototypical of works committed to the linguistic approach and although they explicitly aim at considering non-linguistic aspects of incommensurability, are also closely linked to the linguistic dimension in several important respects.

The linguistic approach to science is often accused of reducing science to language or of being too restricted to achieve a sound characterization of science. However, it is in my opinion neither fair to much work focused on the linguistic dimension, nor philosophically fruitful, to equate it with the positive strong thesis that science is essentially a linguistic reality and nothing more. The emphasis on the linguistic dimension may more productively be understood as a methodological orientation aiming at characterizing an important but not exhaustive dimension of the studied object. This being said, to choose to characterize incommensurability by a linguistic approach is of course not a neutral choice. Assuming that it is coherent, such a choice presupposes the minimal belief that incommensurability of scientific contents corresponds to drastic linguistic changes.

The task is then to analyse such changes with the help of a suitable theory of language: to find or elaborate a linguistic framework able to account for the phenomena associated with incommensurability. We can classify the possible theories by means of schematic traditional oppositions, such as atomistic versus holistic conceptions; extensive versus intensive characterizations; non-descriptive models centred on reference (the most famous being the so-called ‘causal theory of reference’) versus models focused on sense and descriptive aspects; theories relying on the idea of necessary and sufficient conditions governing the ascriptions of meaning and reference, versus theories relying on the weaker, Wittgensteinian idea of family resemblances. . .

The question concerning the most suitable linguistic framework is still a much debated issue today. However, dominant trends emerged in the course of the 20th century that influenced the discussion of the incommensurability problem.

First, it has been largely recognized that the paradigm of necessary and sufficient conditions did not govern actual scientific linguistic usage. Such a recognition led philosophers to cope with indeterminacies linked to the working of language — the most famous one being the Quinian indeterminacy of translation — and to analyse the way these indeterminacies affect judgments of incommensurability²⁶. Correlatively, it gave

²⁶See [Field 1973]; [Norris 1997]; [Quine 1960], [Quine 1969] and [Quine 1987];

rise to developmental perspectives focused on linguistic learning processes, often in close connexion to empirical studies coming from cultural anthropology or, more recently, cognitive sciences.

Second, it has been widely admitted that holistic aspects of scientific languages had to be taken seriously and to be integrated in the characterization of incommensurability. Consequently the discussion has been directed to the strength of such holism and on the associated problem of extracting and delimitating significant linguistic units (units that can be legitimately considered to have a sufficient autonomy relatively to the larger linguistic system to which they pertain). This gave rise to the problem of local/global incommensurability²⁷.

It has been realised, thirdly, that reference-determination of scientific terms was never absolutely independent of meaning- or sense-determinations, and this led to the elaboration of mixed theories of reference combining descriptive and non-descriptive (often conceived as causal) factors. Many people wishing to show that incommensurability is not a threat for realism since it can be conciliated with trans-paradigmatic continuity of the reference of scientific terms, have built on such mixed accounts²⁸.

Fourth, as it is well known, the impossibility of drawing an absolute-invariant-natural frontier between the observational and the theoretical levels has appeared ineluctable. We are thus left with the problem of reconciling the need of the distinction with the inescapability of its relativization. This problem is of course intimately associated with holism and with the dependence of reference on sense: the observational/theoretical demarcation is a moving one since intuitively observational terms are not independent of other terms, including intuitively theoretical terms.

These intertwined recognitions and their associated difficulties lead to what has often appeared as an important paradox. Incommensurable theories have to be *competing* theories, otherwise the judgment of incommensurability is epistemologically trivial and deprived of any interesting consequence (think, for example, of the claim that thermodynamics and the theory of the unconscious are incommensurable). Yet by definition, competing theories target a common realm of phenomena, which means that they must admit shared observations (observations for which they

[Sankey 1991a]; [Soler 2004a]; [Weed 1997].

²⁷Kuhn introduced local holism and incommensurability in [Kuhn 1983a]; see [Hoyningen 1989, 112ff]. See also [Carrier 2001], [Chen 1990], [Soler 2000b] and [Soler 2003].

²⁸For more details and references, see note 35.

both claim responsibility). Thus we must be able to extract a common observational layer assumed by both of them. This amounts to saying that we must retain something of the observational/theoretical dichotomy and make its sense clear. And this amounts at the same time to the assumption of local holism and to tackling the difficulty of stating how to recognize sufficiently independent clusters of terms and statement in concrete cases, how to separate incommensurable concepts from commensurable ones. But if all of this is admitted, it seems now that the so-called incommensurable theories are not so incommensurable after all, since they have a phenomenal common measure²⁹. Aspects of this paradox are examined in some works of the present volume.

People working within the linguistic approach may have very different philosophical aims and outlooks. The latter are most of the time already apparent in the linguistic options that each author favours. As an illustration, we can compare Bird's and Carrier's work on incommensurability. Bird ultimately aims at defending scientific realism and at showing that incommensurability is harmless for scientific realism. In order to do so he favours, like many realist-oriented thinkers, to rely on an accommodated causal theory of reference. Carrier, for his part, assumes that there indeed are drastic descriptive revolutions involving drastic changes in reference, so that common versions of scientific realism are indeed threatened; and he assumes correlatively that causal theories of reference are of little use to save realism (which does not mean, Carrier insists, that we are committed to strong relativism, since rival incommensurable theories, although untranslatable, *can* be compared with respect to their empirical adequacy). This being admitted, Carrier favours a Wittgenstein-inspired (contextual and pragmatic) theory of meaning, and intends to provide in such framework a refined characterization of Kuhn's taxonomic incommensurability.

3. Approaching incommensurability with a **pragmatic perspective** constitutes another option, more and more praised, along the 20th century, in studies of incommensurability as well in other areas of studies reflecting on sciences. Although part of Kuhn's originality in [Kuhn 1962] was to emphasize pragmatic and implicit factors inherent to scientific activity, Kuhn's later language-based characterization of incommensurability took little notice of this dimension, to the regret of some of his readers. In this volume, the pragmatic approach to incommensurability is represented by the papers of Michel Bitbol and Emiliano Trizio.

Pragmatic oriented studies interested in the incommensurability prob-

²⁹Around these lines of thought, see [Carrier 2001], [Feyerabend 1972]; [Shapere 1964]; [Soler 2003]; [Soler 2004a].

lem have most of the time grown in opposition to language- and high-level-theory-centred accounts. Science, they urged, cannot be reduced to language and is an activity involving many other essential dimensions. No more can science be reduced to the production of high-level theories aiming at describing the world: science is *as much* — and in some accounts *primarily* — an empirical and most of the time experimental concrete activity involving concrete material objects and concrete actions-manipulations (for example the ones constituting laboratory life). Thus, a complete characterization of incommensurability cannot itself be reduced to linguistic characterizations and theory comparison. It must include non-linguistic, non-theoretical concrete aspects of scientific practices. And it must take into account the non-explicit and in principle not totally verbalizable elements that irreducibly constitute scientific activities. This is maintained, not only with respect to the level of theorization (for example more or less local know-how concerning abstract problems resolution, partially tacit goals and values constraining theoretical options, etc.), but also with respect to the level of laboratory life (for example more or less local know-how concerning the manipulation of instruments and the interpretation of machinic termini, partially tacit goals and values constraining experimental options, etc.).

On the basis of these shared requirements, the problem is how to elaborate an adequate framework allowing us to describe scientific practices in all their richness and variety. It is at this level that some disagreements arise. Some concern the elaboration and the choice of the relevant variables³⁰. Others concern the allowed and best ways to access to the tacit aspects of science and render them graspable. Still others concern the relations that can hold in principle, or that actually hold in concrete historical cases, between high-level theoretical practices and experimental laboratory practices. What is the most suitable model? A model in which one of the two poles is understood (possibly alternately) as the driving force conditioning the evolution of the other, if not as the cause determining such evolution? Or an interactionist model in which each actual scientific stage of development is understood as the emergent symbiotic result of a complex holistic equilibrium in which all elements mutually support each other? Do the two poles have a certain relative autonomy, and if it is the case, to what extent? And so on³¹.

It is in such a framework that the incommensurability labelled “literal” by Hacking and “machinic” by Pickering takes place. As for the present volume, Michel Bitbol examines the idea of an incommensura-

³⁰See for example [Hacking 1992, sections 6-9]

³¹For references, see note 19.

bility of laboratory life, on the one hand with reference to the Kuhnian and traditional linguistic-based characterizations of incommensurability, and on the other hand in the light of Pickering's and Hacking's account of science and literal incommensurability. Emiliano Trizio, for his part, explores the relations between literal incommensurability and taxonomic incommensurability, arguing that the alleged gap between them can be filled.

4. It would be an exaggeration to talk about a **cognitive approach** to incommensurability, since incommensurability is not a special focus of the proliferating field of cognitive studies. There are, nevertheless, analyses of the incommensurability problem that intend to rely on the results of some cognitive studies, or that want to confront such results with the hypotheses involved in a particular characterization of incommensurability³². The paper of Andersen included in the present book is linked with this trend.

As it is well known, the so called cognitive sciences intend to achieve a characterization of cognition grounded in empirical results and informed by studies produced by a cluster of different disciplines. As language is an aspect of cognition, connections have been established between cognitive sciences and the linguistic approach to incommensurability. This holds especially for the taxonomic characterization of incommensurability. The taxonomic characterization historically developed in close association with considerations about language acquisition and conceptual learning processes (Kuhn himself has been committed to this trend and strongly contributed to reinforcing it, if not to inaugurating it). Cognitive sciences have something to say about such processes. This part of cognitive studies is certainly the most exploited one in cognitively-informed philosophical reflections on the incommensurability problem — usually for the purpose of finding empirical support in favour of philosophical theses. This is the line followed by Hanne Andersen in the present volume, cognitive sciences being convened in order to sustain a conception of incommensurability based on dynamical frames.

5. What can be called **the Wittgensteinian approach** to science and incommensurability³³ has a certain autonomy with respect to the linguistic and pragmatic approaches even if it is intertwined with them in several respects. The paper of Aristides Baltas contained in the

³²[Andersen, Barker & Chen 1996]; [Barker 2001]; [Barker, Chen & Andersen 2003]; [Bird 2002b]; [Bird 2004]; [Chen, Andersen & Barker 1998]; [Nercessian & Andersen 1997].

³³See for example the works of A. Baltas, M. Bitbol, M. Carrier, V. Kindi and R. Read mentioned in the bibliography. See also [Andersen 2000].

present volume represents this tendency. The one of Martin Carrier is also linked to it, although less directly, since it is developed on the basis of a Wittgensteinian-inspired theory of language.

Wittgenstein-inspired accounts of incommensurability have become more and more praised in recent decades and constitute a living active tendency today. They are often part of a broader project aiming at reading Kuhn from a Wittgensteinian perspective. Kuhn, as he explicitly recognized, has been influenced by the philosophy of Wittgenstein, especially by Wittgenstein's conception of language. Many followers of Wittgenstein appreciate Kuhn's work because of its intimate proximity to their orientation. One has even been up to call Kuhn "a Wittgensteinian of the sciences"³⁴.

Approaching incommensurability in a Wittgensteinian framework consists most of the time in the adoption of some Wittgensteinian key-concepts and fundamental insights for the purpose of clarifying what is really at stake. Language-games, forms of life, grammar, 'meaning is use', family resemblances, etc., are put at the service of the task of clarifying incommensurability and renewing its characterization. Such attempts integrate most of the time some pragmatic aspects of incommensurability, without ignoring its linguistic dimension (the later being — depending of the authors — more or less important in the whole picture), so that many connexions and reciprocal teachings can be found between the Wittgensteinian approach on the one hand, and the neo-pragmatic and linguistic ones on the other. In his article, Aristides Baltas intends to understand incommensurability as a change of grammatical space. In order to do so he takes into account linguistic changes involving non-explicit aspects of language, without reducing incommensurability to a purely linguistic phenomenon.

Survey of the Contributions

1. **Alexander Bird** gives a contribution to a debate that began in the Seventies and is still alive nowadays through ever more refined arguments.

At the most general level, the question is to know up to what point incommensurability threatens trans-paradigmatic stability of the reference of scientific terms. The central epistemological issue at stake is scientific realism. Indeed, it is difficult to support realism without a minimum of historical continuity. Yet, the thesis that the history of science

³⁴[Read 2003].

shows the existence of scientific revolutions with corresponding pre- and post-revolutionary incommensurable theories, is precisely the negation of cumulative scientific development. The strategy of many realists have been to try to save cumulativity by preserving a certain continuity at the level of reference. They have intended to show that, although there indeed are scientific revolutions at the level of concepts and meanings, the reference of most scientific terms is nevertheless preserved.

To achieve that goal, most realists tried, at the beginning at least, to rely on the so-called ‘causal theory of reference’ (CTR). As it is well known, the CTR has first been elaborated by Kripke for proper names [Kripke, 1980], and has later been tentatively applied to kind terms. Roughly speaking, the idea is that human dubbing is rigidly linked to — since it is caused by — bits of reality through inaugural acts of baptism, so that the name continues to refer through time, despite any possible later changes of human beliefs, to the original reference — the real substance, thing, property. . . — actually involved in the original baptism.

Many authors tried to use the idea, or variants of it, against undesirable epistemological consequences of incommensurability. Hilary Putnam’s attempt is certainly the most famous one [Putnam 1973], [Putnam 1975a], [Putnam 1975b]. It initiated a quite famous dialogue with Kuhn ([Kuhn 1989], [Kuhn 1990]) and developed well-known exemplars such as the one of twin-earth and twin-H₂O. But since then, there have been many other less known interesting attempts to apply the CTR to theory change as well as to modify the CTR. Attempts to modify it have often developed in response to the criticism that a pure CTR deprived of any descriptive elements is untenable (or is useless because it is impossible to apply it to concrete historical cases). Refined versions of CTR, especially mixed versions including a minimum of descriptive elements side by side with the causal non-descriptive ones, have then been elaborated, providing the basis of a rich and sometimes technical literature on the relations between incommensurability, realism and the CTR³⁵.

In his book published in 2000, *T. Kuhn*, Alexander Bird defends the thesis that, in most historical scientific transitions commonly considered as revolutionary, reference continuity is indeed preserved. Discussing the situation of incommensurable theories in the framework of the CTR, he

³⁵See for example [Bird 2000, chapter 5]; [Devitt & Sterelny 1987]; [Kitcher 1978]; [Kitcher 1983]; [Kordig 1971]; [Kroon 1985]; [Kroon 1987]; [Kroon & Nola 2001]; [Nola 1980]; [Norris 1997]; [Papineau 1979]; [Papineau 1996]; [Putnam 1973]; [Putnam 1975a]; [Putnam 1975b]; [Sankey 1991d]; [Sankey 1994a]; [Sankey 1997b]. For critical reactions to realist attempts based on CTR, see for example [Feyerabend 1987a], [Kuhn 1989], [Kuhn 1990] and [Read & Sharrok 2002b].

admits Kuhn's last definition of incommensurability as untranslatability, but argues that untranslatability names transformations circumscribed to the level of sense, so that the reference remains untouched. In so doing he studies Kuhn's reply to Putnam, reconsidering Kuhn's arguments about cases such as 'gold' and 'water'.

In response to Bird's book, Rupert Read and Wes Sharrock promptly criticized Bird's arguments in a paper published in 2002 [Read & Sharrock 2002b]. The paper of Bird published in the present volume is a reply to these criticisms. Bird understands these criticism as claiming the *incompatibility*, not only in fact but also in principle, of the referentialist versions of scientific realism on the one hand, and incommensurability as conceived by the last Kuhn on the other hand. This is the central general claim he wants to refute, but he also turns to several more specific ones.

Having sketched the general content and central issues of the debate, Bird began to distinguish two "entirely distinct" but often conflated positions: on the one hand referentialism, defined as "the idea that reference is a key concept in assessing the progress of science and that there has been a large measure of referential continuity over time, even over revolutions"; and on the other hand essentialism, defined as "the view that natural kinds and perhaps also natural properties have essential characteristics". He regards as inaccurate Read and Sharrock's characterization of referentialism as "a kind of essentialism" — although he recognizes intimate relations between essentialism and putnamian referentialism.

Bird examines two versions of referentialism. The first one, called the "Fregean version" (although it is certainly not literally fregean), holds that sense determines reference. Bird argues that it is not a good response to incommensurabilism, firstly because too much of the theory may contribute to the reference determination, so that theory change may well imply reference change, and secondly because it moreover presents internal troubles, arising from the impossibility to draw a sharp dividing line between propositions contributing to sense and propositions contributing to reference. The second version of referentialism, called the "Kripke-Putnam version" and described as a "causal (...) externalist conception of reference determination", has Bird's favor, at least as far as kind terms are concerned. True, "the causal theory of reference does not in itself guarantee continuity of reference". But according to Bird, it "shows how there *can* be continuity of reference despite even revolutionary scientific changes, and shows how scientific realism is not threatened by the thesis of incommensurability as Kuhn formulated it".

In such a framework, Bird scrutinizes one by one the objections of Kuhn, Read and Sharrock against the Kripke-Putnam version of the

CTR, relying mainly on the example of the H_2O case. The first objection, raised by Kuhn, concerns the impossibility – in the sense of an incompatibility with modern chemistry — of an XYZ substance having all the superficially observable properties of our H_2O . Against it, Bird first insists that Putnam’s scenario has only to be imaginable, and not realizable in our world.

The next objection examined reflects on a variation of Putnam’s initial scenario involving Twin-Earth. Such an alternative scenario had been introduced by Bird in his book on Kuhn. Bird suggests we reconsider Putnam’s argument, situating it in the early nineteenth century, that is, at a time where scientists did already know that water is H_2O but were not sure that there is no distinct compound sharing the superficial properties of water. In such a situation, Bird claims, “it is not open to a putative forerunner of Kuhn’s to object that there could be no such XYZ that has the superficial properties of water but is a different compound”. Read and Sharrock protest that in such situation, “there no longer is a thought-experiment”, since the impossibility mentioned by Kuhn is an impossibility that refers to a given scientific state and taxonomic structure characterized by a strong solidarity between the assumption that water is H_2O and the assumption that XYZ does not exist. Bird dissects this interpretation of this answer in three assertions that he successively intends to refute, accusing Read and Sharrock at the same time of misunderstanding Putnam’s argument, assuming what Putnam’s argument wants to disprove, and standing in opposition to the commitments of chemists.

From this and other considerations, Bird comes to the strong conclusion that water “functions like a name and so is a rigid designator” picking out natural kinds, and that in consequence, “‘water is H_2O ’ is a necessary truth” — now making the connection between (putnamian) referentialism and essentialism. He reflects then on the delicate clause ‘to be a sample of a particular kind’ or ‘to be a sample of the same kind’, admitting at the same time that “members of the same kind must bear some deeper similarity than mere superficial resemblances” and that “‘sameness in kind’ may be fixed contextually” by chemist’s interests. This leads him to consider another objection of Read and Sharrock, addressing the fact that much of what is ordinary called water is not exclusively H_2O (due to impurities). For this reason, referentialists like Bird commonly appeal to modalities in their quasi-essentialist statements, saying, for example, that “‘in all possible worlds water consists (*largely*) of H_2O ’ (my emphasizes). But then there arises the question, posed by Read and Sharrock and recurrently addressed to the CTR, of whether “essentialism

would look so attractive if the impurities included XYZ, in significant proportions (say 25%)”.

Bird answers, first in appealing to the contextuality of the sameness in kind relation. According to the context, we have to distinguish different extensions of the term ‘water’. Now, the extension of ‘water’ as used in the context of chemistry is not the same than the extension of our everyday term ‘water’. The first one covers exclusively H_2O , whereas the second one includes impure samples. Bird admits in consequence that the adjective ‘largely’ should be dropped in the essentialist assertion above, as long as we are dealing with the chemical term ‘water’. He focuses then, second, on the empirically not forbidden case in which all our samples of water would be discovered by chemists to contain a large proportion of XYZ. In such case, Bird concludes, “we would have to admit that pure water is not a single chemical kind but is a mixture of two compounds. And it would certainly be plausible to argue that water has no real essence (although some might reasonably argue that it is essentially a mixture of H_2O and XYZ)”. But such a possibility, he continues, “does not show that in the actual case, where our samples are not such a mixture, water is not essentially H_2O ”.

Hence, Bird is finally committed to what I would call a ‘revisable essentialism’, according to which essence ascriptions are in principle always destabilizable by in principle always possible future scientific revolutionary discoveries of the kind just mentioned above. The necessity of “all water consists in H_2O ” is therefore a conditional necessity, hanging on the assumption that our science will not meet such destabilizations, the latter assumption being in turn fed by the realist credo that our science has indeed grasped enough of the actual elements composing the world.

Bird also examines Kuhn’s developments about the evolutions of our beliefs concerning the three states of water. Kuhn emphasizes the taxonomic transformation having accompanied the transition from the belief that liquidity is an essential property of water, to the belief that something else is and that ice and steam as genuine water. Bird argues, first that Kuhn’s point is historically inaccurate (many people including Aristotle equated ice and steam with water). Second, that even if it were true that some chemists considered steam and ice as different species, it is not enough to prove that the extension of the term ‘water’ does not cover steam and ice. And third, that even if ‘water’ analytically entailed ‘liquid’ in 1750, it would not be damaging for the essentialist, who could continue to hold that water₁₇₅₀ essentially contains H_2O .

Finally, Bird makes some brief remarks on the relation between superficial and ‘deep’ properties with respect to the question their necessity.

This is a response to a development of Kuhn, in which Kuhn points out the fundamental solidarity between the elements of the lexical scientific structure, from which he emphasizes that “the so called superficial properties are no less necessary than their apparent essential successors”. Bird remains very allusive here, but he accuses Kuhn of irrelevancy and call for more refined analytical distinctions and studies. He insists on the need to scrutinize the multiplicity of the plausible configurations (necessary superficial properties and necessary laws of nature; contingent laws of nature and contingent superficial properties; contingent laws of nature and necessary superficial properties. . .). And he points that necessary properties are not in every case essential properties.

On the whole Bird concludes that Kuhn’s arguments, despite of the efforts of Read and Sharrock to resurrect and develop them, in themselves “do not give the scientific realist a reason to doubt her view”. According to him, the referentialist defense of scientific realism is not threatened by incommensurability equated with untranslatability.

2. Highly different are the approach and the conclusions of **Martin Carrier** — and this illustrates that very divergent positions continue to constitute the debate on incommensurability nowadays. Let us quote Martin Carrier’s last sentence: “The lesson incommensurability teaches is that in the course of theory change, scientific achievements may be conceptually reframed beyond recognition. In particular, the occurrence of reference shifts poses a serious threat to the claim that scientific theories accomplish an ever deeper understanding of the same objects and processes. In this respect the incommensurability thesis retains some epistemic significance after all”. Correlatively, Carrier’s confidence in the usefulness of the CTR in relation to these questions appears very limited³⁶.

In his paper, Martin Carrier reconsiders semantic incommensurability, equated with untranslatability, in the framework of the “theoretical context account” or “context theory” of meaning, as Kuhn and Feyerabend did themselves. He intends to show in the same movement, first that semantic incommensurability is indeed instantiated in history of science, and second, that a coherent reconstruction of the notion can be proposed on the basis of an holistic-contextual theory of meaning.

Such a reconstruction of semantic incommensurability is here developed and illustrated relying on an historical case largely considered

³⁶In the present volume M. Carrier remains silent about the CTR. But in other papers he appears at least reluctant to it. See [Carrier 2001, 81] and [Carrier 2002, 141, footnote 2].

as revolutionary among historians and philosophers of science, namely, Lorentzian electrodynamics versus Einsteinian special relativity. Through this example Martin Carrier intends to show why it turns to be impossible to translate one into another central concepts of both theories (in the example the concept of velocity and the concept of mass).

The skeleton of the argument is the following. According to the theoretical context account, meaning has two main determinants: the inferential integration (i.e. the relations of a given concept to other concepts); and the conditions of application (associated with the set of situations to which a given concept is thought to apply and not to apply). A good translation has to preserve meaning. It has thus to satisfy two demands: preservation of the inferential relations, and retention of the conditions of application. Incommensurable concepts are untranslatable, because in their case, *only one* of these two demands can be fulfilled, but never both of them.

The ultimate reason of such an impossibility lies, according to Martin Carrier, in the fact that the two rival theories assume contrasting and incompatible sets of laws, that is, deep divergences at the level of the corresponding inferential relations. *As a consequence* of this nomological change, natural kinds are restructured: very different and often incompatible classes of equivalence hold before and after a revolution (what was thought to be different may be thought of the same type; radically new ‘natural’ kinds may appear; old central ‘natural’ kinds may completely disappear...). With respect to this point, Martin Carrier claims to reverse the order of priority assumed by Kuhn between nomological change and split up of scientific kinds. True, the last Kuhn put the restructuring of kind taxonomy at the center, and seemed thus to assume kind restructurings to be the primary factor and nomological change to be a derived secondary feature.

Martin Carrier’s last aim is to show that semantic incommensurability, understood in the way previously spelled out, is not at all the threat it has often appeared to be for rational theory comparison. According to him, “empirical comparison does not require translation and remains largely unaffected by incommensurability”. He illustrates the point on two examples: the series of experiments performed by Walter Kaufmann between 1901 and 1905, intended to measure the dependence of the mass of electrons upon their velocity; and the Kennedy-Thorndike experiment of 1932 aiming at comparing Lorentz’s and Einstein’s theories in the spirit of Michelson-Morley experiments.

In order to argue the point, Martin Carrier begins to emphasize that incommensurable theories, not only *de facto* have, considering proto-

typical historical cases, shared relevant phenomena, but moreover *must have* such shared phenomena. Otherwise, incommensurability would be epistemologically non significant. “A conflict between two theories only emerges if there is some shared realm which they jointly address”; “Advocates of each theories have to acknowledge responsibility for coping with these phenomena (which may be disparately understood in either theory). But this much of a common ground is secured by the mere fact that we are dealing with incommensurable theories”.

We encounter here the delicate question, already introduced above in the general presentation, of the ineluctable tension existing between incommensurability understood as a complete absence of common measure, and the requirement that incommensurable theories must be competing theories (if incommensurability is to name an interesting problem) and must therefore have some common phenomenal measure. And we see how Martin Carrier overcomes such difficulty: by assuming a sort of analytical link between incommensurability and the requirement to claim responsibility for at least some identical targeted phenomena. Each camp will describe and interpret these common experiments and results differently, within his own conceptual incommensurable framework, and each will evaluate the significance of the corresponding interpreted results for his own theory. With respect to these task, “no need for translation arises”. Thus incommensurability understood as untranslatability, although real, does not prevent empirical comparison. Moreover, such an empirical comparison can be performed by one and the same person, because of the possibility, elaborated by Kuhn, of bilingualism.

From all this Martin Carrier concludes that incommensurability, understood as untranslatability in the sense specified in the paper, is grounded in “incompatibility rather than unrelatedness”. In fact, incommensurability results from a tension between deep differences and required similarities. On the one hand, incommensurable concepts do not satisfy both the demands of sameness of conditions of application and preservation of theoretical integration. But on the other hand, “the converse aspects deserves emphasis as well: incommensurable concepts exhibit a particular type of relationship to each other; they are connected with one another by empirical or theoretical ties”. And the difference manifested through translation failure is not *any kind* of difference, so that the translation failure characteristic of incommensurability is itself not *any kind* of translation failure. “Not any old taxonomic disagreement is sufficient for producing incommensurability”. “It is a distinctive feature of incommensurability that the relevant classificatory discrepancies resist reciprocal adaptation because they arise from fundamental theo-

retical divergence. Non-translatability due to incommensurability is not the result of a simple conceptual gap. Incommensurability is not about an accidentally missing word; it is rather about an in-principle rift”.

As we shall see, such conclusions are very close to the one endorsed by Hanne Andersen in this book.

3. Martin Carrier’s paper is the object of three commentaries, which are the written (sometimes enlarged) versions of talks given in march 2002 in Nancy, at the occasion of a day organized by the Poincaré Archives in honor of Martin Carrier’s work on incommensurability.

The first commentary consists in two brief remarks by **André Coret**. One is about the relation between Martin Carrier’s conception of incommensurability on the one hand, and realism and relativism on the other hand; the other is about the identification of incommensurability with untranslatability.

The second commentary is a discussion, by **Soazig Le Bihan**, of Martin Carrier’s reconstruction of incommensurability. Soazig Le Bihan investigates the logical relations between three terms: inferential relations, conditions of application and the clause “the set of situations to which a concept is properly applied”. She questions the independence and the symmetrical role of the two former in the determination of meaning, and she then argues that the chief criterion of meaning and translation should be the third. Finally, she suggests that a more adequate and sound reconstruction of incommensurability could be achieved in shifting from semantic theory of *meaning* to semantic theory of *science*. In the later framework, semantic incommensurability would be defined as an (at least partial) incompatibility as regard to internal structure (non congruence of the models of the theories).

The third commentary is an exam, by **Léna Soler**, of the way Martin Carrier uses the two conditions ‘sameness of conditions of application’ and ‘preservation of inferential relations’ in order to reconstruct incommensurability. I argues that two different senses of ‘conditions of application’, and correlatively two different understandings of the clause ‘translation according to preservation of inferential relations’, are involved in Martin Carrier’s analysis of historical cases, and that once these senses are distinguished, the seductive symmetric conclusion of Martin Carrier does no more hold. Hoping to contribute to Martin Carrier’s general project by complementary means, I introduce more fined-grained discriminations and try to clarify some central difficulties. In Martin Carrier’s paper these difficulties take a particular form, but in my opinion, they manifest fundamental difficulties inherent to any holistic-contextual

framework — and thus difficulties that will appear inevitable to anyone thinking that holistic and contextual features are essential features of scientific practices. On the basis of these distinctions and analyses, I investigate the links between conditions of application and inferential relations and, finally, sketch an alternative characterization of semantic incommensurability in which the ‘inferential relations’ determinant appears as the central and primary determinant (from a methodological point of view at least).

4. **Hanne Andersen** starts from Kuhn last characterization of incommensurability in terms of non homology of taxonomic structures and violations of the no-overlap principle. She argues that such a characterization, although based on a plausible model sustained by some recent works on categorization in cognitive psychology, is nevertheless not a sufficient one to distinguish between revolutionary and non revolutionary transitions. She makes propositions to complete and refine Kuhn’s characterization, by insisting on the fact that taxonomic structures are dynamical entities, and by focusing on the reasons why they sometimes drastically change.

According to her, following a suggestion of Murphy and Medin, one must take into account, in order to distinguish incommensurable theories from cases of refinements and additions, not just the taxonomic structure and its given correlations of features in themselves, but, moreover, the justifications sustaining such correlations. These justifications are given by the theories underlying the conceptual framework. Indeed, the accepted explanations involved in theories, at the same time determine the bundling that organize previously encountered cases, determine which combinations are in principle possible and thus empirically expectable (even if they have not been actually empirically encountered at this point of the inquiry), and determine which combinations are in principle ruled out. Only such theoretically impossible combinations would, if empirically encountered, constitute a severe anomaly. The integration of such an anomaly would call the conceptual structure into question. It would lead to a deep restructuration of the previous taxonomy, corresponding to new bundlings of features, new divisions, new categories, new objects. In other words, we would have a case of incommensurability: the case of two unsuperimposable structures coordinated with two mutually exclusive theoretical systems and two mutually exclusive ontologies.

Although achieved by non identical trajectories and analytical means, Hanne Andersen’s last conclusion is perfectly congruent with Martin Carrier’s one when attempting to characterize the specific kind of contrast that is responsible for incommensurability. Hanne Andersen also

suggests a way to clarify the same difficulty, inherent to the incommensurability problem, that, as we have seen, Carrier examined too: the difficulty, described as “a serious challenge that has shown very difficult to handle”, “to explain how theories that are incommensurable theories can nevertheless compete”.

Non-competing theories, equated with theories pertaining to separated disciplines or specialities, could be called ‘incommensurable’ in a more strict and radical sense than competing theories (and Kuhn sometimes called the first ones ‘incommensurable’). But then it seems that they would be incommensurable precisely because they are not ‘about the same thing’ (or to put it metaphorically, following Kuhn’s and Andersen’s imagery: because they are theories occupying separated niches). By contrast, rival theories like Newtonian and Einsteinian physics are conceived as theories targeting the same object. For this reason it seems at first sight that they could be incommensurable in an epistemologically consequential sense. But can they really be incommensurable in a strong sense, since they must target something like the same domain?

Andersen’s proposition in order to distinguish these two cases and to tackle the associated difficulty is the following. We have a case of incommensurability in Kuhn’s original sense, that is, a case of incommensurability between *rival* theories, if and only if the two theories involved, although being differently structured, bundle (at least some) *shared features*. “While it is shared structure and not shared features that yields shared ontology, when structure is no longer shared, it is shared features that provides the overlap between different phenomenal worlds necessary for them to compete in offering the better account of the world in the form of more successful or more promising bundlings”.

5. **Aristides Baltas** is, at a point, led to the same difficulty, but the spirit of his project, and the picture emerging from it, sounds differently, although there is maybe no contradiction at the level of general thesis. The project is to reformulate and clarify incommensurability in a wittgensteinian framework.

Aristides Baltas puts at the center what he calls the “grammatical resources of the paradigm”. The grammatical resources refer to a set of ‘assumptions’ *with quotes*, where the quotes indicate that the assumptions in question are, within a given paradigm, not identified or expressed as such, or at least not subject to discussion, to justification, to test procedures or the like. Remaining tacit or not questioned, it is silently that they perform their work. This work is inextricably semantical and methodological. The considered ‘assumptions’, at the same time constitute “the latent part of meaning” and channel the investigation process.

They give its usual or natural interpretation to the conceptual system coordinated with the paradigm. They underlie the favored analogies and pictures. They determine what is possible and impossible, waited or unexpected, obvious or questionable, important or anecdotic, given or open to test... All this constitute a grammatical space, with its potentialities and its bounds.

In such a framework, radical scientific change is described as a "leap into the ungrammatical". This happens through the following pattern. In the course of the investigation of nature by the characteristic means of a given paradigm, deadlocks arise and resist. Then at a point, scientists (or at least some of them) become convinced that the only way to overpass the problems is to turn the deadlock into a definition. In other words, they now admit what was previously unacceptable and most of the time unthinkable, or at least unthought, precisely because it was out of the grammatical bounds of the paradigm. In this movement, some previously hidden 'assumptions' are disclosed. They become illuminated, they are suddenly viewed as assumptions without quotes, transformed into propositions subject to discussion and test. The work they surreptitiously performed before is now rendered explicit. What they previously induced and forbid is now clarified. On the whole, a new and broader grammatical space is opened. A new vantage point is thus available. From such a new vantage point, the assumptions constitutive of the old paradigm appear, *ex post facto*, as unawares unjustifiably presuppositions, and correlatively, the anomalies having led to the revolution appear as misconstructions due to the work of old illegitimate pre-judgments.

On the basis of such a reading of incommensurability, Aristides Baltas intends to show that the ideals of rationality and scientific progress can be saved from the accusations of relativism recurrently directed against Kuhn's work, and that some varieties of realism are compatible with kuhnian inspired positions.

In order to achieve that aim, he analyzes anomalies as resistances originating in a world independent of ideas and forcing scientists, be it only negatively, to recognize the inadequacy of their current paradigms. He examines the very nature of the communication breakdown between scientists residing in incommensurable paradigms, arguing for its inevitability, but pointing at the same time available means to cope with it, relying on the large amount of common assumptions.

Indeed there is, argues A. Baltas, a kind of continuity between succeeding paradigms. First, the two parties share an enormous grammatical space and large areas of common language: there are blind spots,

but these are circumscribed. Second, two competing paradigms are never totally unrelated. A scientist having experienced a revolutionary transition and living now in the new paradigm is always able to spell out significant relations between the old and the new paradigms — especially in the case of mathematical physics. A. Baltas sketches an analysis of the potential corresponding conceptual, empirical and mathematical relations. He talks about imperfect and fragmentary translations and (re)interpretations, or sometimes about “interpretation/translation” in order to indicate that his use of the word “translation” is not the specific one retained by Kuhn in his late characterization of incommensurability as non-translatability [Kuhn 1983a]. Among significant relations that can be spelled out between two succeeding paradigms, we find an important one targeting phenomena. Indeed — and third, a sense has to be given to the idea that the two parties share identical phenomena to be explained, even if it is not strictly correct to talk about the same phenomena *tout court*. We have to admit that “something of the materiality of the phenomena (...) is carried invariant across the leap”. Otherwise we are no more able to render justice to the shared assumption that the old and the new paradigms are *competing* paradigms — here we encounter one more time the difficult question, enounced in the general presentation above and also faced by Andersen’s paper, of giving sense to the idea that paradigms are at the same time rival and incommensurable. According to A. Baltas we have to admit — with qualifications — that the new paradigm “capture the same ‘something’ that resisted its capture by the old” paradigm. This is what “makes the new paradigm not merely different from the old, but the one that *succeeds* it”.

On the whole, the new paradigm conserves somehow, at different levels and notably at the level of phenomena, the memory of the old. The post-paradigm continues to enfold the impossibilities of the pre-paradigm to cope with determined anomalies, even if, by construction, it enfolds these impossibilities under the form of a “repressed memory”. “In repressing this memory, it resolves the anomaly and becomes the grammatical space of a new paradigm; but still enfolding it, it remains linked to the old and thence continuous with it in this sense”.

Such a continuity constitutes, joined with a claim of asymmetry that we are going to consider now, the support of A. Baltas’ claim according to which incommensurability is perfectly compatible with scientific progress. The pre- and post- paradigms, he insists, are not at all in an equivalent position. On the contrary, there is an *inherent asymmetry* between them. To analyze the nature of such asymmetry shows that the post- is *objectively superior* to the pre-.

The succeeding paradigm is, first, objectively wider than the preceding one. Indeed, more is thinkable and visible in it – and this holds independently of any claim concerning its empirical adequacy. From the new vantage point P2, The post-revolutionary scientists are aware of assumptions that remained unseen or unquestioned by the pre-revolutionary scientists. They can show how these assumptions assured the coherence of the old conceptual system. They can render explicit what these assumptions suggested and what they forbid. They can give reasons concerning why it was questionable to endorse them. . . Whereas the pre-revolutionary scientists cannot, remaining blind to these assumption and a fortiori to their effects. Hence, the sort of ‘interpretation-translation’ involved here is a “one way” interpretation-translation, only performable from the new to the old. The new grammatical space, the new horizon of the inquiry, is thus objectively wider than the old one.

As a particular case, it is wider as well at the level of evaluations of empirical adequacy/non-adequacy. Post-revolutionary scientists have at their disposal explanations spelling out why old scientists encountered certain empirical anomalies and were not able to overcome them. They can give reasons for the success and failure of the way P1 accounted for its empirical phenomena, and they are able to account successfully for at least some phenomena (as reinterpreted) that were at the heart of the deadlocks having originated the revolutionary change. Whereas adherents of the old paradigms do not have symmetric means at hands: they remained blocked. The former has the advantage on the latter, also at the level of “rhetorical ammunition”. The former possess objectively powerful means than the later in order to convince of the superiority of their viewpoint. They have thus more chances to succeed than their adversaries.

Moreover, the involved asymmetry induces a strong irreversibility. Once scientists have seen, they cannot do as if they had not seen. There is no possibility to go back. In that sense, once a wider grammatical space have been opened, the old paradigm is definitively superseded; there is no real choice between the old and the new.

The pattern just described is what constitutes scientific progress as understood by A. Baltas. Scientific progress so understood takes places intricately at both conceptual and empirical levels, and it is a genuine progress, although not a cumulative one.

So far, we dealt with incommensurability in the traditional sense. Now, the two last paper of the book enlarge the perspective in considering the case of the machinic-literal incommensurability.

6. **Michel Bitbol** considers the incommensurability problem in a neo-pragmatic and structural perspective. This perspective is fundamentally sympathetic to Wittgensteinian insights, although these are not explicitly mentioned in the article. Michel Bitbol urges the incompleteness of the linguistic-based account of incommensurability, and argues for the fruitfulness of an explanation that equates the technico-experimental practices, and more generally the performative aspects, with the central axis of physical knowledge. He is then led to discuss machinic-literal incommensurability.

He begins in distinguishing three strata within a physical paradigm: the ontological, the performative and the structural ones. The ontological stratum is associated with models, representations, physical scenarios or stories (it is, in other words, the narrative side of high-level theories). The performative stratum names the instrumental and behavioral culture characteristic of a laboratory tradition (including the technico-experimental knowing-how, the specific goals of concrete realizations and so on). The structural stratum designates the mathematical formalism and the associated legal-predictive potentialities of high-level theories (such a formal skeleton being possibly the common core of a range of different and incompatible ontological interpretations and models).

From there Michel Bitbol stresses two points. 1/ All is not linguistic, verbalised and explicit in the paradigm, especially at the performative level: here in particular, the philosopher must take into account commitments and projects, backgrounds norms of action, acquired habitus, entertainment to act in certain ways, knowing how, tacit background knowledge, etc. 2/ Although the three strata are in fact related, each of them have a certain non negligible autonomy.

These two points constitute the basis on which Michel Bitbol develops his conception of a non linguistic kind of incommensurability: an incommensurability of laboratory practices inspired by the work of Pickering and Hacking, also labeled “performative incommensurability” since it primarily concerns the performative stratum³⁷.

³⁷“Performative incommensurability” is an expression first introduced by A. Pickering. But in his paper, M. Bitbol uses the expression in a quite different sense than Pickering’s original one. In Pickering text, “performative incommensurability” names the possibility of an extreme form of incommensurability, in fact just sketched but not really explored. This possible incommensurability would involve more exotic practices than the scientific ones. It have been for this reason let aside in the general presentation above, but here is an occasion to mention it.

Pickering talks about “the possibility of a radical *performative incommensurability* — an incommensurability of *powers*” [Pickering 1995, 245], an “incommensurability in human (and non human) powers” [Pickering 1995, 192], residing in the contrast

Such an incommensurability of laboratory practices may happen in the absence of any incommensurability at the structural-ontological levels: it may happen between scientific practices sharing a lot structurally and ontologically — the possibility of this disconnecting being opened by the relative autonomy of the three layers. Correlatively, such an incommensurability cannot be grasped through an exclusively-linguistic characterization — due to the non verbal and implicit constitutive aspects of laboratory practices.

Incommensurability of laboratory practices names the relation between two highly different forms of laboratory life: between laboratory cultures that are organized by very different background presuppositions and that involve mutually exclusive gestures, experimental setups, experimental configurations and results. M. Bitbol gives two examples. The example of two laboratory traditions in high-energy physics, the first one being governed by a culture based on images (produced via bubble chambers etc.), and the second one by a culture of counting (electronic detectors like scintillation counter, spark chambers etc.). And the example of experimental chemical practices before and after Lavoisier, the second one conferring a crucial advocating role to scales, whereas the first one takes such procedures as non crucial, non significant or sometimes even irrelevant.

Reading Michel Bitbol's developments, it appears that such laboratory traditions are autonomous in several senses. First they are autonomous with respect to one another as well as to higher-level theoretical strata, in the sense of possessing proper internal characteristics and having relatively independent internal conditions of coherence and closure. But they are moreover autonomous in a stronger sense with respect to high-level theories: they are credited of having the power to be (sometimes) the driving force of important changes at the higher

between standard and non standard performances. "None of the studies I have discussed involve much more of human beings than observing what is going on and manipulating medium-size objects in humdrum ways". But in some cultures we find "non-standard material agency, (...), stories, for example, of mines inhabited by dwarves, demons (...). On the other side, we also find human beings with quite non-standard powers — magi, alchemists, witches, and so on" [Pickering 1995, 243]. "Within different cultures human beings and the material world might exhibit capacities for action quite different from those we customarily attribute to them". "The question remains of whether we should think it", but "that our own powers as human beings might be bound up with culture in this way is a quite startling idea that I find it fascinating to dwell upon. It is, of course, absent from traditional discussion of incommensurability" [Pickering 1995, 245].

Bitbol "performative incommensurability" is closer to what Pickering names "machinic incommensurability".

structural and ontological levels (and as a particular case, of having the power to impulse the development of incommensurable theories). This may happen when they presuppose, most of the time tacitly, assumptions and characteristics which prove to be in tension or disharmony with some central assumptions of the theoretical levels. In such a situation they contain, in a way potentially, the power to destabilize higher strata, or in other words the seeds of theoretical revolutions. And that sometimes indeed happen. Thus high-level explicit theories and high-level (in general less explicit) values governing theoretical elaboration, are not, as some philosophers of science have often assumed, the only or the primary actual driving vector of scientific changes.

Michel Bitbol's paper develops such ideas in association with other claims that have consequences for the incommensurability problem. According to him and to many neo-pragmatic thinkers, traditional dual schemes putting face-to-face a knowing subject and a preexisting object, or a proposing-scientist and a nature-thing, or theories and phenomena or the like, have to be replaced by a co-production scheme according to which the two poles of each duality are nothing more and nothing less than the meta-stable products that have co-emerged from an internal-to-practices dialectical process.

Following [Hacking 1992], M. Bitbol emphasizes moreover that in such a neo-pragmatic perspective, the classical Duhem-Quine under-determination of scientific theories has to be enlarged in principle, so as to include the new elements of experimental practices (notably the material and performative ones) that have been recognized susceptible to enter into the possible infinite combinations constituting empirical co-adjustments. He militates in favor of the notion of a *generalized coherence* that would apply not only to ideas or propositions, but also to concrete actions. But he recalls at the same time with Hacking that we need to reflect, correlatively, on the reasons why the feeling of under-determination is so reduced in practice. Finally he examines the temptation to maintain certain form of realism in such a framework, grounding the reflection on the new epistemological situation offered by quantum mechanics, and extending it to the case of mathematical practices.

7. In his paper **Emiliano Trizio** intends, for his part, to clarify the relations between two forms of incommensurability that are considered to be intrinsically different. The first one is the 'traditional' kuhnian incommensurability identified with taxonomic untranslatability. The second one is the machinic-literal incommensurability introduced by Hacking and Pickering, and presented by them as a *new* form

of incommensurability having *nothing to do* with meaning change and untranslatability (cf. above).

According to Emiliano Trizio, taxonomic and literal incommensurabilities, far from being two different in nature, well separated epistemological configurations, are, rather, two particular species of a generic super-ordinate characterization of incommensurability describable as “an enlarged taxonomic version of incommensurability”.

In order to argue for this thesis, Emiliano Trizio provides a deep analysis — notably inspired by Pierre Duhem’s and Nancy Cartwright’s discussions of the (often instrumented) relations between theory and observation — of what we may mean when we say that scientific terms “apply to nature”. He urges fine-grained discriminations between different kinds of applications to the world. The aim of such discriminations is, above all, to capture and highlight what he takes to be an essential difference: namely, the difference between applications that involve instrumented acts of measurements and applications that are exempt of such kind of acts. Kuhn is accused to remain blind to such a difference especially in his latest works, this blindness being manifest in Kuhn’s uniform treatment of all scientific examples, regardless of the degree of abstraction of the theories and of the degree of sophistication of experimental activities involved.

Let us reconstruct Emiliano Trizio’s own position. The philosopher of science reflecting on modern physics should pay more attention to the difference between perceiving and measuring. Indeed, unaided, normal perception and highly instrumented cognitive activities involved in experiments are two different kinds of acts of the knowing subject, two performances that have an “intrinsically different status”. There are, correspondingly, two kinds of terms or, if one focuses on structured clusters of terms, two kinds of taxonomies: terms or “taxonomies that can be applied perceptually”; and terms or “taxonomies that require acts of measurements”. Consequently the “ostensive/non ostensive character” of a theory is introduced, or more realistically — shifting from sharp contrapositions to a continuous scale of ‘ostensiveness’ — the degree of ‘ostensiveness’ of a theory, that is, its “degree of perceptual accessibility”. Moreover and correlatively the idea — central for the suggested enlarged conception of incommensurability — of dual/non-dual taxonomies is formulated

An example of non-dual taxonomy is a “perceptual” or “observational” taxonomy, the most obvious example being the “common-sense” taxonomies underlying ordinary observations (but “descriptive sciences” are also mentioned), which involve only “ready-for-use” terms: only terms

that are, in a given context, *immediately* applicable on the basis of an act of unaided, normal perception. By contrast, a dual taxonomy *moreover* contains terms that are split into different sets in turn linked to *disparate* specific *instrumented* conditions of application. Taxonomies of contemporary physics, or taxonomies of any physico-*mathematical* theory, are dual.

A dual taxonomy is called “dual” because it is composed of two parts (or two “semi-dual taxonomies”): the “experimental part” and the “objective part”. The experimental part is composed by names and descriptions targeting different sets of instrumented experiments: each of these sets being “the class of equivalence of all possible experiments leading to the same conclusion (conceptually unified by physical theory)” which “can in turn be divided in types of experiments”, subtypes etc. The “objective part” of the dual taxonomy is composed by descriptive terms purporting to refer to the world (is only composed by “objective terms” referring to reality).

The two parts of the dual taxonomy are “highly interrelated”. Roughly speaking, the types of laboratory practices constituting the experimental part define the appropriate conditions of application of the objective terms. The relation between the two components is more exactly described as a relation of “co-classification” (“the entities physical theory describes are classified if and only if the experimental contexts that reveal them are classified in turn”), or as a relation of “co-subsumption” on the one hand of what is equated with objective entities and processes, and, on the other hand, of actions in experimental contexts. In other words, the interrelation is of the following nature: experimental actions *aim at* measuring *physical objective quantities*; and reciprocally, physical theoretical quantities supposed to refer to physical objective unobservable realities *cannot be recognized as such* without performing experimental actions and assuming the relevance and reliability of these actions. Now, assuming the relevance and reliability of these actions amounts in turn to be committed to assumptions that order the available experimental activities in classes *taken as equivalent with respect to the determination of* the targeted physical quantities and that give a definite particular content to the methodological condition of the repeatability of the experiments.

An inverse proportionality holds between the ostensive strength of a theory and the dual character of its taxonomy. In the case of descriptive sciences, the experimental part of the dual taxonomy is most of the time embryonic if not null, whereas in contemporary mathematical physics, it is hypertrophic (the last point being illustrated in the paper through

the case of the crystalline lattice structure). The role of perceptual similarities is very reduced in mathematical physics: the dual taxonomy groups into equivalent classes — via the experimental part — *infinitely different perceptual features*.

Linking the latter reflections and distinctions to the theory-learning process, it appears that we have to take into account a “hierarchy of increasingly concrete and quantitatively defined exemplars” in Kuhn’s sense of the term, some of which include experimental instrumented actions in the laboratory. However, to learn theories by solving paradigmatic problems through expositions to (more or less ostensive and dual) exemplars, does not amount to ‘*genuinely apply scientific terms to nature*’. Kuhn conflates learning or understanding a theory (by solving problems with a pencil and a sheet of paper) with “applying it to the world” (and he mainly focused on the first activity, more and more exclusively characterized in terms of linguistic differences). But the authentic application of a theory to the world only takes place during a real laboratory session involving the recognition that what happens before our eyes *is* the situation described (in an idealized and purified way) in the textbooks. Now, very talented skilful problem-solvers may be completely lost in a well-equipped laboratory.

Thus on the whole, dealing with the application of scientific concepts to nature, we have to take into account, not only degrees of concreteness of the exemplars, not only degrees in the extension of the experimental part of dual-taxonomies but, moreover and above all, *degrees of acquaintance*. Authentic application requires the coordination of (more or less ostensive, more or less dual) scientific taxonomies and exemplars with *indeed experienced* concrete (instrumented in the dual-case) situations.

Relying on these distinctions Trizio claims that an “enlarged taxonomic formulation” of incommensurability can be defined. “*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*”. In such framework, Hacking’s and Pickering’s versions of incommensurability on the one hand, and Kuhnian incommensurability on the other (illustrated through the example of Ptolemaic/Copernican astronomies), can be equated to two particular extreme cases of such “enlarged taxonomic incommensurability” in the spectrum of “an ideal classification of theories with respect to the strength of their ostensive character and the inversely proportional duality of their taxonomies”.

Hacking's and Pickering's cases correspond to "theories that differ radically not only with respect to the objective part of their dual taxonomies, but also with respect to the experimental part". In other words, they involve "two rival schools of physicists accepting different dual taxonomies that define distinct classes of equivalence of experimental settings and procedures", so that the "two rival theories are supported by disjoint sets of measurement procedures".

Between such a configuration and Kuhn's example concerning Ptolemaic/Copernican astronomies, all intermediary cases may be conceived (although their plausibility remains to be examined): all combinations that may be generated from the idea that *only some* elements are not shared in the objective and experimental parts of the dual taxonomies.

8. **The bibliography** at the end of the volume contains the references mentioned by each contributor in his own paper, plus a selection of references about the incommensurability problem that takes into account the descriptive, axiologic and machinic dimensions. Given the impressive volume of the literature devoted to incommensurability and the space allowed, it has been impossible to provide the reader with a complete bibliography. I emphasized recent works and tried to include all the major classical references³⁸.

³⁸I am particularly grateful to Alexander Bird and Emiliano Trizio for helping me by their critical comments and their remarks concerning the English language; I am further indebted to Paul Hoyningen-Huene, Howard Sankey and Scott Walter who read and commented parts of this introduction.