RETHINKING SCIENTIFIC CHANGE AND THEORY COMPARISON

BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE

Editors

ROBERT S. COHEN, Boston University
JÜRGEN RENN, Max-Planck-Institute for the History of Science
KOSTAS GAVROGLU, University of Athens

Editorial Advisory Board

THOMAS F. GLICK, Boston University
ADOLF GRÜNBAUM, University of Pittsburgh
SYLVAN S. SCHWEBER, Boston University
MARX W. WARTOFSKY†, (Editor 1960–1997)

VOLUME 255

RETHINKING SCIENTIFIC CHANGE AND THEORY COMPARISON:
STABILITIES, RUPTURES, INCOMMENSURABILITIES?

Edited by

LÉNA SOLER
LPHS-Archives Henri Poincaré, Nancy, France

HOWARD SANKEY
School of Philosophy, University of Melbourne, Australia

and

PAUL HOYNINGEN-HUENE
Center for Philosophy and Ethics of Science, University of Hannover, Germany

Springer

Library of Congress Control Number. 2008920285

ISBN 978-1-4020-6274-2 (HB)
ISBN 978-1-4020-6279-7 (e-book)

Published by Springer,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

www.springer.com

Printed on acid-free paper

All Rights Reserved
© 2008 Springer
No part of this work may be reproduced, stored in a retrieval system, or transmitted
in any form or by any means, electronic, mechanical, photocopying, microfilming, recording
or otherwise, without written permission from the Publisher, with the exception
of any material supplied specifically for the purpose of being entered
and executed on a computer system, for exclusive use by the purchaser of the work.

CONTENTS

Contributors ix

Léna Soler
Introduction 1

Part 1: Incommensurability, As Differences in Quasi-Intuitive Cognitive Capacities: A Task for Psychology? 19

Alexander Bird
Incommensurability Naturalized 21

Paul Hoyningen-Huene
Commentary on Bird's Paper 41

Part 2: Incommensurability in a Wittgensteinian Perspective: How to Make Sense of Nonsense 47

Aristides Baltas
Nonsense and Paradigm Change 49

Eric Oberheim
Commentary on Baltas' Paper 71

Part 3: Intra-Theoretical Change, as a Subjective Creative Elucidation of an Objective Formerly Present Content 85

Anouk Barberousse
From One Version to the Other: Intra-theoretical Change 87

Igor LY
Commentary on Barberousse's Paper 103

Part 4: Investigating the Continuities of Scientific Theorizing: A Task for the Bayesian? 107

Stephan Hartmann
Modeling High-Temperature Superconductivity: Correspondence at Bay? 109

Edward Jurkowitz
Commentary on Hartmann's Paper 131

Part 5: From the Cumulativity of Physical Predictions to the Cumulativity of Physics 143

Bernard D'Espagnat
Is Science Cumulative? A Physicist Viewpoint 145

Marcel Weber
Commentary on D'Espagnat's Paper 153

Part 6: From Denotational Continuity to Entity Realism 157

Robert Nola
The Optimistic Meta-Induction and Ontological Continuity: The Case of the Electron 159

Steve Clarke
Commentary on Nola's Paper 203

Part 7: Is a Realist Interpretation of Quantum Physics Possible? 207

Hervé Zwirn
Can we Consider Quantum Mechanics to be a Description of Reality? 209

Soazig Le Bihan
Commentary on Zwirn' Paper 219

Part 8: Ontological Continuity: A Policy for Model Building or an Argument in Favour of Scientific Realism? 223

Rom Harré
Reasons for Choosing Among Readings of Equipollent Theories 225

Mauricio Suárez
COMMENTARY ON HARRÉ'S PAPER 239

Part 9: A Change of Perspective: Dissolving the Incommensurability Problem in the Framework of a Theoretical Pluralism Incorporating an Instrumental Rationality 245

Paul Teller
OF COURSE IDEALIZATIONS ARE INCOMMENSURABLE! 247

Ronald N. Giere
COMMENTARY ON TELLER'S PAPER 265

Part 10: What Can Philosophical Theories of Scientific Method Do? 271

Martin Carrier
THE AIM AND STRUCTURE OF METHODOLOGICAL THEORY 273

Michel Bitbol
COMMENTARY ON CARRIER'S PAPER 291

Part 11: A New Kind of Incommensurability at the Level of Experimental Practices? 297

Léna Soler
THE INCOMMENSURABILITY OF EXPERIMENTAL PRACTICES: AN INCOMMENSURABILITY *OF WHAT*? AN INCOMMENSURABILITY *OF A THIRD TYPE*? 299

Howard Sankey
COMMENTARY ON SOLER'S PAPER 341

Part 12: Pragmatic Breakdowns: A New Kind of Scientific Revolution? 349

Thomas Nickles
DISRUPTIVE SCIENTIFIC CHANGE 351

Emiliano Trizio
COMMENTARY ON NICKLES' PAPER 381

CONTRIBUTORS

Aristides Baltas
National Technical University, Athens, Greece

Anouk Barberousse
Institut d'Histoire et de Philosophie des Sciences et des Techniques, Paris, France

Soazig Le Bihan
LPHS – Archives Henri Poincaré, Nancy, France and Illinois Institute of Technology, Chicago, IL, USA

Alexander Bird
University of Bristol, United Kingdom

Michel Bitbol
CREA, CNRS/Ecole Polytechnique, Paris, France

Martin Carrier
Department of Philosophy, Bielefeld University, Germany

Steve Clarke
Program on the Ethics of the New Biosciences, James Martin 21st Century School, University of Oxford, United Kingdom, and Centre for Applied Philosophy and Public Ethics, Charles Sturt University, New South Wales, Australia

Bernard d'Espagnat
Institut de France, Paris, France

Ronald N. Giere
Center for Philosophy of Science, University of Minnesota, Minneapolis, MN, USA

Rom Harré
Linacre College, Oxford, United Kingdom and Georgetown University, Washington, DC, USA

Stephan Hartmann
Center for Logic and Philosophy of Science, Tilburg University, Tilburg, The Netherlands

Paul Hoyningen-Huene
Center for Philosophy and Ethics of Science, University of Hannover, Germany

Edward Jurkowitz
Department of Humanities, Illinois Institute of Technology, Chicago, IL, USA

Igor Ly
LPHS – Archives Poincaré, Université Nancy 2, CNRS, Nancy, France

Thomas Nickles
University of Nevada, Reno, USA

Robert Nola
Department of Philosophy, The University of Auckland, New Zealand

Eric Oberheim
Humboldt University, Berlin, Germany

Howard Sankey
School of Philosophy, University of Melbourne, Australia

Léna Soler
LPHS – Archives Henri Poincaré, Nancy, France

Mauricio Suárez
Department of Logic and Philosophy of Science, Complutense University, Madrid, Spain

Paul Teller
Department of Philosophy, University of California, Davis, CA

Emiliano Trizio
LPHS – Archives Henri Poincaré, Nancy, France

Marcel Weber
University of Basel, Basel, Switzerland

Hervé Zwirn
Institut d'Histoire de Philosophie des Sciences et des Techniques, and Ecole Normale Supérieure de Cachan, Paris, France

INTRODUCTION

LÉNA SOLER

This volume is a collection of essays devoted to the analysis of scientific change and stability. It represents the most recent thinking on the topic of incommensurability and scientific theory change. It explores the balance and tension that exists between commensurability and continuity (or stabilities) on the one hand, and incommensurability and discontinuity (or ruptures) on the other. And it discusses some central epistemological consequences regarding the nature of scientific progress, rationality and realism. With respect to these topics, it investigates a number of new avenues and revisits some familiar issues, with a focus on the history and philosophy of physics, in a way that is informed by developments in cognitive sciences as well as the claims of "New experimentalists".

The essays in this book are fully revised versions of papers which were originally presented at the international colloquium, "Repenser l'évaluation comparative des théories scientifiques: stabilités, ruptures, incommensurabilités?" organized by Léna Soler and Paul Hoyningen-Huene at the University of Nancy, France, in June 2004. Each paper is followed by a critical comment, which either represents an opposing viewpoint or suggests some developments. The conference was a striking example of the sort of genuine dialogue that can take place between philosophers of science, historians of science and scientists who come from different traditions and endorse opposing commitments. I hope that this is evident in the book too and that it will constitute one of its attractions. The book is also unique in reflecting and promoting interactions between French philosophy of science and Anglo-American philosophy of science.

As an introduction, I will describe the way the problem of scientific change has been framed and transformed in the philosophy of science throughout the twentieth century and up to the present, emphasising general tendencies in the way problems have shifted, and indicating how the different contributions of this book are related to each of these issues.

The twentieth century has been the theatre of important scientific transformations – so important that they have often been described as ruptures, revolutions or mutations. These transformations manifested themselves at different levels: at the level of high-level theories; of scientific instrumentation; of experimental practices; of the organisation of scientific research. Philosophers of science have sought to characterize these changes, to understand the reasons for them and to explore their implications.

L. Soler, H. Sankey and P. Hoyningen-Huene, (eds.), Rethinking Scientific Change and Theory Comparison: Stabilities, Ruptures, Incommensurabilities, 1–17.
© 2008 *Springer.*

1. SCIENTIFIC CHANGE AS RUPTURE BETWEEN THEORIES

What struck philosophers of science first, and what has been at the centre of the debates during the latter two thirds of the twentieth century, is *rupture at the level of theories*. Indisputably, a number of new scientific *theories* with unexpected characteristics emerged during this period, which broke ontologically and methodologically with earlier theories. This was most notably the case in physics with the theories of relativity and, especially, quantum physics. The result was a considerable enrichment of the range of theories and inter-theoretic relations available for examination. And the comparative evaluation of competing scientific theories, understood as one of the crucial tasks of working theoretical scientists, became an increasingly important problem for philosophers of science.

From an epistemological point of view, what was at stake, in that context, was nothing less than scientific realism, scientific progress, scientific rationality and relativism. A number of scholars, reflecting on these perceived-as-deep transformations of scientific contents and criteria of theory-choice, came to conclude that traditional scientific realism was untenable. They came to endorse weaker and weaker conceptions of scientific progress (so weak that their foes equate them with the claim "*no* scientific progress"), as well as to deny that there may be rational grounds for the judgement that one theory is objectively better than another.

Such alleged dramatic consequences of radical ruptures arising in the theoretical sphere have usually been discussed under the heading of "incommensurability". Two kinds of incommensurability have been progressively recognized as different in principle, although most of the time intertwined in real historical cases. They are commonly labelled "semantic incommensurability" and "methodological incommensurability".[1]

2. THE PROBLEM OF SEMANTIC INCOMMENSURABILITY

Semantic incommensurability involves radical changes at the level of theoretical contents that manifest an incompatibility not reducible to a simple logical contradiction. The incompatibility is apparently related to differences in the semantic resources themselves, so that one is inclined to say that there is no common measure at the very level of what is thinkable or expressible in symbols. Such incompatibility has been characterized by the late Thomas Kuhn, and is currently described in recent works, as the impossibility of translating some key words of one theory into the vocabulary of the other, or as a non-homology between the taxonomic structures of successive paradigms.

At the level of the incommensurability of theoretical contents, the task is to achieve a fine-grained characterization of the differences involved, as well as to discuss how

[1] For recent overviews on the incommensurability problem, see Soler (2000, chap. VII, 2004), Hoyningen-Huene and Sankey (2001).

deep the differences encountered in the actual history of science are, and why these differences have opened up.

The most obvious and most debated epistemological issue of semantic incommensurability is the problem of scientific realism. 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, important ruptures at the level of "what science tells about the world", is, in itself, already a threat to any form of "correspondentist" or convergent realism (i.e., the thesis that the content of successive scientific theories approximately corresponds to the world and converges ever more toward the truth). Yet, the more precise claim that these descriptive scientific disruptions originate in deep differences rooted in the very resources of what is expressible, is far more subversive, since it throws into question the very formulation of the problem of the relation between theories devised by humans and their non-human referent. At least, it forces us to face seriously the idea of a constitutive power of language in science.

3. THE PROBLEM OF METHODOLOGICAL INCOMMENSURABILITY

Methodological incommensurability involves an irreducible incompatibility between the norms that underlie alternative programs of scientific investigation or traditions of research, for example, between the (often tacit) commitments about what is a genuine scientific problem, solution or proof. The most extreme case would be the situation in which no standards and values are shared between two traditions, each considering itself as scientific.

At the methodological level, the task is (1) to identify the efficient standards involved, which are, in normal conditions, largely tacit, intuitive in application, and by nature not precise; (2) to understand the way the different standards are or may be related to each other and to semantic differences between theories; (3) to evaluate how deep are or may be the methodological transformations encountered in the real practices, and to discuss why these transformations have occurred. At a more general meta-methodological level, the task is also (4) to reflect on the aim, the fruitfulness and the limits of any methodological theories of science.

The central epistemological issue commonly associated with this methodological incommensurability is the problem of scientific rationality and relativism. Relativism is here understood as the problem of knowing whether scientists have at their disposal, in each stage of scientific development, sufficiently good uniformly compelling reasons for deciding what is better at the level of validation procedures.

4. SCIENTIFIC CHANGE ENLARGED AND REFRAMED IN TERMS OF RUPTURES BETWEEN SCIENTIFIC PRACTICES

This is the traditional way to frame the problem of scientific change. The debate about these issues continues to be lively today. However, important shifts have taken place at the level of the interests of philosophers of science as well as historians, sociologists and ethnologists of science, and, correlatively, of the problem formulations. While interests and formulations were first mainly, if not exclusively, centred *on full-blown theories* usually understood as sets of explicit statements, more and more other dimensions have been claimed to be essential to the understanding of science and have been integrated into the picture as the investigation has continued. These include non-linguistic and partly tacit aspects such as know-how; experimental actions and material aspects constituting laboratory life; more or less local standards and values; commitments and projects of all sorts; not to mention psychological or social factors. Following what has been called the "turn to practice", the original issues have been re-framed in terms of scientific (theoretical or experimental) practices, if not in terms of cultural traditions socially labelled "scientific".

As a result, theories have progressively ceased to be reduced to sets of explicit statements, and scientific change has ceased to be thought of only in terms of theory change. The "New Experimentalists", in particular, have denounced the exclusive focus of philosophers of science on theories, representations, truth appraisal and epistemic issues. Particularly representative of these critics and their rhetoric is the contribution of Thomas Nickles in this volume. Paul Teller's paper, which adopts the "modelling approach to science", is also a case in point. It argues that most of the traditional issues related to incommensurability vanish if the philosopher stops taking truth as the aim of science and is instead attentive to a multiplicity of local and often pragmatic goals and values. As Ronald Giere points out in his commentary, Teller's conception leads us to be especially attentive to practical virtues in methodological appraisal – not just to cognitive values, or worse not just to one of them, the over-valued "truth" or "proximity to the truth". Real practitioners often evaluate the comparative merits of different available models on practical grounds (e.g., computational tractability). Anouk Barberousse, whose approach is more akin to the analytic tradition, also joins those who regret that philosophers of science have focused so much on theories as products. She wants to take into account the "first person use of theories". In this perspective, scientific theories are not just viewed as sets of objective scientific sentences that one can find in textbooks. They are also considered as sets of individually interpreted sentences.

As a special case of this general tendency, the linguistic characterization of incommensurability has been criticized as too narrow, dispensable or even misleading. In this volume these criticisms are articulated in Nickles' and Teller's articles. They are also echoed in several other papers. Bird, for example, stresses that "world change" is richer than a change at the level of meaning, that the focus on theories, concepts and language has been misplaced, and that although one side of incommensurability concerns understanding, incommensurability can perfectly well be characterized without recourse to meaning or linguistic issues. Aristides Baltas offers another illustration. By contrast with Bird, he finds the characterization of incommensurability in terms of concepts and language especially relevant (although he explicitly recognises that scientific practices are never merely linguistic). But the linguistic characterization of incommensurability he proposes is grounded in a shifted, enlarged conception of language inspired by Wittgenstein's famous dictum that "meaning is use". In such a framework, language is understood as an activity and the performative power of words is integrated into the picture.

Correlatively, the methodological side of incommensurability has been broadened, in the sense that conflicts between incompatible standards and values have been exhibited at many other levels than at the level of theory appraisal – although such cases have not always been discussed under the heading of methodological incommensurability. Several New Experimentalists, in particular, claim that there are significant conflicts of this kind at the experimental level, notably regarding what different practitioners consider to be a convincing experimental demonstration or a robust piece of evidence (e.g., Pickering, 1984, 1995; Galison, 1997). My contribution in this volume discusses aspects of these issues, and suggests that such cases should be recognized as local instances of methodological incommensurability (or "value incommensurability" to use the expression favoured by Giere in this volume).

Many authors urge, in the spirit of Kuhn's early writings, the need to examine the nature of scientific education in order to understand science, because specific features of this education appear responsible for essential characteristics of science. In this vein, scholars have especially insisted on learning processes which involve repetitive practice with exemplars. It is by such processes, they argue, that the scientific apprentice acquires the efficient (partly tacit) know-how and the background knowledge that render him able to solve new scientific problems by analogy with previously incorporated solutions. This has led to paying special attention to the tacit dimension of scientific building and scientific judgments.

Such a tendency is exemplified in several contributions of the book. For example, Bird suggests that incommensurability may be understood as differences in largely unconscious "quasi-intuitive cognitive capacities" which involve tacit background knowledge and commitments. He also relates the nature and efficiency of these quasi-intuitive cognitive habits to the process through which they have been learned, namely, repetitive training and exposure to standard exemplars. Baltas also makes room for unconscious background elements and is interested in educational aspects. In his Wittgenstein-inspired characterization of semantic incommensurability, he puts forward the hidden assumptions that constitute linguistic uses in general and scientific practices in particular. These background assumptions are tacit, self-evident and unquestioned ingredients that play the role of Wittgensteinian hinges and silently drive the understanding and the correct use of concepts. This is true for professional scientists as well as for students. Baltas attempts to draw the pedagogical implications of such a situation. He examines the resources that scientific professors have at their disposal when they teach new incommensurable concepts and theories to students who are entrenched in another incommensurable scientific paradigm. His proposal is to use nonsense, that is, to build bridge-sentences that are literally nonsensical but that do something (that are "performatively meaningful"). In his comment on Baltas' paper, Eric Oberheim enriches Baltas's proposal with other pedagogical resources that also involve pragmatic and tacit aspects, such as the use of analogies and metaphors. Barberousse's first person perspective also gives an important place to implicit aspects. In this conception, the different versions of the same scientific theory are analyzed as sets of sentences interpreted by different individual scientists from their own singular perspective, and the process of this individual appropriation – notably the process of individual understanding and development of concepts – is described, drawing on Robert Brandom's work, as a special kind of activity, namely, the activity of "making a claim explicit". The elaboration of a new version by a particular scientist is described as the act of making explicit, in an original manner, a theoretical content that, in a sense, was already there, although implicitly. This act, Barberousse insists, is not at all obvious or "automatic". It requires talent, creativity and a lot of know-how.

This being said, the emphasis on science as an activity, and the new interests that go with it, do not eliminate or dissolve traditional issues previously associated with linguistically and theoretically centred approaches. In particular, the central problem of theory comparison has not disappeared. This is why "theory comparison" is mentioned in the title of the book. It remains a genuine and important philosophical task, even if it does not exhaust the problem of scientific change and if its characterization must take into account other aspects of scientific practices than theories considered in isolation.

5. STILL ALIVE TRADITIONAL ISSUES RELATED TO THEORY COMPARISON AFTER THE TURN TO PRACTICE

The traditional problem of theory comparison, as it developed following the introduction of incommensurability by Kuhn and Feyerabend in the 1960s, has two related faces. Semantic incommensurability challenged the very possibility of a point-by-point comparison of subparts of old and new incommensurable theories, and called for a precise characterization of the relationships between alternative or competing theories. Methodological incommensurability challenged the universal, compelling and objective status of the criteria according to which practitioners evaluate the relative merits of competing theories. These two interrelated faces of theory comparison are still active problems, and many contributions to this volume are related to them.

With respect to the kind of relations that exist between successive theories, Stephan Hartmann argues that significant continuities always hold, although only partial and often complex ones, even when "scientific revolutions" occur. He elaborates a repertory of different types of partial correspondences between theories. This leads him to revisit the difficulties and ambiguities of the correspondence relations.

Robert Nola argues that even if a point-by-point comparison between semantically incommensurable theories is impossible, this is not at all a problem as long as we can separate the incommensurable theories from an independent set of reproducible observational effects consensually recognized as 'the effects these different theories are about'. In such cases, frequent in the history of science according to Nola, the stable set of reproducible effects points to the same thing (or possibly set of things) as their cause (despite the fact that the incommensurable theories conceive the thing or things very differently). In this manner, referential continuity is preserved despite drastic theoretical changes.

As for Barberousse, she insists that interest in scientific revolutions has led to a neglect of non-revolutionary developments, with the result that we are not well-equipped to think about the relation between different versions of one and the same theory. In other words, she addresses the issue of theory comparison in the case of intra-theoretical change (comparison between theoretical versions of a unique theory). Her problem is related to Kuhn's "essential tension": the aim is to reconcile the idea that different versions are essentially similar (otherwise they would not be viewed as versions of the same theory) with the intuition that a version can nevertheless introduce significant novelty and require great creativity. It is in order to make progress with this problem that she appeals to Brandom's notion of "making a claim explicit".

The difference between intra-theoretical and inter-theoretical changes, or between non-revolutionary and revolutionary scientific episodes, reactivates traditional unsolved difficulties that can be grouped under the heading of what I would call "theory individuation". As Igor Ly stresses in his comment on Barberousse's paper, the borderline between cases of the type "from one theory to a different theory" and cases of the type "from one version to another version of the same theory" is not so clear. In this respect the notion of "making a claim explicit" does not help as long as no indication is given about its conditions of application.

Problems related to theory individuation are also highlighted by Edward Jurkowitz in his comment on Hartmann's paper. The questions: "what a theory consists in?", "What a theory 'says'?", and "How far a theory must deviate from a 'core' in order to be a new (possibly incommensurable) theory or even a distinct theory?", are indeed, Jurkowitz argues, very difficult questions that must be decided before considering Hartmann's question "what are the correspondence relations between two theories?". In any case, in order to be in a position to answer, one has to specify when and for whom the answer is supposed to hold. This answer will in general differ for different periods and different subjects.

Several contributions to the book revisit the other face of theory comparison, that is, the comparative evaluation of competing theories and the power of criteria of theory choice. As has been well-known at least since Duhem, empirical and logical criteria are not sufficient to impose unambiguously a unique choice between a set of competing theories. Rom Harré attempts to fill the gap by a criterion of ontological plausibility. He argues that decisions of real practitioners, more especially decisions about which theory is the best candidate with respect to a realist reading, are indeed determined by such considerations, that is, by a requirement of ontological continuity. The point is advocated in the framework of an understanding of scientific activity as an activity of building models, with a special accent on iconic models. In this framework living scientists elaborate models about unknown entities by analogy with known observable entities, and this activity, as well as comparative judgments concerning the merits of different available models, are guided by the maxim: 'prefer the model coordinated with the ontology that is in best harmony with the actually admitted ontology'.

In her comment on Hervé Zwirn's paper, Soazig Le Bihan considers a contemporary, still unresolved case of theory choice which deeply involves the ontological preferences of practitioners: the choice between different (empirically equivalent) scenarios associated with the mathematical formalism of quantum physics. How will realistically inclined physicists be able to choose, for example, between an Everettian scenario and an (incompatible) Bohmian scenario?

With respect to the comparative evaluation of two semantically incommensurable paradigms, two contributors suggest that some objective asymmetries exist, which indeed crucially constitute in fact, and should constitute in principle, judgments of superiority among paradigms. In this vein, Giere argues that the Einsteinian theory is superior to the Newtonian theory, in the sense that on the basis of the former we can understand why the latter works as well as it does in its domain, or why it is impossible to shield gravitational forces, whereas the reverse understanding is not possible. In a similar vein, although within a very different framework, Baltas insists that the new vantage point of the post-revolutionary grammatical space allows a reinterpretation of (some aspects of) the pre-revolutionary one, whereas the reverse is blocked.

Martin Carrier also tackles the problem of underdetermination, but with respect to its implications for philosophical theories of scientific method. He first argues that both Duhemian and Kuhnian underdetermination of theory choice are inescapable and will thus never be solved by any methodology. This leads him to articulate the idea that we should modify the traditional conception of methodology. The aim of a methodological theory should no longer be identified as the exhibition of definite criteria that would unambiguously indicate the best theory among a set of competing ones – this would amount to assigning an unattainable aim to methodologies. Instead, Carrier suggests that the aim of a philosophical theory of scientific method should rather be to order, to classify and to make explicit the mutual relations of the multiple criteria used by scientists.

Faced with the same problem, Paul Teller takes another road. He argues that the problem is artificially generated by a misplaced exclusive valorisation of truth as the aim of science. According to him, traditional issues related to methodological incommensurability vanish once the philosopher renounces the ideal of exact truths, admits that science will never provide anything else than literally false idealizations, and understands that such idealizations give scientists exactly the same practical means as approximate truths would do. In such a perspective, the philosopher becomes convinced that it is desirable to develop a multiplicity of idealizations: indeed, each will have specific virtues, and altogether they will provide a high diversity of resources to the scientific community. In such a perspective the philosopher no longer understands theory choice as a definitive choice in favour of a unique theory with respect to its better proximity to truth, but as a multiplicity of contextual evaluations with regards to the respective capacities of available models as good means for achieving specific (often practical) ends: in Giere's words he becomes a theoretical pluralist. In the framework of such an instrumental rationality, a model that is elected as the best with respect to a given particular aim can be dismissed with respect to other ends. Because contextual instrumental evaluations of this kind are, according to Teller, most of the time consensual and unproblematic, the underdetermination of theory choice by experimental and logical criteria, or the impossibility of justifying criteria in the context of conflict between practitioners, are no longer a problem, or at least they become tractable practical problems.

6. THE SHIFT OF ISSUES ASSOCIATED WITH SCIENTIFIC CHANGE IN THE CONTEXT OF AN EMPHASIS ON LOCAL AND CONTINGENT ASPECTS OF SCIENTIFIC PRACTICES

The net effect of the move toward practice has been, at the level of general philosophical tendencies, an emphasis on local and contingent aspects of science.

The sensitivity to local and contextual features of science is manifested in several papers in the book. Nickles, for example, recommends a pragmatic and flexible conception of scientific method and points to the methodological opportunism of practitioners. Hartmann himself, although in search of a general Bayesian formalization of scientific development, describes the methodologist's task as the construction of a "toolbox" from which scientists may draw when needed, and insists that general methodological principles show their fecundity only when adapted to specific problems. His commentator Edward Jurkowitz, as an historian of science "for whom 'practice' has become a keyword, and who regularly insists on the determinative force of local and even culturally specific forms of theoretical practice", welcomes such declarations.

The issue of contingency is also considered in the book, although to a lesser extent. In the context of her reflection on intra-theoretical change in the case of the history of mechanics, Barberousse asks whether this scientific development was inevitable or not. She attempts to think through the relation between, on the one hand, the objective content of the theory – already there but only implicitly and potentially – which would be responsible for the inevitable aspects of scientific development, and on the other hand a subjective appropriation of this content by individual scientists, which would be responsible for the contingent aspects of scientific development. Following Ian Hacking, my own paper describes the opposition between contingentism and inevitabilism, and identifies it as one of the implications of a possible incommensurability at the experimental level. Other papers encounter this issue in a more peripheral way (see, e.g., Nola Sect. 3.2 or Nickles Sect. 2).[2]

The stress on local and contingent aspects has thrown into question the pretension of science to universality, be it at the level of scientific results or at the level of scientific methods. New varieties of relativist positions have emerged. The possibility of a general picture of science has been questioned in principle. Some have endorsed a radical historicity. In any case, the multiplication of very particular case studies, while it has clearly enriched our approach to science, has correlatively increased the difficulty, for philosophers of science, of the elaboration of a credible global understanding of science. At the same time, many such philosophers think that this is the most important challenge in the current situation.

Many contributors to the volume seek such a global understanding. Bird's account of incommensurability as differences in cognitive habits is indeed a very general picture that purports to be valid for all cognitive processes, included those occurring

[2] On the contingency issue, see L. Soler and H. Sankey eds. (forthcoming). This is a symposium devoted to the contingency issue, with contributions from Allan Franklin, Howard Sankey, Emiliano Trizio and myself.

outside of science. Hartmann's explanation of continuity in science with the help of a Bayesian framework also vindicates a very general validity. Even if the point is questioned by his commentator, Hartmann's research program illustrates the enduring search for general characterizations of science. Teller's and Giere's pluralist and perspectivist picture driven by an instrumental rationality can also be viewed as a description of very broad scope. As for Michel Bitbol, in his comment on Carrier's paper, he endorses a neo-Kantian stance which involves a quasi-universal methodological principle characteristic of the scientific enterprise: the quest for maximal objectivity in a Kantian sense, that is, something closely linked with the search for maximal invariance. The principle takes its quasi-universal status from the vindication that it is built into the very idea of science, that it is constitutive of the very project of science and can thus be seen as one of its conditions of possibility.

Along with the emphasis on contextual and contingent determinants in science, the move toward practice has raised new questions and altered the answers to traditional ones. Here are some examples. What is the exact function of high-level theories, and of competition between them, in scientific research? Are they only one element among many others, no more important than others or indeed, sometimes at least, far less important? How should scientific progress be redefined to take into account the contextual, local and often contingent aspects of scientific practices? When material things, actions, future-oriented concrete projects involved in the practice of science are put forward to the detriment of theoretical representations, are we forced to leave traditional varieties of scientific realism?

7. REALISM VERSUS ANTIREALISM: OLD AND NEW FORMS

Scientific realism remains a central issue in the philosophy of science. Already before the turn to practice, many Kuhnian-inspired philosophers claimed that realism was no longer credible as a consequence of the existence of semantic and methodological incommensurabilities. Within the pragmatic orientation, new and often weaker forms of realism have been introduced, for instance Hacking's and Cartwright's "entity realism", or Pickering's "pragmatical realism" (Hacking, 1983; Cartwright, 1986; Pickering, 1989, 1995). The realist issue, in some of its new forms, and the division between realists and anti-realists, are well represented in the book.

On the side of realist-inclined philosophers, we find notably Rom Harré, Robert Nola, Steve Clarke and Howard Sankey. In his contribution to the present volume, Harré actually does not directly argue for realism. But clearly, a philosopher who accepts the theses he advocates in his paper is in a position to consider the realist stance with sympathy, in agreement with the fact that this is the stance Harré in fact favours. In particular, Harré claims that a realist interpretation of quantum physics, grounded in an ontology of causal powers, is possible. This being said, the theses Harré articulates in his paper do not imply realism, as Suárez insists in his comment. Harré's description of scientists as realists, and his description of ontological plausibility as a criterion that guides model building and the comparative evaluation of models, do not imply, or even strongly favour, the metaphysical thesis of scientific realism. The requirement of ontological continuity, and the ontological stability that the history of science displays as a result, can, Suárez argues, be explained on other grounds in an instrumentalist spirit. For example, one can invoke convenience and economy, that is, reasons that are related to features of human cognition rather than to properties of the world. In other words, the fact that practitioners favour realist readings of models and use criteria of ontological plausibility can be viewed as a good scientific policy (given some characteristics of human cognition) but is not a guarantee that scientific theories indeed pick out components of an independent world.

Robert Nola does not directly address the realist issue either, but he argues for a thesis that is traditionally used to support scientific realism, namely denotational and referential continuity. Taking this for granted, his commentator, Steve Clarke, advocates a particular form of realism, entity realism. According to Nola, the entities that are denoted by theoretical terms can be identified as (roughly) the same, even when radical changes occur in scientific theories. Hence we can conclude that most of our successive past scientific theories refer to the same entities despite the fact that these theories assume radically different conceptions of the entities involved. On this basis, an optimistic meta-induction (OMI) leads us to admit that the same will continue to hold for future theories. Admitting these conclusions, Clarke dissociates OMI and realism about entities on the one hand, and OMI and realism about theories on the other hand, and argues that denotational continuity through theoretical revolutions, at the same time supports entity realism and dismisses realism about theories. Following Hacking and Cartwright, he relates referential success to the possibility of an experimental manipulation of the corresponding entities.

As for Howard Sankey, he claims, in his comment on my paper, that semantic incommensurability, because it is only local, poses no threat to scientific realism. Paul Hoyningen-Huene expresses a different view in his commentary on Bird's paper. In the context of a neo-Kantian framework according to which subject-sided features of scientific theories can never be subtracted from object-sided features, he expresses his conviction that incommensurability, through the experience of world change, essentially challenge and, according to him, has "seriously undermined" any form of scientific realism. Hervé Zwirn, for his part, discusses some aspects of a realist interpretation in the special case of quantum physics equipped with the decoherence theory. His more general position, which is only sketched in the present paper but argued in several writings, is that quantum physics cannot be interpreted as an (even approximate) description of an independent reality. Soazig Le Bihan lists the special difficulties one has to face in order to sustain a realist interpretation of the quantum theory, given, on the one hand, the tension between the uniqueness of measurement results and the multiplicity of the superposed states of the formalism, and, on the other hand, the fact that several empirically equivalent ontological scenarios can be associated with the formalism. Michel Bitbol, who in company with Hoyningen-Huene favours a neo-Kantian orientation, starts with the claim that scientific realism lacks a solid methodological ground, and from there concludes that (approximate) truth must be integrated into our account of science not as an actual achievement but as a regulative ideal.

8. DISCOVERING NEW ASPECTS OF SCIENCE: PRACTICAL REVOLUTIONS? NEW KINDS OF INCOMMENSURABILITY?

The move toward practice has led a number of philosophers of science to claim that it has shed light on important but previously invisible aspects of science. In this vein, Nickles' contribution argues that when we are attentive to pragmatic and future-oriented aspects of science, we are led to discover new kinds of scientific revolutions: disruptive changes that do not obey the Kuhnian scheme. These revolutions are practical disruptions: they are competency destroying. They are ruptures at the level of skills, practical standards, practical expertise and heuristic appraisal. They involve what Nickles calls a "recognition problem": before and after, what appears relevant, fruitful, reliable and legitimate to the field can change drastically. Such disruptions, Nickles claims, often go with a new mapping of the speciality areas and may be relatively independent of theoretical revolutions. In his comment, Emiliano Trizio specifies the characterization by distinguishing two components of Nickles' recognition problem: conflicts between judgments of relevance and conflicts between judgments of fecundity. Only practical disruptions that involve disagreements at the level of relevance might induce the re-structuring of scientific specialties.

In a similar vein, "New Experimentalists" such as Andrew Pickering and Ian Hacking have argued that there is a new form of incommensurability that is found at the level of experimental practices.[3] This form of incommensurability has been overlooked by traditional philosophers of science because of their theory-dominated orientations and exclusive concern with language-based characterizations of incommensurability. Such an incommensurability obtains between scientific practices that have stabilized on the basis of different measurement procedures, instruments, machines and laboratory practices. As a result, the practices are, in Hacking's terms, "literally" incommensurable, in the sense that there is, properly speaking, no shared physical measure between them. (Hacking talks about a "literal incommensurability" and Pickering about a "machinic incommensurability"). Claims of this kind open a possible new field of discussion, devoted to the novelty of what is at stake, to its epistemological implications and its exact relations with the traditional semantic and methodological forms of incommensurability. My paper and Sankey's comment on it address several aspects of these questions.

9. A NEW INTEREST IN SCIENTIFIC STABILITY, CONTINUITY AND CUMULATIVITY

The shift of focus, in the philosophy of science, from theory to practice, is not the only change that deserves mention. If it was discontinuities, ruptures, revolutions and incommensurabilities that previously struck philosophers of science, in recent times,

[3] See my paper for all the references.

an increasing number of authors have insisted on the need to attend to important continuities and stabilities that science manifests. Ian Hacking (1999) expressed the point in a striking manner, in his book, *The Social Construction of What?*: "The old idea that sciences are cumulative may reign once more. Between 1962 (when Kuhn published the Structure) and the late 1980s, the problem for philosophers of science was to understand revolution. Now, the problem is to understand stability."

This is the reason behind the third element of the subtitle of this book, "stabilities", side by side with "ruptures" and "incommensurabilities". In the present volume, this renewed interest in scientific stability is manifest in the contributions of several authors: Hartmann, who presents a repertory of different types of continuities or "correspondences" between theories; d'Espagnat, who argues that science is cumulative since predictions of observations and recipes for experimental actions are themselves cumulative; Harré, who stresses ontological continuity as a policy of research; Nola and Clarke, who argue for denotational and referential continuity.

10. THE SEARCH FOR CONTINUITY AS AN OLD PHILOSOPHICAL STRATEGY AGAINST THE INCOMMENSURABILITY THESIS

With respect to stability, continuity and cumulativity, we can note that, from the outset, it has been a common strategy to look for stable items in the history of science in order to bypass the alleged undesirable consequences of incommensurability, or more exactly, to confine the ruptures to a certain level of science, and to claim that, at another level, we find trans-paradigmatic invariants that can be identified as the carriers of a cumulative scientific progress. This has been the strategy of many realists with referential invariants (Nola's research program can be seen as a new attempt of this kind); the strategy of some rationalists with methodological invariants; as well as the strategy of some instrumentalists and pragmatists with predictive and performative invariants.

Along related lines, and in the context of the discussion of the semantic incommensurability of theories, it has been early recognized that in order for semantic incommensurability to create an epistemological problem, the semantic ruptures have to arise between *competing* theories, that is, between theories that must have something significant in common (otherwise they would not be in a position to compete). The claim that physics and psychoanalysis are two incommensurable theories, for example, does not seem to have any epistemologically damaging implications, since such theories are not supposed to be rival theories. Competing theories are theories that must have some important common points. They must notably take some common phenomena as relevant to their field, which means that they must be connectible, at a certain level, with a set of common "basic statements" (in Popper's sense) and hence with some common semantic categories (Soler, 2003). This is an in-principle minimal requirement, to which it has been added that in fact, the prototypical pairs of contemporary incommensurable theories share more than that: they always share some theoretical problems and theoretical concepts. In other words, the incommensurable areas are circumscribed, the incommensurability is only local (as Kuhn (2000a) explicitly recognized in 1983) – although it can be *more or less* local. In brief, semantic incommensurability cannot be understood in too extreme a sense. We cannot understand "incommensurable" too literally: philosophically interesting semantic incommensurability presupposes some common points between theories. Hence there must be a minimum of continuity in the history of science, even when a revolution occurs. This has sometimes appeared paradoxical, but this is one way to understand Kuhn's "essential tension" between radical novelty and the need of a certain conservation of the tradition. A new theory would not be recognized as relevant (and a fortiori as better) to an ancient one, if the new one shared nothing in common with the old and could not be connected with anything of the old tradition.

This problem arises in the context of my own paper and Howard Sankey's commentary. I extend the "quasi-analytic" association between rivalry and incommensurability verdicts to the case of experimental incommensurability. Sankey points to difficulties that have never been fully resolved in the discussion of semantic incommensurability, in particular, the problem of how assertions stated in semantically incommensurable languages can be rivals, while the absence of shared meaning entails that they are unable to enter into a relation of conflict. He argues that structurally analogous difficulties arise in relation to the experimental incommensurability proposed by Hacking and Pickering. For it remains to be explained how exactly different sets of instruments or experimental practices are able to enter into a relation of rivalry with one another.

11. PHILOSOPHICAL ISSUES ASSOCIATED WITH THE THESIS OF SCIENTIFIC CONTINUITY

With respect to scientific stability, the philosophical task is to identify what exactly has remained stable and grown cumulatively in the history of science, to evaluate the significance of the stable core that can be isolated in retrospect, and to propose an explanation of the observed stability. This task, originally focused on theories, has been directed as well toward scientific practices. With respect to this task, we can identify the following difficulties.

The first difficulty is to identify the alleged stable core. This is not so easy, especially if we take into account the partially holistic nature of science and knowledge, and consider that the answer will be heavily dependent on what we take to be the origin of genuine science. As Hartmann stresses, there are always multiple possible relations between theories and multiple correspondence strategies. Even if continuity is real, continuity appraisal is ambiguous. It depends on an evaluation of the significance of the multiples similarities and differences that always exist. Where some philosophers see continuities (as does Nola in the case of the history of the electron), others might see ruptures (see, e.g., Kuhn's (2000b) discussion of Putnam on the case of water).

The second difficulty is to evaluate the significance of the isolated stable core: it is always possible, retrospectively, to find common points between two stable stages of scientific development, otherwise we would not be ready to consider both of them as "scientific" and we would not be inclined to see them as two entities that call for a comparison. But it remains to discuss if the stable layer that is recognized to be shared by two stages of scientific development is sufficiently representative of the old science, or is nothing more than a minor, a posteriori artificially extracted part, of the beliefs and techniques that were efficiently at work in the old scientific practices.

Especially significant in this respect is the dialogue between Bernard d'Espagnat and Marcel Weber. D'Espagnat sees a significant continuity where Weber, who represents Kuhn's point of view, diagnoses a radical discontinuity. The dialogue illustrates how the description of the same historical sequence may differ, depending on what matters to the eyes of the analyst. D'Espagnat, who represents the point of view of the physicist, argues that cumulativity at the level of predictions is sufficient to conclude to the cumulativity of science. Weber, who represents the point of view of the historian and is anxious to take into account the experience of practitioners at the time, disagrees. According to him, for a scientist who experienced a revolution and came to adhere to a new paradigm, the change is radical. It is artificial and misleading to isolate a set of stable predictions from the other elements that went with them at the time and gave them sense.

The third difficulty lies in the explanation of the stability. Are we going to explain it on the basis of constraints coming from the supposedly unique invariant referent of the scientific study, in the spirit of scientific realists? Or are we going to explain it on the basis of more or less universal human categories, schema and commitments, for example commitment to conservatism, economy, convenience or the like (in the spirit of Suárez's suggestions in his comment on Harré's paper)? Or are we going to introduce into the picture elements such as the necessity for scientists to manifest their affiliation to the tradition in order to promote their careers? Or are we going to sustain any possible combinations of these options? Hartmann addresses this problem and looks for a solution that includes both "internal" and "external" factors. He attempts to explain scientific continuity in the framework of what he calls an "epistemological coherentism". According to this conception, scientists would strive for continuity, and should strive for continuity, because continuity increases coherence (leads to a better "hanging-together") and because "coherence is truth-conducive". Hartmann tries to build a coherence measure in a Bayesian framework.

The fourth difficulty lies in the legitimacy of projecting the present stability in the future and in the status ascribed to present stable results: can we admit that what we take to be as the common stable core of science now, will remain, for the most part at least, the stable core of future science, as realists and inevitabilists suggest? Can we consider that what is presently taken as a stable set of sound scientific results had inevitably to occur? Barberousse's paper and my own touch on these questions.

Acknowledgments

My gratitude goes first of all to Howard Sankey, co-editor of this volume, for his assistance in the preparation of this volume, and for interesting intellectual exchanges associated with this task. I am grateful to him as well for his support, his good humour, and the amount of time he spent improving my English expression.

I am grateful also to Paul Hoyningen-Huene, co-editor of this volume and co-organizer of the conference held in Nancy in June 2004, as well as to the other members of the scientific committee, Gerhard Heinzmann, Howard Sankey, Marcel Weber and Hervé Zwirn.

I would also like to thank all of the contributors to this volume and participants in the conference, both for the quality and openness of exchanges, as well as for the rigour and care with which they have undertaken detailed revisions of the papers they have contributed to this volume.

I wish to express my gratitude to the Henri Poincaré Archives (Laboratory in Philosophy and History of Science, UMR CNRS 7117), which provides researchers with not only an exceptional intellectual context but a convivial atmosphere, especially due to the personality of its Director, Gerhard Heinzmann, but also due to the solidarity of its members, which is even more effective because it is independent of any hierarchical structure.

Neither the conference nor this volume would have been realized without the financial support of a number of institutions. I wish in particular to thank the National Scientific Research Centre (Centre National de la Recherche Scientifique); the Urban Community of Grand Nancy; the General Council of Meurthe and Moselle; the Regional Council of Lorraine; the Scientific Council, Department of Philosophy and the “UFR Connaissance de l’homme” of the University of Nancy 2; the National Polytechnic Institute of Lorraine (INPL); the French Minister of Research; and the University of Nancy 1. I would also like to thank the two institutions responsible for the organization of the conference, the Center for the Philosophy and Ethics of Science of the University of Hanover and the LPHS–Henri Poincaré Archives of Nancy.

Many thanks to Alexander Bird, Ron Giere, Hania Grosbuch, Rom Harré and Tom Nickles for their help in the improvement of the English language of the final version of this manuscript, as well as to Charles Erkelens and Lucy Fleet, in the Editorial Office at Springer, for their advice, support, and patience.

Finally, thank you to Emiliano Trizio for his assistance on numerous occasions, especially for English corrections, and for his friendship.

BIBLIOGRAPHY

Cartwright, N. (1986) *How the Laws of Physics Lie*. Oxford: Clarendon Press/Oxford University Press.

Galison, P. (1997) *Image and Logic, A Material Culture of Microphysics.* Chicago, IL: University of Chicago Press.

Hacking, I. (1983) *Representing and Intervening. Introductory Topics in the Philosophy of Natural Science.* Cambridge: Cambridge University Press.

Pickering, A. (1989) Living in the Material World: On Realism and Experimental Practice. In *The Uses of Experiment, Studies in the Natural Sciences.* Cambridge: Cambridge University Press, pp. 275–297.

Hacking, I. (1999) *The Social Construction of What ?* Cambridge, MA: Harvard University Press.

Hoyningen-Huene, P. and Sankey, H. (eds.) (2001) *Incommensurability and Related Matters.* Dordrecht, The Netherlands: Kluwer.

Kuhn (2000a) Commensurability, Comparability, Communicability. In J. Conant and J. Haugeland (eds.). *The Road Since Structure. Philosophical Essays, 1970–1993.* Chicago: University of Chicago Press, pp. 33–57.

Kuhn (2000b) Possible Worlds in History of Science. In J. Conant and J. Haugeland (eds.). *The Road Since Structure. Philosophical Essays, 1970–1993.* Chicago: University of Chicago Press, pp. 80–86.

Pickering, A. (1984) *Constructing Quarks, A Sociological History of Particle Physics.* Chicago, IL: University of Chicago Press.

Pickering, A. (1995) *The Mangle of Practice. Time, Agency and Science.* Chicago, IL: University of Chicago Press.

Soler, L. (2000) *Introduction à l'épistémologie.* Ellipses, 2000.

Soler, L. (2003) De la commune mesure phénoménale entre théories physiques dites incommensurables, *Philosophia Scientiae*, 7(2), 239–265.

Soler, L. (2004) The Incommensurability Problem: Evolution, Approaches and Recent Issues. In *Le problème de l'incommensurabilité, un demi siècle après/The incommensurability Problem, Half a Century Later. Philosophia Scientiae*, 8(1), 1–38.

Soler, L. and H. Sankay eds. (forthcoming) Are the results of our science contingent or inevitable?, *Studies in History and Philosophy of Science*.

PART 1

INCOMMENSURABILITY, AS DIFFERENCES IN QUASI-INTUITIVE COGNITIVE CAPACITIES: A TASK FOR PSYCHOLOGY?

INCOMMENSURABILITY NATURALIZED

ALEXANDER BIRD

Abstract In this paper I argue that we can understand incommensurability in a naturalistic, psychological manner. Cognitive habits can be acquired and so differ between individuals. Drawing on psychological work concerning analogical thinking and thinking with schemata, I argue that incommensurability arises between individuals with different cognitive habits and between groups with different shared cognitive habits.

Keywords Kuhn, incommensurability, world change, paradigm, exemplar, cognitive psychology, analogy, schema, model.

1. INTRODUCTION – INCOMMENSURABILITY AND QUASI-INTUITIVE COGNITIVE CAPACITIES

Incommensurability was one of the primary topics in the philosophy of science of the later twentieth century, having been introduced principally by Thomas Kuhn while also vigorously promoted by Paul Feyerabend. In *The Structure of Scientific Revolutions* (1962) Kuhn discussed two kinds of incommensurability – incommensurability of standards and incommensurability of meaning. He spent much of the rest of his career developing the latter using various ideas from the philosophy of language. Interest in such "classical", Kuhnian accounts of incommensurability has declined from its peak, largely because most philosophers hold that other developments in the philosophy of language have shown how incommensurability may be defeated or at least deflated (as is amply demonstrated in the work of Howard Sankey (1994, 1997)).[1]

Kuhn's later work on incommensurability contrasts with the tenor of much of *The Structure of Scientific Revolutions*. The latter I regard as having a strong naturalistic streak that was lost in Kuhn's subsequent discussion of incommensurability, which was much more standardly philosophical and aprioristic in character (Bird, 2002, 2005). While I agree with Sankey's criticisms of meaning incommensurability, I believe that a return to Kuhn's earlier work will provide us with the starting point for a rather different approach to the phenomenon of incommensurability. First, we may revisit the issue of incommensurability of standards that Kuhn did not develop in much detail. And, secondly, we may explore

[1] Declined but very far from evaporated. See Hoyningen-Huene and Sankey (2001) and this volume as examples of considerable continued interest in the general issue of incommensurability and in allied issues.

L. Soler, H. Sankey and P. Hoyningen-Huene, (eds.), Rethinking Scientific Change and Theory Comparison: Stabilities, Ruptures, Incommensurabilities, 21–39.
© 2008 *Springer.*

how Kuhn's early naturalistic account of the functioning of paradigms (as exemplars) may furnish a naturalistic, primarily psychological conception of incommensurability.

The key idea in what follows is that we all use in thinking various cognitive capacities and structures that have the following features: (i) they cannot be reduced to general, formal rules of reasoning (e.g., formal logic); (ii) their existence and the mechanism of their employment are typically unconscious, so that they are deployed in a manner that is akin to intuition – what I call a *semi-intuitive* manner; (iii) they are often acquired as a matter of practice and repeated exposure, so that they have the character of skills. The sorts of skill or capacity I am referring to here include: mental schemata, analogical thinking, pattern recognition, quasi-intuitive inference. As I shall describe below, these are related, and together I shall refer to them as an individual's quasi-intuitive cognitive capacities (QICCs).

The proposal of this paper is that as a result of social induction with a set of paradigms a scientist in a given field acquires a set of QICCs specific to that field. Incommensurability arises when scientists operate with different QICCs; this incommensurability is an incommensurability of standards and an incommensurability of understanding (which is not quite the same as an incommensurability of meaning, unless meaning is understood in the relatively loose sense corresponding to the intended message of a communication). Such incommensurability may arise in communication between radical and conservative scientists during a scientific revolution, in the understanding of the work of scientists from another era, and also between scientists working on similar problems but from differing background fields.

2. EXEMPLARS IN SCIENCE – AN EXAMPLE

Much cognition is habitual. These habits are acquired. This occurs in scientific thinking no less than in informal, everyday cognition. Consider the following example of a test question that might be set for physics students:

> A thin rectangular plate, *O*, of mass *m* is free to rotate about a hinge at one edge. The length of the plate from the hinge to the other edge is *l*. It is placed within a stream of fluid which exerts a pressure P in the direction of flow. Let θ be the angle between the plate and the direction of fluid flow. Write down an equation describing the motion of *O* in terms of the change of θ over time, assuming θ is small. (See *Fig. 1.*)

Many will be able to see immediately that the motion will be simple harmonic and will be able to write down the equation of motion straight away as having the form: $\theta = \theta_{max} sin(kt)$. In some cases this will be assisted by seeing an analogy between the oscillating plate and a rigid pendulum (see Fig. 2). The motion of the

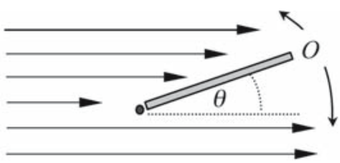

Fig. 1.

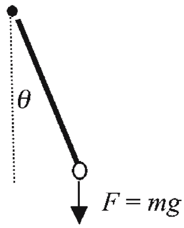

Fig. 2.

pendulum is the first and best known instance of simple harmonic motion that any student of physics gets to meet. Seeing the analogy will thus enable the student to know the correct equation of motion, or at least its form, without further ado. The analogy in this instance will be easy to spot, since the standard diagram for a pendulum is very similar to that employed in the question given, rotated through a right angle.

This example illustrates important aspects of cognition in science:

(i) The employment of paradigms. Kuhn (1962) defines paradigms as standard exemplars of problems and their solutions. In this example, the motion of the pendulum and its standard solution are a paradigm.

(ii) Analogical thinking. The use of a paradigm is essentially analogical. The analogy between the new problem of the oscillating plate and the well-known case of the pendulum shows or suggests to the student that the equation for the former will have the same form as that for the latter.

(iii) Scientific cognition is not a matter of following very general rules of rationality (where "rule" is understood in a strong way, implying that it can be followed mechanically). (This was a common background assumption among many logical empiricists, and the significance of paradigms in Kuhn's work is that they provide a refutation of the logical empiricist and positivist view of scientific cognition.)

(iv) Learned, quasi-intuitive inference patterns and associations: the ability to answer immediately or via the pendulum analogy is quasi-intuitive. It is like intuition in that it is non-reflective or only partially reflective, being a more-or-less direct inference (rather than one mediated by the sort of rules mentioned in (iii)). But it is unlike intuition in that it is learned, acquired by training with paradigms/exemplars (e.g., many student exercises are of just this sort).

(v) Cognitive processes can be acquired habits. The quasi-intuitive inferences mentioned in (iv) are made as a matter of habit. The habits are acquired as a result of repetitive exposure and practice. For example, what may start out as a conscious, sequential activity of reasoning, eventually becomes a one-step quasi-intuitive inference. This does *not* mean that the same process of sequential reasoning takes place unconsciously. Rather the habit replaces the conscious reasoning process.