Riccardo Viale

Methodological Cognitivism

Vol. 2: Cognition, Science, and Innovation

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Riccardo Viale
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To my wife Caterina
To her spiritual serenity and her intellectual liberty

Foreword

When I started to work in 1987 on my book on cognitive theory in science, *Metodo e Società nella Scienza* (1991), the philosophy of science was dominated by the confrontation between those who supported methodological rationality and those who did not. On the one hand there was a diminishing band of Popperian and post-Popperian philosophers and on the other the growing adherents of relativism or the methodological anarchism of Kuhn, Feyerabend and the sociology of science. The issue at stake continued to be the context of justification, which was backed by the Popperian tradition and denied by its adversaries who instead proposed a vision of science dominated solely by the context of the discovery. Analysing the dynamics of scientific change could only be done by reducing scientific "facts" to their social, cultural and economic causes. There was no other way of establishing any rational principle for theoretical change in science. The context of justification was eliminated by that of discovery. At that time, the standoff between discovery and justification was the mainstream of philosophy of science. Both the opposing camps regarded an "equal" connection between discovery and justification as unthinkable. One was either in the rationality camp or in that of social relativism. "Tertium non datur". Therefore, when I started to put forward some of the theses that were then developed in the book, linked to the identification between justification and discovery and to the cognitive foundation of scientific rationality—namely to a foundation of rational justification linked to discovery—I found myself being attacked on all sides. I had already experienced this type of hostility from the philosophical world directly a few years earlier in Oxford in 1984. When I proposed the outline for my doctorate thesis in the Philosophy of Science, I had to overcome a great deal of hostility and disparagement on the part of my supervisor towards the issue of the *cognitive theory of science*, which was very important to me. In Europe, in particular, the neopositivist culture still prevailed, maintaining a clear separation between the context of discovery and justification, an opinion strenuously sustained even by a non-neopositivist like Sir Karl Popper. Methodological rationality could not be analysed empirically because of the risk of falling prey to the mortal sin of naturalistic fallacy. The naturalistic revolution of W.O. Quine and the contribution of philosophers like Steven Stich or Alvin Goldman were not represented in the philosophy of science. Even Larry Laudan, who had considered the problem of the empirical–historical evaluation and justification of methodological rationality, was not part of the mainstream of the philosophy of science and was underestimated in

Europe. My work in Oxford and my subsequent work at the L. Bocconi University of Milan, where I began to teach Logic and Epistemology as part of the course in Economic and Social Disciplines (DES) in 1987, provided the basis for my book *Metodo e Società nella Scienza*, which was published in 1991, soon after Ronald Giere published his *Explaining Science* in 1988. Both set out to propose a naturalistic approach to the philosophy of science. Mine also argued in favour of the explanatory supremacy of the cognitive approach over the social approach in the study of the dynamics of conceptual change in the scientific community. The two books on *Methodological Cognitivism* are both a synthesis of my work on the *cognitive theory of science* and the start of two working programmes, one in the epistemology and methodology of social sciences designed to overcome the intentionalism and rationalism inherent in the current Methodological Individualism, and the other designed to develop the cognitive foundations of social rationality, with particular reference to the economic context (both are developed in the first volume).

This book, Cognition, Science, and Innovation, tackles a classic philosophical question, that of causality, in the first part, before going on to present a new version of the cognitive theory of science in the second. The work on causality is the result of a research project undertaken at CREPCO, at the University of Aix en Provence. The approach taken to causality is typical of experimental philosophy. The psychological modalities of causal reasoning are identified, from its infancy to today, and they are compared with epistemological and metaphysical models of causality in order to highlight possible dyscrasias and infeasibilities. Based on this analysis, the study attempts to justify causal realism by using the results of evolutionary and developmental psychology.

This book then tries to query the implications of methodological cognitivism in the context of scientific policy and innovation. The topics covered in the third and fourth parts of the book relate to a study carried out from the late 1990s to the present at Fondazione Rosselli in Turin, at Fondazione Cotec in Rome, at the Scuola Superiore della Pubblica Amministrazione in Rome and during various meetings held as part of the Triple Helix international network. What should be the bases for policy choices in research? Does knowledge matter to politics? Has truth any role in public policy? What institutions should be promoted in order to ensure that science is more effective in producing valid knowledge? These are some of the questions that the book tries to answer by introducing a form of social epistemology, inspired by a variant of realism dubbed as "Cognitive Realism". The subject of institutional values as the foundations for an economy's ability to innovate is highlighted in the third part of the book where, starting with a historical reconstruction of the origins of the phenomenon of permanent innovation, I argue how epistemological and cultural incentives have played a determining role alongside economic factors.

The concluding part of the book examines the application of methodological cognitivism to the analysis of models of knowledge transfer between university and enterprise. Part of this work was completed in 2009 during my Fellowship at the Italian Academy of Columbia University. It starts by emphasising the tacit

dimension of knowledge as a fundamental factor in knowledge transfer. However, this does not only involve skill-based characteristics, as tends to be underlined in the economic literature. Other components linked to background knowledge, and above all those from the cognitive part of knowledge, play an important role in the transfer process and its obstacles. Starting from this premise, the tendency is to develop an analysis of which value components and cognitive rules should be the focus of research on the challenges of technology transfer. The book hypotheses a series of differences in the social and working values of industrial and academic researchers, which influence phenomena like linguistic communications, group work, reasoning, decision-making, problem-solving, etc. These differences allow a greater understanding of the best institutional and organisational configurations to adopt in order to maximise the possibilities of collaboration and knowledge transfer. Lastly, an important chapter addresses the implications of epistemological and computational complexity of knowledge in promoting forms of organisation that can effectively deal with scientific and technological problems. This part could be summed up by the slogan, "from computational complexity to organisational complexity", with all the implications that this outcome has on the study of organisational assets using complexity theory models such as Complex Adaptive Systems.

Before this second volume a first volume was published in 2012 with the title *Mind, Rationality, and Society*. In the first volume I introduced the term "Methodological Cognitivism" to define the proposal of an individualistic methodology of social sciences, which based its theory of action on a causal model of the mind–action relationship that was cognitive and not intentionalistic or rationalistic. To develop this programme, I had to analyse a number of issues related to the philosophy of mind, the epistemology of causality, the methodology of social sciences and the cognitive models of reasoning and decision making. In the years between 1997 and 2001, some of these issues were refined at the meetings of the Scientific Network of the European Science Foundation "Human Reasoning and Decision Making", particularly the encounters at the Sorbonne, Paris IV and the College de France. My recent work on methodological cognitivism, which I developed in the direction of neurocognitive social sciences and the theory of mirror neurons in particular, was triggered by a theory of the strong identity between the mind and the brain. My work on the cognitive theory of rationality, particularly in an economic context, evolved in parallel in that same period, from 1987 on. From the 1988 seminar with Herbert Simon at the Rosselli Foundation in Turin, until the conference on "Cognitive Economics" at the Bocconi University in Milan in 1996, the subsequent creation of the academic journal *Mind & Society* in 2000 and, after Simon's death, the foundation of the Herbert Simon Society in 2008, numerous initiatives have enabled me to develop Simon's programme. The term "Cognitive Economics", which I coined at the conference in 1996 and subsequently returned to in the publication of the proceedings *Cognitive Economics* (1997), underlines an "extreme" attempt to overcome the epistemological bottlenecks of experimental economics and the scientific bottlenecks of behavioural economics. Only a reformulation of the theory of economic action according to the

best models of neurocognitive sciences can stimulate positive economics and its realistic and empiric goals. And finally, we come to the empirical work undertaken with Dan Osherson from 1995 on the inductive models of reasoning based on categories (category based induction). This work was undertaken in Italy, but also in Vietnam at the University of Ho Chi Min City, and in New Delhi at the Indian Institute of Technology, and it allowed us to study the effects of the membership of different cultural or social groups on inductive reasoning among adults and children, and in particular the application of the difference principle and the Bayesian model. These studies of cognitive anthropology also contributed to the debate taking place in the scientific community, between supporters of relativism and of epistemological and cognitive universalism and between those of innate bases or the cultural bases of the inferential capabilities of the human brain.

Both volumes are characterised by a common denominator: Methodological Cognitivism as the new methodological tool to analyse social action.

I have had the good fortune to develop some of these topics during the classes at the Bocconi University in Milan, at State University of Milan, at Milan Bicocca University and at the Scuola Superiore di Pubblica Amministrazione in Rome, and during periods spent at foreign universities such as Oxford, Aix en Provence, Fribourg, Rice-Houston, California-Santa Barbara and Columbia.

Cognition, Science, and Innovation is the second book of Selected Papers on *Methodological Cognitivism*. Most of the chapters are modified versions of earlier publications. In this regard, I wish to first thank Laura Gilardi, without whose punctilious and professional editorial assistance I would not have been able to complete the work.

My main scientific debt is to Herbert Simon, who has been a constant benchmark for my work over the years.

Some months ago Raymond Boudon sadly passed away. As the social scientist whom I felt was closest to my work, his open and generous personality allowed me to learn a lot about the methodology of social sciences. Among those who are no longer with us, I am particularly indebted to Norberto Bobbio, who taught me to see new horizons in the relationship between the philosophy of science and human sciences. I must also mention Kathy Wilkes for her important comments on the philosophy of mind and Martin Hollis for his considerations on the theory of rationality.

My encounter with Sir Karl Popper marked an important moment in my development; his intellectual reference has been fundamental throughout the years.

I am grateful to Giulio Giorello, Angelo Petroni, Dario Antiseri, Diego Marconi and Massimo Egidi for having introduced me to the issues that I have developed in the past years.

I would like to mention with particular gratitude the study and research undertaken in Oxford with William Newton Smith and in Milan and Houston with Dan Osherson.

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Many of the issues examined in this volume have been discussed at seminars or developed in research organised by the Rosselli Foundation in Turin from 1988 to 2008. For this reason, I would like to thank all those people who have made these important meetings and research possible, first and foremost Claudio Roveda, Francesca Traclò, Daniela Italia, Anna Mereu and Laura Gilardi.

Interesting ideas on the themes of realism, bounded rationality, creativity and duality of mind emerged from the seminars held at the Italian Cultural Institute of New York between 2010 and 2013, above all from the initiatives organised with the Herbert Simon Society. In particular, I would like to acknowledge the special contribution given at meetings and discussions by, among others, Daniel Kahneman, Joseph Stiglitz, Gerd Gigerenzer, Edward Feigenbaum, Colin Camerer, Umberto Eco, Hilary Putnam, Maurizio Ferraris, Gianni Vattimo, Dan Sperber, Ned Block, Paul Boghossian, David Over, Ron Sun, Jonathan Schooler, Giovanni Dosi, Massimo Egidi, Laura Macchi and Mario De Caro.

A final thank you goes to Barbara Fess of Springer for her patience and for the interest she has always shown in the publication of my work.

And finally, this book would have been impossible without my wife's help, support and advice.

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Testimonials

This erudite book highlights how cognitive science can clarify philosophical questions—from causality to intuition and tacit knowledge and from the rationality of individuals to that of science. But Riccardo Viale does not stop here. His analysis extends to research policy, the innovative power of small companies, and the need for academy–industry interactions. The vision of this book inspires.

Gerd Gigerenzer, Director, Max Planck Institute for Human Development, Berlin. Author of *"Gut Feelings"*

A bold, ambitious, and original book.

Stephen Stich, Board of Governors Professor, Rutgers University

Riccardo Viale is one of the few pioneers who have been exploring the rich implications of the cognitive revolution for the social sciences. Combining the competencies of a philosopher, a cognitive scientist, and a social scientist, he has proposed a new integrative approach, "Methodological Cognitivism" that goes way beyond the rather stale debates in current philosophy of the social sciences. This second volume on Methodological Cognitivism well illustrates how richly thought through and how stimulating Viale's approach can be.

Dan Sperber, Institute Jean Nicod, Paris; International Cognition and Cultural Institute

Viale has produced an interdiscplinary treatise that combines his mastery of ideas from philosophy, cognitive science, and social science, integrating them into a framework greater than the sum of its parts. He rejects the extremes of descriptive vs. normative theories, empiricism vs. nativism, and rationality vs. irrationality, offering instead a balanced account of human behavior that reconciles the bounded character of cognition with our ability to solve complex problems in science and technology. The breadth and vision of his work follows in the great tradition of Herbert A. Simon.

Pat Langley, Carnegie Mellon University, Pittsburgh

This panoramic book takes us a long way beyond Popper's image of the scientist as a medieval mason carving his lonely but splendid contribution to a cathedral that towers over the town. The essays gathered in this volume contain wide-ranging insights on what scientific knowledge is and how it emerges of out of everyday psychological concepts such as causal understanding and deductive reasoning. But they also show how science is a social enterprise with its specific norms and values and analyze how individual contributions are communicated within the scientific community. Viale's view is doubly realist—it shows the modern social reality of science as a virtual town in its own right with its markets, councils, cliques, and networks, but never loses sight of the fact that scientific knowledge must be tested against the reality of nature, and that some scientific procedures and organizations are more likely to succeed than others. The book will be refreshing for modern scientists wishing to set their everyday practices in a wider context and informative for policy makers seeking to understand how to promote better science.

Denis Hilton, Professor of Social Psychology, Université de Toulouse II

Networks of university–industry–government relations evolve increasingly like ecosystems. The evolutionary model (re)combines market forces, government policies, and knowledge exchanges as interacting in shaping niches. The current transformation of market economies into knowledge-based economies is analyzed in this book from the perspective of the increasing role of knowledge exchanges. How do cognitive structures at the cultural level intervene in Triple-Helix relations? A wealth of examples is also provided.

Loet Leydesdorff, University of Amsterdam, Amsterdam School of Communications Research (ASCoR)

Contents

Part III Research Policy and Social Epistemology

Introduction 1

The book covers a wide spectrum of topics from experimental philosophy and cognitive scientific theory to social epistemology and research and innovation policy. In this sense it connects to Volume I, "Mind, Rationality, and Society". It is a further application of Methodological Cognitivism in areas such as scientific discovery, technology transfer and innovation policy. It analyses the impact of cognitive science on philosophical problems like causality and truth. The book is divided into four parts. The first is about the philosophy of causality; the second deals with the cognitive basis of scientific rationality; the third examines cognitive realism, social epistemology and science policy; and the fourth focuses on knowledge transfer and innovation policy. This Introduction will present some of the main topics of each chapter by referring to parts of the original texts.

Logic, rhetoric and intuition are the main conceptual tools in philosophical reasoning. Intuition often acts as a sort of empirical verification of the acceptability of a particular thesis. Rather like a sort of empirical test or an experimental control, to use an analogy with what happens in natural science. The basis for this method is that intuition is generalisable, or in other words, broadly speaking, it can be shared at a universal level. Moreover, intuition must have foundational validity, a primary capacity for justification that is greater than any other alternative information. It should be greater than the reference to data from the cultural and religious tradition, for example, or the recourse to the theses of classical authors. Likewise it should be able to withstand the hypotheses and empirical confirmations of scientific and technical knowledge.

Experimental philosophy appears to question intuition's alleged foundational and universal nature. Intuition is a psychological phenomenon linked to what is conventionally known, according to some authors (Stanovich 1999), but not to others (Gigerenzer 2007), as System 1 of mind. Contrary to System 2, which is rational and explicit, this system is implicit and highly context-dependent. It is permeable to the influences of emotional variables derived from the cultural and environmental context. Seen in this way, it would seem difficult to affirm the thesis of the universality of human intuition. The underlying hypothesis derived from the findings of cognitive science argues the contrary: namely that intuition is local and

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contingent, changing in relation not only to cultural context but also to individual psychological variables, like personality traits or emotional and affective contingencies. Experimental philosophy has explored the universality or otherwise of human intuition at an empirical level. In the first place it has debunked the myth of a form of universal intuition typical of the domain of philosophers. Like all experts philosophers present the same variability and context-dependency as ordinary people. Experimental philosophy uses the methods of cognitive and social science to understand the phenomenology of intuition: how we construct theories around concepts of external reality, how we construct conceptual categories around objects from the same reality, or how the mind elaborates the meaning we give to concepts.

Part I *"Experimental Philosophy and Causality"* tackles a problem that is often seen as straddling metaphysics, ontology and epistemology, namely causality. It is argued that by analysing causal reasoning from early infancy to adulthood it is possible to attempt to give an answer to the law of causality in nature and to causal laws and explanations.

Chapter 2 *"Causality: Epistemological and Cognitive Considerations"* has the goal (1) To describe what cognitive science may suggest to philosophy concerning the reality of Causal relations and (2) To highlight the convergence between epistemology and the psychology of causality concerning tentative models of causal attribution and their anomalies. Some of the main issues that arise in the philosophy of causality concern the following questions: Which are more basic, Causal relations or causal laws? Are both or neither related to the non-causal state of affairs? If the latter answer is negative, does the Causal relation derive immediately from experience or is a theoretical relation not directly observable? There are three main answers to these questions. (a) According to the Humean interpretation, causal laws are more basic than Causal relations since the latter are logically '*supervenient*'[1] on the former, together with the non-causal properties of, and relations between, events. (b) According to the *Theoretical Realism of Causality (TRC)* Causal relations are real, but we cannot experience them directly. Causal concepts are theoretical concepts so that Causal relations can only be characterized, indirectly, as those relations that satisfy some appropriate theory. (c) According to the *Empirical Realism of Causality (ERC)*, Causal relations are more basic than causal laws and do not depend on the non-causal state of affairs.

What is the contribution of cognitive science to these questions of the philosophy of causality?

Data from developmental studies and a certain universality in the characterisation of causal perception in cross-cultural studies seem to support the hypothesis that we are endowed with early-developed cognitive structures, which correspond to maturational properties of the mind–brain. These orient the subject's attention towards certain types of cues, but also constitute definite presumptions about the

[1] A set of properties *A* supervenes upon another set *B* in order to ensure that no two things can differ with respect to *A*-properties without also differing with respect to their *B*-properties. In slogan form, 'there cannot be an *A*-difference without a *B*-difference'.

existence of various ontological categories, as well as what can be expected from objects belonging to those different categories. The three main intuitive theories individuated by developmental psychology are the theory of physical objects, the theory of biology and that of psychology. These theories allow infants to individuate some theory-specific causal mechanisms that explain interactions among the entities in the domain. The child has intuition of what characterizes a living being from an artefact or an object. Between the ages of 2 and 5 the child assumes that external states of affairs may cause mental states and that there is a causal chain from perception to beliefs, and from intentions to actions. What are the implications of these data for the epistemology of causality? It appears that these studies provide greatest support for the second position of theoretical realism. In order to recognize a relation between objects as a Causal relation we appeal, automatically, to an implicit, innate theory that is domain specific.

This chapter analyzes also one of the most debated problem in the philosophy of causality that is how to individuate the cause of an effect. The three main approaches, based on different criteria of causal attribution, are deepened. (A) *Normality criterion*: A cause does not need to be statistically unusual, but it must be abnormal in the sense that is not 'a default value among the elements that the event [to be explained] has evoked. (B) *Conversational criterion*: a cause is always a condition assumed to be unknown to the hypothetical inquirer—e.g. the short circuit in the house fire—and an enabling condition is typically a condition assumed to be already known to the inquirer—e.g. the presence of oxygen during the house fire. Both normality and conversational criteria have the most serious problem in separating enabling conditions from non-causal ones. (C) *Probabilistic contrast model*: the identification of a cause depends on its covariation with effects on a focal set—the set of events implied by the context.

Chapter 3 *"Cognitive Reasons for Causal Realism"* deals with the philosophical analysis of causal realism. Like many other epistemological and metaphysical questions, it relies heavily, and more or less explicitly, on the concept of mind and of mental activity. In this chapter I try to support a version of causal realism which, starting with the empirical possibility of the perception of Causal relations without previous experience, asserts the reality of singular Causal relations and their non-dependence or supervenience upon causal laws and non-causal states of affairs. In particular, I try to show (1) The new empirical results that cognitive science has brought to the discussion on causality and, in particular, on causal realism; (2) How this new knowledge seems to neutralise some of the arguments supporting the non-reality of Causal relations; (3) Which justification might support the reality of Causal relations, on the basis of new understanding of causal cognition.

Hume's theory of mind fails to justify two kinds of mental activity related to causality: the perception of the singular Causal relation without any previous experience of a similar relation, and the a priori inference of the relationship between cause and effect. Research into causal cognition, on the other hand, has shown that there are perceptions of causality that are not affected by previous experiences, whether of the same specific kind of relations or of an analogous kind. Besides, there is a great amount of empirical data showing that there is, in many

cases, a strong cognitive tendency to infer a priori the effect by observing a cause, without any dependence on previous regular experience.

Another example of the crucial role of the psychology of causality in philosophy is evident in the work of John Mackie, a leading contemporary philosopher of causality. His best known theory is how to separate causes from mere conditions (1974): his well-known Millian solution is that the cause should be an INUS, i.e. an Insufficient but Necessary part of an Unnecessary but Sufficient condition of the effect. His proposed "causal field", as background information against which to identify the relevant causes, develops the brilliant work done by Hart and Honoré on causation in law (1959), based on common sense psychology and anticipates the cognitive models of the causal attribution based on the normality criterion (Hilton and Slugoski 1986), the conversational model (Hilton 1990), and the contrast model (Cheng and Novick 1991).

What is the kind of cognitive structures that are responsible for our causal cognition? Are the same structures involved in the perception of causality as in a priori inference? Are they similar to the gestalt principles involved in visual perception, and in the Muller-Lyer illusion? Or are they intuitive theories that are present in the central cognitive system?

A first answer might be that the perception of non-experienced causality at a developmental age relies on an a priori inference based on innate background information and principles. Then the perception of causality and the a priori inference of causal connection become the same phenomenon: a priori inference is the condition 'sine qua non' for the perception of the causal connection. Faced with current data, the second answer might be that we do not know whether the core of innate beliefs or principles is small or large, or whether the beliefs are general or specific. In any case, the core of innate beliefs is the necessary condition to have the first inferential causal perception at a developmental age.

It might be true that, by showing the existence of a synthetic a priori inference of the connection between cause and effect and, therefore, of experience-free perceptions, we have undermined one of the most important pillars of causal antirealism. But how do we reply to a possible objection from an obstinate causal antirealist that, in fact, our data only prove that there is a mental activity that projects the causal attribution to a particular kind of relationship in the external world? Our data might only prove that causal perception and reasoning exist, not that reality is causal. I will attempt to give three possible answers to this problem.

(a) Naturalistic justification: According to this kind of justification, Causal relations are real because people think in this way. According to Harman (1977), there are no other ways of founding a justification, be it ethical or epistemological, other than *common sense and intuition*. The justification of causal realism is objective to the extent that most people interpret the Causal relation as something real, and not only as a projection of previously experienced regularity. Another approach is to set aside the common sense criterion and to try to discover the *cognitive mechanisms* that are responsible for the justificatory processes of our inferences—in this case, of causal attribution. In the '*naturalising epistemology*' programme, cognitive science might contribute to our models of mental machine

functioning, in the way that it processes information and produces a 'torrential output' of knowledge from the 'meagre input' of information from the world (Quine 1985). A weaker alternative position concerning the role of cognitive science is the work of philosophers like Goldman (1986, 1993). Cognitive science may be useful when addressing the question of the *feasibility* of our epistemological desiderata in relation to the constraints of the human mind. Cognitive science may be relevant in setting standards for epistemology that might fit cognitive feasibility.

(b) Perceptual Justification 1: According to this kind of justification, Causal relations are perceived in the same way as objects; hence causality is as real as they are. The claim that observables are real is a central topic in many fields, including scientific realism. But even on the antirealist side, there is acceptance of the real status of *observables*. One of the main problems concerning the reality of external objects is how to identify the individuality and unity of objects. For example, why do we identify as a unit the Tower of Pisa and not the conglomeration: Pisa-Tower-Japanese-armed-with-a-camera? Psychology has studied visual object-recognition and the principles that support object unity. Max Wertheimer, the founder of Gestalt psychology (1923), claimed that several unlearned factors used in perceptual grouping and organisation—proximity, similarity, good continuation and closure—allowed for object unity. Infants seem to use a number of other principles to determine object unity: cohesion, boundedness, rigidity and no action at a distance. But from these empirical findings, a traditional question arises naturally: is the correspondence between a real object and the perception of it correct, or is there some form of underdetermination of perception by reality? Cognitive science seems to opt for underdetermination and variability. (c) Perceptual Justification 2: The variability of the relationship between external referents and mental signs or representations supports Putnam's thesis of the impossibility of establishing the chimerical relation 'R' between objects of the real world and universals. This relation is empirically impossible, even when it is a question of sensorial experiences, as in the example of colour perception. Besides, it is a known fact that the mental representation of conceptual categories is a default process which is variable among individuals. We can identify two alternative views that try to overcome this kind of difficulty. Even if the concepts are variable at a subjective level, they are the product of, roughly, common inputs from the environment and of similar innate beliefs and principles that explain inter-subjective communication and coordination among individuals. It is the common communication and coordination among perceivers that allows us, by inference to the best explanation, to support the theory that there might be a common source (or cause) of our common coordination and communication: the reality of the perceived object. This view, which we might label '*default realism*', can be applied to every representation based on perception, and hence to the perception of singular Causal relations as well.

(d) Evolutionary Justification: According to this kind of justification, causal realism is justified by the evolution of the human mind as a result of selection. The argument is based on two considerations: first, we are endowed with innate principles and beliefs that allow causal perception as well as the perception of

object unity; secondly, as research by some animal psychologists has proved (Sperber et al. 1995), not only primates, as orang-utans and chimpanzees, but also brainless micro-organisms, are capable of causal behaviour. If the inherited mental endowment to perceive singular Causal relations is so widely present in the animal world, its role in the process of selection and evolution of many species is evident. From the beginning, in natural selection, reality has forcefully imposed a series of constraints on natural beings. But natural constraints have produced a progressive improvement in experiential and inferential capabilities and, in the case in point, in those dedicated to causal perception and inference.

Part II *"Cognitive Rationality of Science"* deals with the cognitive foundation of scientific rationality, starting from a strong critique of the neopositivist rationality of science on the one hand and of the relativist and social reduction of the methodology of science on the other.

Chapter 4 *"Deductive Rationality and Cognition"* starts from some fundamental questions which epistemology has sought to answer:

1. How ought we to arrive at our beliefs?
2. How do we arrive at our beliefs?
3. Are the processes by which we do arrive at our beliefs the ones by which we ought to arrive at our beliefs?

Traditionally, the answers to these questions were as follows: both epistemology and psychology should carry out their research independently and separately, and then, once they have answered questions 1 and 2 respectively, they will attempt to answer question 3.

However, there is another way to answer the three questions. This is the approach used by the project for the naturalizing epistemology: question 1 cannot be answered independently of question 2. The question of how we actually arrive at our beliefs is therefore relevant to the question of how we ought to arrive at our beliefs.

What prompted this reversal of approach? Largely it was the failure of the foundationalist project which tried to show that there is a class of beliefs—typically beliefs about our own sensory experience—about which it is impossible to be wrong. Moreover, these beliefs were held to be sufficient to justify the rest of our beliefs. Carnap's project was aimed at the translation, the rational reconstruction of every assertion about the world in terms of sensory data, logic and set theory.

Does this mean that the empirical foundation of knowledge, the empirical meaning of sentences about the world is no longer founded on solid bases? Quite the contrary. Our knowledge of the external world is based and founded precisely on the empirical meaning of language, as is actually attained in the process of individual learning of language.

A further shift towards a naturalisation of epistemology occurs at the moment when the meaning of the three questions is further examined by focusing attention on the cognitive mechanisms of rationality, the various internal processes of the cognitive elaboration of beliefs, on the processes whereby from one belief we reach a different belief, namely on the processes of deductive and inductive reasoning and inference. In the past it was believed that man was a rational animal because his

reasoning was thought to comply aprioristically with the precepts of classical logic. The answer to question 2 was therefore taken as being non-problematic and established a priori within the terms of a positive response to question 3.

The presence of rooted prejudices of a logicist type in the study of human inferential performances is well illustrated by a number of traditional theories on schizophrenic thinking. In many of the theories on abnormal and in particular schizophrenic thinking present in treatises of psychopathology and psychiatry, we find the thesis on conformity of normal human deductive reasoning set out along the lines of classical logic. According to some theories prevalent in psychiatry, the schizophrenic displays a clear deviation from classical canons of logical reasoning. This different logical behaviour was thought to be characteristic not only of the psychotic but also of cognitive behaviours in men who lived in archaic cultures, and it was therefore termed, by Arieti, for example, as "*paleologic*" (Arieti 1963). Another theory linked to the traditional model of rationality and normality of reason in the sense of conformity to the principles of classical logic is that of Matte Blanco (1981). He identifies a number of fundamental laws in schizophrenic thinking and uses them in an attempt to explain the symptoms found. These theories are conceptual edifices whose foundations rest on an a priori definition of the deductive performance of normal humans, nowadays empirically confuted by cognitive psychology. From experimental observations it can be demonstrated therefore that also normal subjects at times show forms of reasoning, traditionally seen as aberrant and considered peculiar to the cognitive symptomatology of schizophrenia. It is no longer convincing, therefore, to characterise schizophrenic reasoning based on the infraction of the laws of the classical logicist ideal of deductive rationality.

Given that classical logic has been used for centuries as the rational canon of deductive reasoning for normal humans, the first question that comes to mind is the following: is classical logic justified in its normative claim to act as the criterion for the effectiveness or otherwise of human deductive inferences? Is there a justified claim for an external criterion like this to decide on the rationality of deductive inferences in humans? To this question various responses can be advanced of a theoretical nature or deriving from the empirical results of cognitive science.

A. Firstly, one might ask why classical logic and not one of the many logics that have been developed in this century, like one of the modal logics that also have the advantage of formalising the concepts of possibility and necessity, or more exotic logics like the non-monotonic logics or "fuzzy" logic which have the advantage of trying to emulate the real characteristics of human reasoning, such as its ambiguity, scant definition and its ability to navigate effectively through a sea of contradictions, inconsistencies and imprecision, nonetheless achieving satisfactory results by solving problems and inferences.
B. No grounds for the validity of inferences exist that do not rest on the logical intuitions of those who have them and that is justified by some logical theory. Such a recourse to logic as a prescriptive model for the validation of human deductions is not possible because logic itself is ultimately justified by human intuition.

C. Even if we could accept the proposal of classical logic as a model for mental logic and the inferential character of human reasoning, we would not avoid another serious problem. Any set of premises implies an infinite number of different valid conclusions. Many will be wholly trivial, such as a simple conjunction of the premises. Only a few, if any, are likely to be drawn spontaneously in ordinary circumstances. Heuristic principles must exist that avoid this redundancy of conclusions, which filter the trivial and the useless. These heuristic principles must lie outside logic and they are incorporated in the inferential characteristics of the psychology of human reasoning.

Classical logic may be regarded as an inadequate criterion of the correctness of an inference because its logical relations do not correspond accurately to linguistic conventions, and because of its precise rather than fuzzy concepts of truth and falsity. Various empirical results from cognitive science show that no mental logic exists that accords with the rules of classical logic, but human deductive cognitive capacity has unique characteristics which cannot easily be matched to a priori models. Every theory of human deductive rationality ought therefore to refer to the real constraints/features (the cause of errors in logic tests) evident in everyday reasoning, such as, among others: the bottleneck limitation of short-term memory, which reduces computational ability and induces peculiar effects in reasoning (for example, the figural effect in syllogisms); the compartmentalization of long-term memory which facilitates the creation of inconsistencies and contradictions in reasoning (while consistency has always been a necessary minimal ideal requisite for a rational agent); the limited availability of time available in reasoning and the combinatorial explosion (of an exponential nature) of each problem to be resolved with formal deductive instruments which indicate the necessary presence of shortcuts and heuristic filter mechanisms, simplifying and accelerating reasoning. These characteristics are difficult to combine with any a priori model of deductive rationality of the human mind.

Chapter 5 *"Cognitive Theory of Scientific Rationality or of Scientific Reasoning?"* tries to answer the question if it is possible to propound a theory of scientific rationality that highlights the methodological specificities underlying the conceptual change of science and that sets it apart from man's other cognitive activities.

Does rationality exist in science? A series of answers has been given to this question which can be exemplified by the following situations. (a) There is no scientific rationality for the behaviour of a scientist who, in order to defend his scientific reputation, decides to "trick" his colleagues when faced with a series of experimental anomalies of the theory he has elaborated, by proposing a series of ad hoc hypotheses which he knows are unacceptable. The only rationality contained in this action is that instrumental to attaining his own pragmatic personal ends. (b) There is no scientific rationality in the decision made by members of the community of physicists in the early years of the nineteenth century to prefer Newton's corpuscular theory of light to Young's wave one. The prevalence of the corpuscular theory was based on Newton's fame, on the unfortunate way Young presented his theory and on Young's energetic refutation published in the "Edinburgh Review".

(c) There is no scientific rationality in the passage from Newtonian mechanics to relativistic mechanics. The two theories could not be compared because they were incommensurable, and consequently the passage was based on social and psychological factors.

The philosophy of science has gradually lost ground in this century: scientific rationality has become increasingly less inclusive of scientific reality (both historical and contemporary as exemplified by case b and c). And at the same time, while the situations cited in case (a) were originally the only ones certainly not forming part of the examples of scientific rationality, paradoxically case (a) becomes a paradigmatic example of rationality present in science. The rationality of science therefore becomes the rationality of the scientist committed to achieving his own diverse ends. The scientist is deemed to be rational if he can select the best action to attain the goal he has set himself, whatever this is. This therefore annuls any criteria of demarcation and selection between what is acceptable as scientific and what is not.

The few epistemological formulations that can now be defined as a cognitive theory of science have raised, in passing, the problem of defining a rationality of science. And when they have done so, as in the two following examples, they have remained trapped by the model of *instrumental rationality*, at best of a *procedural* type. What does it mean to construct a normative theory of discovery? According to Langley et al. (1987, p. 45) it means "identifying a set of criteria to judge the efficacy and efficiency of the processes used to discover scientific theories. Presumably the criteria can be derived from the goals of the scientific activity". For a scientist rationality means using the best methods he has available to achieve his aim, in the sense of choosing the least wasteful inferential means in terms of computational costs, but at the same time the most efficacious in terms of attaining the set aims (1987, p. 47). And another cognitive theoretician of science like Giere (1988) emphatically underlined the exclusively instrumental nature of scientific rationality, ruling out every other attribute not linked to the analysis of means and purposes and to the efficacious attainment of the aims.

Clearly Giere's thesis is absolutely unselective compared to the paradigmatic examples (a), (b) and (c). Its scientific rationality is reduced to the instrumental rationality used in economics. No attribute marks it out as cognitive rationality, let alone as cognitive rationality of science. Using this model it is not possible to distinguish the cheat's theses from Velinowski's visions (in *"Worlds in Collision"* 1950) and from those of Newton's theory.

What are the features of this type of rationality? It seems not cognitive because it is not based on inferential procedures that should allow an adequate solution of scientific problems. It is not scientific because it does not envisage any common criterion of choice and selection relating to the product of the research activity within the scientific community. In the model proposed by Langley, Simon et al. the ends of scientific activity are not specified and clearly not justified. Whatever the end, there are no barriers to entry. It would be a problem concerning the empirical analysis of the specific context of problem solving. The criteria used to judge whether the discovery process is efficacious are context dependent and ad hoc:

"If process X has to be efficacious in attaining aim Y, then it must have properties A, B and C" (1987, p. 45). Simon's model gives a normative response that obviously does not manage to steer clear of the traps presented by a theory of generic rationality to which we might add the attribute "cognitive", but which does not merit that of "scientific". This minimal rationality includes any "problem solving" activity, ranging from those of everyday life to that of the scientist, and cannot distinguish between cooking and laboratory activities. Ultimately, even with the model proposed by Langley, Simon et al., we remain in case (a) of rationality.

At this point it appears legitimate to ask ourselves: is a cognitive theory of scientific rationality possible? Is it possible to devise a theory of scientific rationality based solely on cognitive procedures and which does not fall into the trap of being a merely descriptive hypothesis of scientific reasoning?

The first conjectural response to this question appears to be the following. If we want to pursue the attempt to construct an hypothesis of the cognitive rationality of science, we must achieve this by posing the problem of identifying the peculiar cognitive features that specifically and exclusively characterise the production procedures of scientific knowledge. What this should identify is a normative theory of the psychological procedures—of observation, reasoning and decision—that exclusively characterise the activity of scientific discovery and justification. Let's see if this is possible. The most natural methodological candidate to undertake this project appears to be Nelson Goodman's *relective equilibrium* (1965). Its application to the construction of a cognitive theory of scientific rationality—as yet never accomplished—should start with its simplest formulation, that of *narrow* reflective equilibrium. This only seeks to match scientific inferences and inferential procedures. Reflective equilibrium is carried out by the scientists themselves, namely the people who in practice are the protagonists of inferential activity. What could be the results of this experiment?

Three types of inference—falsifying, probabilistic and causal—will be taken into consideration in view of their important role in scientific reasoning (as is clearly underlined by epistemological and sociological literature) to understand the expected results of the test. If the expected results of the test highlight the acceptance of uniform and shared norms between the various scientific communities, which differentiate scientific reasoning from that used everyday and are not proved patently fallacious, then it might be possible to hypothesise that scientific rationality has a cognitive foundation.

The principle of falsification regarded as being necessary to identify a cognitive rationality of science doesn't appear to be supported by the test results. There is a systematic tendency towards the fallacy of affirmation of the consequent which is manifest in different ways depending on the social and cognitive disciplinary contexts. Moreover, the same systematic tendency towards the confirmation bias, in which acceptance of the rule embodied in the fallacy of affirmation of the consequent can be glimpsed, is found in the reasoning used in everyday life.

A similar application of narrow reflective equilibrium between inferential norms and procedures could be attempted in relation to the procedure of elaborating new empirical data compared to working hypotheses. Here again, the results of