

Foreword

This work has a long history, so long that it well warrants being considered my life's work. In my book in the philosophy of physics in Italian, *Temi e problemi di filosofia della fisica*, which was published in 1969, I presented a theory of scientific objectivity which succeeded in attracting the attention of a number of scholars at that time, including, for example, Marian Przelecky, Ryszard Wóycicki and Marja Kokoszynska in Poland and Carl Hempel in the United States. That book, however, was born under an unlucky star, its publisher becoming insolvent soon after its publication. And, though the unsold copies were acquired by another publisher and put on the market as a second edition in 1974, this new house discontinued its philosophical collections after a short while. The result of all this was that the book appeared hardly at all in bookstores, even in Italy, and for many years has only been available in libraries or by direct order from the publisher via a rather complicated procedure.

On the positive side, however, the Spanish translation of the book, which came out in 1978, has had a broad circulation in Spain and Latin America thanks to the solid marketing policy of the Spanish publisher. And over the years, I have had the opportunity to present its central ideas at conferences and in lectures, and to increase their circulation, particularly among philosophers of science. The pleasant consequence of this was that I could note their wide acceptance and even see them inspiring other scholars. Less pleasant, however, was the fact that I seldom received credit for being the source of these ideas. This had nothing to do with a lack of professional honesty, but simply with the fact that no other work of mine existed (particularly none in English) to which reference could be made, except for a few scattered papers.

The only way to correct this situation seemed to me to be to write a book in English, which could enjoy the possibility of the broad readership offered by this language, especially in the philosophy of science. I discarded the idea of a translation of my original book because it contained several parts that were specifically relevant to physics while not being directly related to my original position regarding scientific objectivity, and furthermore contained only partial elaborations of more general issues that deserved greater attention. Therefore, I decided to prepare a new book, in which the view of scientific objectivity already proposed in *Temi e problemi* would be presented and further developed, along with much broader references and a discussion connected to past and present authors whose

work was relevant to the topic. The result is the present work, in which certain parts of *Temi e problemi* have been omitted, while, at the same time, chapters and sections have been included whose content was at most only hinted at in the earlier book.

I was able to begin realising my project when I had the opportunity to present and discuss my ideas in detail while teaching a seminar for graduate students at the University of Pittsburgh in 1977 and working as a research fellow at the Center for Philosophy of Science of that University. A similar opportunity presented itself in 1978, when I was visiting the University of Dusseldorf. It was there that I began to work out the general structure of the book and organise my numerous notes. The fulfilment of the project, however, would have required a year's concentrated effort, and the opportunity to exert such effort did not offer itself for a long while. On the contrary, my academic activities and my numerous international responsibilities increased between 1978 and 1993, leaving me only a couple of occasions for uninterrupted work (twice in Oxford during summer vacations, one term in Stanford in 1981 and one term again at the Center for Philosophy of Science in Pittsburgh in 1992).

This does not mean that, during the intervals between these fortunate opportunities, this work remained in a state of hibernation; indeed it has constantly been in the forefront of my mind, and several parts of it have been redacted, and even published from time to time as self-contained papers, which have been incorporated with a few adaptations as sections of this book. Their listing in the references of this work is, therefore, a documentation of its progressive construction over many years.

The long history of the writing of this book explains how I have come to be indebted to many people for inspiration and suggestions, though this may not always be clear from its content. For example, the fundamental orientation of the whole of my thought, due to my having been a disciple of Gustavo Bontadini, only occasionally surfaces in my constant reference to and criticism of 'epistemological dualism', and in my way of conceiving of the cognitive status of metaphysics (two fundamental aspects of his teaching); and the same may be said regarding the essential contribution made to the final shaping of my theory of objectivity by the reflections of Vittorio Mathieu on this topic. During my stay in Pittsburgh in 1977, I had the privilege of spending many hours in philosophical discussion with Wilfrid Sellars, sharing with him many points of view; however, only our major point of disagreement is what appears in the book, namely my not accepting his opposing of the 'manifest' and the 'scientific' images of the world. In a similar way, the conversations I had with Karl Popper are also reflected in this work mainly through criticisms I express concerning several of his doctrines.

In spite of all this, let me mention at least a few philosophers with whom I had especially fruitful exchanges of views: Larry Laudan and Nicholas Rescher during my first stay at Pittsburgh, again Rescher and Peter Machamer during my second stay, Patrick Suppes, Edward Zalta and John Etchemendy during my stay at Stanford, Alwin Diemer and Wolfram Högge during my stay in Düsseldorf, and Kurt Hübner and Hans-Georg Gadamer on several scattered occasions.

More substantial have been the suggestions I received from those who have accepted to read and discuss parts of this work during its elaboration. Jonathan Cohen, under whose supervision I had spent a year as a research postgraduate in Oxford in 1960, read my developing work on the occasion of several stays I spent at Oxford much later. Also Rescher had the kindness of doing the same in Pittsburgh in 1992. I also received valuable comments from Marco Buzzoni, a former student of mine who (having become a respected colleague in the meanwhile) has helped me in a thorough revision of a first draft of this work. I have received equally valuable comments and suggestions from Mario Alai regarding certain central parts of this book. The scholar who has most directly assisted me in the redaction of the book, however, is Craig Dilworth, whom I first met in 1977 on the occasion of a lecture I gave in Uppsala, where he was working on his doctoral dissertation (later published as the book *Scientific Progress* in 1981). We discovered a fundamental affinity between the 'perspectivist' view of scientific theories he was advocating and my own theory of scientific objectivity that was also perspectivist, and began a collaboration that has lasted right up to the present. His critical appraisal of my writings, including the present work, has been precious to me, as will be clear from my several references to his work, references that do not imply either a direct influence of his views on mine, nor the reverse, but rather a fruitful convergence of often different paths. The frequent references to his work, however, are also intended to compensate for the little attention that mainstream philosophers of science have paid to his very valuable production.

A few words now regarding certain features of this work. Its general spirit is in keeping with the *analytic* approach that has characterized philosophy of science during the twentieth century, and this is a natural consequence of the fact that my training in philosophy of science has been based on a detailed study of this tradition, from logical empiricism to the subsequent developments within the Anglo-American world. From the beginning, however, I did not share certain elements of this tradition, that is, its radical empiricism, syntacticism, linguistic exclusivism and lack of historical sensitivity. It is true that such features have been gradually overcome during the evolution of the said tradition, but the fact of having been free from them from the beginning has offered to my perspective, I believe, the advantage of anticipating several of such developments, and also of avoiding certain excessive reactions they contained. For example, the awareness of the limitations implied in the purely linguistic view of scientific theories has often led people to discard completely the so-called 'statement view' of theories and the nomological-deductive model of scientific explanation. According to my view, theories are not *just* systems of statements, but they are *also* this, because they are linguistic explications of the content of a particular *Gestalt* proposed for the understanding and explanation of a given domain of objects. Therefore, the sentential view and the nomological-deductive model can be preserved as a *partial* characterization of scientific theories, whose more adequate characterization needs the introduction of *hermeneutic* tools. Similarly, the appreciation of the dependence of the meaning of a concept on its linguistic context has come as a development of the linguistic approach to theories, and has prompted the ideas of

meaning variance, incommensurability, incomparability, and so on. I had arrived at a similar conclusion, instead, as a consequence of my studies on formal systems and axiomatic method, in which I had stressed a genuine *semantic function* of the axiomatic method (in addition to its commonly recognized syntactic function); this means that the axiomatic context contributes to the shaping of the *sense* of the concepts occurring in a theory (therefore, meaning variance is a real fact). However, not having remained prisoner of the 'linguistic turn,' I always maintained that sense cannot produce or ensure *reference*, for which an extralinguistic source must be provided, and this source consists in *operations* that are not reducible to the *observations* that radical empiricism requires, since they are essentially related to *praxis* and can be connected to sense thanks to its *intensional* nature. This position, in turn, has led me to vindicate a fundamental role for *truth* in science (something that had been almost banned from philosophy of science) and to study how truth can be attained, either by direct reference, or by argument, and this offers a foundation for admitting also the truth of non-observationally testable statements. Finally, the referential commitment of truth justifies a (carefully and duly specified) *realist* view of science. In my perspective, scientific objectivity is not context-dependent in a purely linguistic sense, but in a *historical* sense (of which the linguistic dependence is only a very particular aspect). The exploration of such a historical contextualization (that does not amount to *relativism*) opens the way to a due appreciation of all the right points stressed by the sociological interpretation of science, without falling into its excessive conclusions, and at the same time it justifies the consideration of those problems (for example, problems of an ethical and metaphysical nature) that cannot be treated in a consideration of science as a closed system of concepts and procedures. This approach has also provided a more comprehensive framework for the treatment of the relations between theories and models, and the strongly 'structuralist' conception of models, already explicitly presented in my book of 1969.

Owing to all this, several important works that were published during the long elaboration of my book did not appear all that new and original to me, since their basic views had already been anticipated in certain sections of this work or, sometimes, even published in papers of mine. Nevertheless, I am indebted to them for having pushed me to better formulations, or for further deepening certain views that I had conceived of independently. By the way, it was because of the publication of such works that I have been obliged to resume and revise from time to time my work in order to keep it up to date with the pertinent literature, a fact that has obviously slowed down its redaction. Yet this should not be understood as a pretension of 'completeness' and, in particular, it does not mean that I underestimate the importance of authors whom I do not mention. I do not 'ignore' them, but I simply had some particular reason for not mentioning them in the book. (In particular, in spite of having lectured for 19 years in French and German at the University of Fribourg, and of being well acquainted with twentieth century French and German philosophy of science, I preferred not to mention the relevant French and German authors, rather than make a few occasional references in

footnotes to works hardly accessible to the English-speaking readers to whom this work is particularly addressed.)

I want to conclude by mentioning the favourable conditions that have significantly helped the realisation of this work. Regarding the stimulating intellectual atmosphere and the availability of research facilities, I must stress the importance of my repeated stays in Oxford and the great opportunities offered me by my two stays at the Center for the Philosophy of Science of the University of Pittsburgh (where the concrete redaction of the greatest part of this book was completed). In order to terminate my work, however, I needed a long period of time to devote almost exclusively to this enterprise, and this has been granted me by a research appointment of the Accademia Nazionale dei Lincei. It is thanks to this that this longstanding life's work of mine could be 'practically' concluded. By saying 'practically', however, I mention an innate hypercritical attitude of mine that has imposed on me the obligation of a 'final global revision', via which repetitions and redundancies would be eliminated; but the leisure required for this revision has hardly occurred, so I finally decided to close this enterprise after a 'normal' careful control. Owing to such a long elaboration I must say that this book is like Theseus' ship that, after many repairs and replacements of its parts, was no longer the original one (and I gladly admit that several 'layers' can be found in the book's structure which are, however, systematically connected), but at the same time I am satisfied that it did not result, after all, in a Penelope's web.



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