

Enrico Bellone, *Il mondo di carta*

The Logic That Governs Each Step of Scientific Research

Luca Guzzardi

When it first appeared in 1976, Enrico Bellone's *Il mondo di carta* was another tile in the vast and multicolored mosaic of the growing interest of Italian philosophers and historians in the structure and development of the sciences.¹ In the preceding decade, a group of young scholars with a philosophical as well as a physical/mathematical background gathered around the leading figure of Ludovico Geymonat, professor of philosophy of science at the University of Milan. Beginning in the 1930s and 1940s, he had, on the one hand, introduced in the Italian context some trends of Austro-German neoempiricism—mostly polemically, against the idealistic tradition of Giovanni Gentile and Benedetto Croce and their tendency to place the natural sciences under the patronage of philosophy. On the other hand, Geymonat's education in philosophy and mathematics enabled him to investigate concrete historical cases (such as Galileo or the history of the calculus), thus combining epistemology with the history of physics and mathematics.

Prepared by Geymonat's approach, the new generation of the 1960s was ready to extract some trends in the “nonstandard” view of philosophy of science from the Anglo-American context. They began to discuss Karl Popper's methodological fallibilism, debated T. S. Kuhn's *Structure of Scientific Revolutions*, and became interested in the novel approaches of Imre Lakatos and Paul Feyerabend, questioning whether and how to integrate them in a post-Marxian

framework (dialectical materialism being another thread of Geymonat's interests). This enthusiasm resulted in the translation, around the mid-1970s, of some classic works, such as Kuhn's *Structure*, the conference volume *Criticism and the Growth of Knowledge*, and Feyerabend's *Against Method* (the latter two were edited by Giulio Giorello, Geymonat's successor as chair of philosophy of science in Milan). Homegrown reflections on these perspectives were offered as well—for example, in *History of Philosophical and Scientific Thought*, a collaborative work in several volumes, directed by Geymonat and authored by many of his former pupils (Giorello, Corrado Mangione, and Silvano Tagliagambe among them).^{2a}

This background surfaces in many of the pages of Bellone's *World on Paper* and allows today's reader to understand the book, to see its limits, and to appreciate its potential. First of all, it explains why, in a book subtitled "Studies on the Second Scientific Revolution," one does not find as much about history as might be expected; to be honest, one is tempted to say that the historical details are extremely limited. Bellone advances a fascinating thesis: that the conspicuous turn experienced by physics at the end of the nineteenth and the beginning of the twentieth century—namely, the replacement of the mechanical worldview by relativity theory and quantum mechanics—did not result from the bankruptcy of mechanism; rather, it was the effect of the dominance of mathematical theory over philosophical principles. The evidence quoted from the sources, however, is poor, as recognized in the generous review by Stephen Brush in *Isis*. Others were harsher: see, for example, Kenneth Caneva's comment in *Physics Today* to the effect that discussion of the literature is lacking and possible counterexamples are not even taken into account.³

^a Please check my revisions in this paragraph and the notes I have cobbled together in footnote 2. **Thank you! I agree with your changes and improvements! Also note 2 is ok!**

This is enough to suggest that the fundamental point (and the interest) of the work is not its announced historical scope. Rather, history was invoked only to provide a pretext for a broader discussion and a more ambitious aim, which emerge from the first epistemological chapter and the concluding pages.^b Here Bellone proposes the “scientist’s dictionary”—that is, the set of rules and concepts a scientist implicitly applies in translating her or his theoretical terms into real practice—as a tool for historical research. Dictionaries in Bellone’s sense embrace scientists’ assumptions and beliefs, philosophical and theoretical frameworks, mathematical and experimental methods, and so forth. They are aggregates of such diverse elements, and “the task of the historian of science is to analyze the various levels of the dictionary and to evaluate critically the correlations that form its inner framework” (p. 15).^c Both philosophical conceptions and physical theories are part of a particular dictionary, but “the construction of individual theories is relatively autonomous with respect to philosophical views of the world, since it is entrusted to locally stable areas of rules within the dictionary” (p. 24).

Dictionaries also guide the dynamic of scientific change:

A dictionary is an open process: it interacts with other dictionaries, it may at times absorb some of their relevant chapters, and it is subject to local readjustments in an effort to reach a situation of relative stability. In Maxwell’s case the dictionary is not only the dynamic matrix within which he builds a theory that describes the world rationally; it is also an element of the historical development of the quest for knowledge in the second half of the nineteenth century. If all this has no part in Popper’s logic or in the naive applications of classical causality, it does not mean that it can be reduced to irrationality.

^b I want to be sure that I am interpreting this correctly: You mean to say that this aim emerges in the first epistemological chapter and then resurfaces in the concluding pages; yes? **Correct! Please, add “resurfaces” if it sounds clearer.**

^c Please see my query at note 1 regarding the source of your quotations (and the page numbers). **My answer is in the remark at note 1.**

What it means, instead, is that the historical reconstruction of nonlinear processes and the study of ever more complex logics are wide-open, fertile fields. [P. 17]

Popper epitomizes the perfect enemy here. He **is** stands as a token of any philosophy according to which scientific practice is a theoretical construction of rational agents, who explicitly or implicitly apply *a priori* rationality criteria to build physical theories. The job of philosophers of science, therefore, is to unravel the rational structure lurking behind scientific practice; their task is only to provide a coherent and rich theory of rationality. Bellone would object, however, that dictionaries escape this rigid scheme, for rationality criteria are integral parts of a dictionary but also, in turn, depend on it. To investigate the logic of scientific research means to uncover the internal logic that locally governs the dictionaries and the way they interact.

However, another enemy awaits at the other extreme—one for whom science is the product “of intuitive flashes or personal faith, of a scientist’s psychological upheaval to be described in terms of Gestalt theory . . . , of an ideological invasion that muddles the precepts of scientific inquiry to pay homage to the cultural dominance of a particular social class” (p. 181). In the final lines of the book, Michael Polanyi personifies this enemy; but some notes (which appear only in the Italian edition and are not included in the English translation) suggest that Bellone really had Kuhn and Feyerabend in mind. Dictionaries are not paradigms: they are open to modifications and are typically unstable (see, e.g., pp. 20, 171). Therefore, there is no such thing as a paradigm shift, nor are there incommensurable terms; instead we find a substitution and hybridization of some elements of one dictionary with some elements of another.^d And it is certainly not the case that “anything goes.” Something “goes” only if it can be integrated within the various elements that form a given dictionary, taking for granted that some relations are

Commento [LG1]: Shouldn't be deleted?

^d Please check my revisions in this sentence; have I preserved your intended meaning? (Or not?) **Yes, thank you for the corrections and for improving the text fluency!**

tighter than others and that historians should specify the reasons and conditions in which “correlations ... vary in intensity” (p. 15).

Forty-some years later, this proposal may or may not satisfy today’s historians and philosophers, according to their own tastes and historiographical tendencies. In any case, the book entered the English-speaking context (1980)^e only after a delay of **approximately** a decade. **Bellone’s historiographic concerns were typical, say, for the early 1970s; but he completely overlooks novel, emerging tendencies such as** laboratory and instrumental practices, institutional aspects, scientists’ genealogies, the problem of how new disciplines and scientific fields emerge—in a nutshell, he missed the cutting edge of ongoing historical research. On the other hand, in the 1980s philosophers began to worry more and more about the philosophy of the special sciences (including information sciences), in closer collaboration with logicians; even philosophers of physics were increasingly more preoccupied with the foundations and developments of relativity and quantum theory than with general epistemological issues. **Of course, Bellone cannot be blamed for not having been able to anticipate tendencies which were emerging or would emerge only later. However, preoccupied as he was with his criticism against Popper, Kuhn, Lakatos, and Feyerabend, he could hardly have something to say about the novel approaches. He did not see how rich and changing was the world *after and beyond* Popper, Kuhn, Lakatos, and Feyerabend. Nor did those questions emerge in his later career as a historian of science in Genoa and then in Milan; in addition, his reluctance to share his ideas with the**

^e I changed your original “(1980)” to 1990, in keeping with the citation of Brush’s review; that is correct, yes? **No, the English translation appeared as early as 1980. I am not aware of another English edition in 1990; in any case, the first English edition is definitely 1980, so it is correct that the book “entered the English-speaking context (1980) only after a delay of approximately a decade.”** (If it would be entered in 1990, then the delay would amount to two decades at least). Note that Brush’s review traces back to 1981: Vol. 72, pp. 284–286.

growing community of historians of physics in Italy meant that his views were not refreshed by their input.

In hindsight, Bellone's notion of the scientist's dictionary could have been a good starting point for renewal. Of course, its emphasis on the theoretical-linguistic-reflexive aspect and the idea that a dictionary is a function of the individual scientist appears largely superseded. It is, however, a first approximation of the notion that scientific practice is (also) a network of irreducible epistemic constraints: philosophically, the scientist's dictionary responds to the need for emerging rationality criteria within individual scientific discourses. In Bellone's own words, it embodies "the logic that governs each step of scientific research" (p. 86). And investigating this kind of conditional, limited rationality could, I think, stimulate today's historians and philosophers to work together.

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¹ Enrico Bellone, *Il mondo di carta* (Milan: Mondadori, 1976). For the English translation see Bellone, *A World on Paper: Studies on the Second Scientific Revolution*, trans. Mirella Giacconi and Riccardo Giacconi (Cambridge, Mass.: MIT Press, 1990).^f

² Thomas S. Kuhn, *La struttura delle rivoluzioni scientifiche* (Turin: Einaudi, 1969); Giulio Giorello, ed., *Critica e crescita della conoscenza* (Milan: Feltrinelli, 1974); Paul Feyerabend, *Contro il metodo*, ed. Giorello (Milan: Feltrinelli, 1975); and *History of Philosophical and Scientific Thought* [in Italian] (Milan: Garzanti, 1972-).^g

³ Stephen G. Brush, rev. of Enrico Bellone, *A World on Paper: Studies on the Second Scientific Revolution*, *Isis*, 1981, 72:284–286; and Kenneth Caneva, rev. of Enrico Bellone, *A World on Paper: Studies on the Second Scientific Revolution*, *Physics Today*, 1984, 37:89–90.

Commento [LG2]: It should be 1980

^f Please check my citation for the English translation; I hope it is correct, since I picked it up from Brush's review in *Isis*. Also, we need to clarify whether the quotations that appear in the text are your own or whether you've taken them from the published translation. And where do the page citations come from – the Italian or the English? (I'll add all that to this note once I know what to say.) **I always used the edition Cambridge, Mass.: MIT Press, 1980 also for quotations. The text was available on Archive.org, I am sending it in attachment).** I hope the pagination has not changed throughout the years. The year of publication is definitely 1980: <http://www.journals.uchicago.edu/doi/abs/10.1086/352726>.

^g Please check these notes, which I have pulled together from the text and online sources. **Checked! Ok!**

