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REVIEW ESSAY

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A critique of the history of economic ideas

Measuring utility: From the marginal revolution to behavioral economics, by Ivan Moscati, Oxford Studies in History of Economics, New York, Oxford University Press, 2018, 326 pp., £22.99 (paperback), ISNB: 978-0-19-937276-8 (hbk.); 978-0-19-937277-5 (pbk.)

> 'Philosophy of science without the history of science is empty; history of science without philosophy is blind' (Lakatos, 1970, p. 91).

Ivan Moscati has written a rich and detailed history of the measurement of utility in economics. It covers almost all, if not all, the economic literature from the 1870s to the 1980s related to the measurement of utility. In this sense, I expect it will become a standard reference for this topic.

But what kind of historical account is it and for whom is it written? The answers to these questions are implicitly presented in the 'goals' of his book: 'The first goal is historical in nature and is met by reconstructing in detail economists' ideas and discussion about utility measurement and investigating how these ideas and discussions influenced the development of utility theory' (p. 2). The three other goals are less historical, and more epistemological: The second goal is 'the interplay between utility analysis and the understanding of measurement' (p. 3), the third the relationships between utility measurement, psychological measurement and measurement theory, and the fourth 'the epistemological dimension of utility measurement' (p. 4). The account is a 'reconstruction,' and the four goals make clear that Moscati is addressing historians as well as philosophers.

The question, then, is whether a reconstruction is an appropriate way to achieve these goals. To investigate this question, I start with a strong advocate of reconstruction as a historical approach, namely the historian and philosopher of science Imre Lakatos.

In his 'History of science and its rational reconstructions' (1970), he proposes

> a 'historical' method for the evaluation of rival methodologies. The arguments were primarily addressed to the philosopher of science and aimed at showing how he can – and should – learn from the history of science. But the same arguments also imply that the historian of science must, in turn, pay serious attention to the philosophy of science and decide upon which methodology he will base his internal history. I hope to have offered some strong arguments for the following theses. First, each methodology of science determines a characteristic (and sharp) demarcation between (primary) internal history and (secondary) external history and, secondly, both historians and philosophers of science must make the best of the critical interplay between internal and external factors. (Lakatos, 1970, p. 122)

This long quotation emphasizes two aspects a reconstruction should take account of: 'pay serious attention to the philosophy of science' and 'make the best of the critical interplay between internal and external factors.' To these two valuable recommendations I would like to add an important nuance that Lakatos made: 'I disagree [with] philosophers of science who have taken it for granted that general scientific standards are immutable' (p. 121), that is to say, methodology has also a history, and therefore one has to consider what the employed 'normative methodology' is in a certain time period for the reconstruction of a scientific practice in that period.

Lakatos, however, wrote not so much about how the interplay between internal and external factors should be taken into account. Therefore, we better consult Leo Corry's (1989) article in which a distinction is made between the 'body of knowledge' and the 'image of knowledge.' The body of knowledge includes all those contents related to the subject matter of a given discipline; these are theories, facts, methods, and problems. The image of knowledge include all claims about knowledge itself: they serve as guiding principles and pose and resolve questions that arise

from the body of knowledge, but are not part of and cannot be settled within the body of knowledge itself, hence are called 'second-order questions.' These second-order questions, for example, include the following: Which of the problems of the discipline most urgently demand attention? How should we decide between competing theories? What is considered a relevant experiment? What procedures, individuals, or institutions have the authority to adjudicate disagreements with the discipline? What is taken as a legitimate methodology? Corry's motivation for this distinction is that the study of the interaction between these two sets of questions 'might provide a coherent explanation of the effect of sociohistorical factors on the realm of pure ideas' (p. 412). It is particularly the interplay between internal and external factors from which the scientific norms and standards of a practice emerge. Therefore, a history cannot be restricted to an internal account, because such a history cannot clarify where these standards come from and how they structure the internal history.

> Measurement is the link between mathematics and science. (Ellis, 1968, p. 1)

Lakatos's recommendations and Corry's distinction are particularly relevant when it comes to history of measurement. Well-known, some famous, statements that include the message that 'science is measurement,' suggest that measurement is precisely that field of science where scientific norms and standards in fact are assumed to be met, or at least play a major role.

Measurement is the attempt to acquire quantitative knowledge about a phenomenon. This quantitative knowledge is usually expressed in numbers. To ensure that these numbers provide meaningful and reliable information about a phenomenon, specific rules have to be followed. Any history of measurement therefore must be a history of these rules.

The rules of reliability are related to scientific standards or norms, such as rigor. They most often came from mathematics or physics. So a history of measurement overlaps with the history of mathematics and the history of physics.

But also the rules for meaningfulness do not come from economics alone. The current dominant theory of measurement is the representational theory of measurement or model theory of measurement.[1] The core of this theory is that measurement is a process of assigning numbers to attributes or characteristics of an empirical phenomenon in such a way that the relevant qualitative empirical relations among these attributes or characteristics are reflected in the numbers themselves as well as in important properties of a numerical relational structure. This numerical relational structure is a mathematical representation – a model – of the empirical relational structure of the phenomenon. As a result, the rules that enable meaningfulness are rules related to the field in which the phenomenon is studied. In the case of utility that can be economics, psychology, sociology and even ethics. The representational theory of measurement implies that rules of meaningfulness are also related to the field of mathematics.

In other words, the rules of reliability and meaningfulness come from an interplay of a broad range of disciplines. Therefore, a history of the measurement of utility must involve at least the interaction of the history of mathematics, the history of physics, the history of economics, and the history of psychology.

Although Moscati shows that utility measurement has roots in physics, psychology, mathematics, and areas of economics (p. 6), he does not explain why measurement in economics has these roots nor how they may be related to the norms and standards of science. This is misleading, because the consequence is that the history is presented as a 'rational' development, and the interplay with these other disciplines seems just to happen on the right time, when it is rationally needed.

To answer these second order-questions, one has to take the relevant 'image of science' into account. To demonstrate this, I will discuss an example, namely the role of axiomatization in the history of utility measurement. I choose axiomatization in particular because it has no other role in measurement than that of being a norm of science.

The first time axiomatization plays a role in the measurement of utility is when Frisch introduces it in his first publication 'On a problem in pure economics,' in 1926. As Moscati rightly notes, this is also

the publication in which Frisch coined the term econometrics. But Moscati does not discuss how econometrics may be linked to axiomatization:

> Apparently, Frisch's is the first axiomatic treatment of utility. However, since Frisch's axiomatization had little impact on the subsequent literature and because my main concern here is the econometric part of his article, I do not here enter into Frisch's axioms on utility. (p. 119, n. 3)

Although Moscati (p. 118) cites Frisch's description of econometrics and its aim: 'to turn pure economics, as far as possible, into a science in the strict sense of the word,' no link with the aim of Frisch's axiomatization is made. Axiomatization did not appear again in Frisch's 1932 book *New Methods of Measuring Marginal Utility*, which can be considered as an extended follow-up of this article. The why of this disappearance is also not discussed by Moscati. A possible explanation may be related to the fact that in the same period when Frisch was working on this book, he, with Irving Fisher, was very much involved in the foundation of the Econometric Society. One of the aims of this new association was to promote in economics 'constructive and rigorous thinking similar to that which has come to dominate in the natural sciences' (Frisch, 1933). Therefore, a possible explanation for this changing role of axiomatization may be that Frisch has changed his idea of rigor, from a mathematics' (Hilbertian) standard of axiomatization to a physics' standard of empirical verification. I must admit that I have no evidence for this suggestion, nor an explanation why Frisch has changed the model for science from mathematics to physics, but whatever the causes of these changes were, the changes of standards are relevant for the history of the measurement of utility and should therefore be taken into account.

The axiomatic approach was introduced into economics by the work of the mathematician John von Neumann and the economist Oskar Morgenstern in their *Theory of Games and Economic Behavior* in 1944. This axiomatic approach, and the other mathematical concepts and techniques this work contained, actually only had an influence on the mathematization and the theoretical development of both economics and psychology, and not on the measurement practices in economics. Even von Neumann (1947, p. 196) made rather clear that too much focus on axiomatization could lead to 'de-empiricization.'

Similar to the history of econometrics, the history of utility reflects the history of economic methodology.[2] In the time of what came to be called the marginal revolution, 1870–1910 (part one of the book), science was associated with mathematics. In the period 1900–1945 (part two), science was measurement. The next period, 1945–1955 (part three) the science image was dominated by John von Neumann's ideas and work, in particular on axiomatization. And the last period (discussed in the book in part four), 1950–1985, is when the experimental methodology gained in dominance, first in psychology and towards the end of this period also in economics. This is of course a very crude historical sketch, almost a caricature, but it shows how the history of utility measurement cannot be disentangled from the history of the image of science.

Moscati's history of utility is not a history of measuring utility. Although this history indeed starts with measurement, it is an internal history of one of the core concepts of modern economics. A history of measurement involves a history of the image of science. The rules of science changed across the twentieth century in which the development of the utility concept took place. The reasons for the change of measurement practices cannot be explained 'internally,' or 'rationally,' but have to do with changing standards of science: from empirical standards of rigor, such as measurement and testing, to mathematical standards of rigor, such as consistency. But I should not end without emphasizing that as an internal history of utility, Moscati's history is rich, detailed and rather complete.

Notes

1. Moscati uses the term representational theory of measurement in a more restricted way. He uses it only for 'the approach elaborated by [Patrick] Suppes and his coauthors' (p. 248).
2. See Boumans and Dupont-Kieffer (2011) for the relationship between the history of economic methodology and the history of econometrics.

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