



## Bringing Them Alive

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# Bringing Them Alive

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André Lapidus\*

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## 1. Introduction

It might seem impertinent to begin a presidential address to a society of historians of economic thought by raising the question of why we – as historians of economic thought – exist, and whether our existence is in any sense justified.<sup>1</sup> And this sense of impertinence betrays the fact that neither our core intellectual goals, nor the efforts we make to satisfy a certain social demand, are as able as we would have liked to justify the kind of work we are doing. This is rather uncomfortable; and might even at times have led some of us to cast envious glances towards our colleagues specialized in, say, fractional calculus or biochemistry, not to mention banking and finance.

This is an enduring discomfort, which can be traced back to the birth of economics as an autonomous discipline. And Jean-Baptiste Say proposed a sweeping remedy in the “Abridged history of the progresses of political economy” which concludes his *Cours complet d’économie politique pratique*: let us get rid of our history, he enjoined, as other disciplines are wont to do once they are sufficiently mature (Say 1828, vol. 2, pp. 540-41). Yet it seems that the remedy, though still advocated today by a number of doctors, has been incompletely adhered to: thus the same discomfort remains, the history of economic thought still exists, and self-searching questions are still raised about its future, often focusing on *how* we do the history of economic thought, as if this could provide the answer to the question of *why* we do it (see the contributions gathered in Weintraub (ed.) 2002). Most of the previously published

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<sup>1</sup> This paper develops a general argument presented in the Presidential Address to the 2018 ESHET Conference in Madrid, borrowed in particular from Lapidus (1996; 2016).

Presidential Addresses to the ESHET conferences (that is, from the 2006 edition onward), for instance, assigned a prominent place to the justification of the history of economic thought through an investigation of the ways in which we practice it (Heinz Kurz 2007, Maria-Cristina Marcuzzo 2008, Annalisa Rosselli 2013, Hans-Michael Trautwein 2017); and for the others (Harald Hagemann 2011, José Luis Cardoso 2015) it was at least implicit in their discussions of tradition and of its possible transmission.

Despite having persisted since the beginning of the 19<sup>th</sup> century, about three decades ago the need for a justification took on a new character as a result of Mark Blaug's pioneering introduction of a typology of approaches to the history of economic thought (Blaug 1990). His typology spread throughout our community, quickly becoming *canonical* – the kind of thing with which you agree or disagree, but which you can hardly bypass. And its influence was all the more pervasive in that the typology gave birth to conflicting positions that firmly distinguished two opposing ways of doing the history of economics, typically referred to as Historical Reconstruction and Rational Reconstruction, to which were added two subsidiary approaches, *Geistesgeschichte* and Doxography (section 2).

This canonical typology was presented as borrowed from an important paper by Richard Rorty (1984), whose concern was the historiography of philosophy. But it was a special interpretation, resting on Blaug's own reading of Rorty, which set aside the possibility of a fifth genre introduced by Rorty, Intellectual History. Yet taking into account the possibility of this fifth genre opens the path to a non-canonical typology, blurring the border between Historical Reconstruction and Rational Reconstruction and lessening the divide between them, while also promising to challenge contemporary knowledge and enrich it with statements which do not belong to it (section 3).

The idea that the history of economic thought also has something to do with present-day changes in economic theory obviously preceded the birth of Blaug's canonical typology. It could easily be traced back to Schumpeter (1954) and, since the mid-twentieth century, has been a recurrent preoccupation among historians of economics, as typified by several presidential addresses to the ESHET. As such, that idea has shaped the long story of the way we have done history of economic thought in recent decades, leaving room for the progressive systematization of non-canonical typologies of approaches (section 4).

Drawing on this evolution, I have proposed a typology comprising three alternative approaches, distinguished on the basis of the way they conceive of the link between statements,<sup>2</sup> old and contemporary (Lapidus 1996; 2016): these approaches are *extensive*, *retrospective*, and *intensive*. The extensive approach, quite general in its scope, refers to any account of old statements in terms of other old statements, whatever their nature. The retrospective approach accounts for old statements in terms of the present-day economic statements which they prefigure. Finally, the intensive approach addresses old statements insofar as they can produce new statements, thus renewing our present knowledge (section 5).

Unlike the extensive and retrospective approaches, however, the very possibility of an intensive approach seems at odds with its relatively thin outcomes – witness, for instance, the paucity of handbooks in the history of economic thought which provide a firm and long-lasting expression of their authors’ methodological perspective. Taking the works of Sraffa (1951; 1960) and Sen (2002) as examples, I will argue that this is a consequence of the intensive approach being heuristic, and hence its results being by nature transient, such that it is destined to remain poised between the extensive and retrospective approaches. As a result, among the three approaches available to historians of economic thought – the extensive, retrospective, and intensive approaches – it is the latter which appears as a privileged route by which the history of economic thought can begin to engage with economic theory (section 6).

## **2. The canonical typology**

The story of the canonical typology of approaches in the history of economics begins in 1962, when Blaug published the first edition of his well-known *Economic Theory in Retrospect*. In the introduction, he drew a sharp distinction between two approaches: relativist and absolutist (Blaug 1962, pp. 1-2). This today is quite familiar to us: according to the relativist approach, each theory is to be considered – and justified – in its own context. The absolutist approach, on the contrary, allows a ranking of earlier theories according to the present state of knowledge.

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<sup>2</sup> I employ the underspecific word “statement” precisely for its wide meaning, intending thereby to encompass judgements, propositions, beliefs, and so on.

Blaug's position at this time is also well known: while not dismissing the relativist approach, he clearly expressed sympathy with the absolutist viewpoint. And the later editions of *Economic Theory in Retrospect* reiterated the same position, at least until his influential 1990 paper on the historiography of economics. This paper had the notable aim of introducing the community of historians of economic thought to a typology designed by a major philosopher, Richard Rorty, who had posited a distinction between four "genres" in the history of philosophy (Rorty 1984).

The result of Blaug's reading of Rorty's paper was a kind of compounding of two typologies. The first was Blaug's old typology, in which he contrasted the absolutist and the relativist views. The second was his own interpretation of the four genres identified by Rorty: *Geistesgeschichte*, Historical Reconstruction, Rational Reconstruction, and Doxography. This gave birth to a refinement of the initial relativist-absolutist typology which we today acknowledge as a canonical, that is, as a familiar gateway for taking up methodological issues in the history of economic thought.

This refinement, relying on Rorty's four genres, was produced as follows. Firstly, Blaug showed rather little interest in *Geistesgeschichte* [history of the spirit], which aims at showing how such-and-such a question became important in the work of a past author: typically, it might concern whether the Napoleonic wars bear any responsibility for Ricardo's concern with a decrease of the profit rate. He then considered Historical Reconstruction as equivalent to his own conception of relativism, which he split between moderate and strict relativism, the latter being dismissed on the grounds that it is untenable. As regards absolutism, meanwhile, he asserted the equivalence between, on the one hand, Rational Reconstruction and Doxography and, on the other hand, strict and degenerate absolutism, before also dismissing Doxography, which he regarded as an ill-founded approach in the history of economic thought (Blaug 1990, pp. 27-28). What remains are the two well-known approaches Historical Reconstruction and Rational Reconstruction, which strive towards different kinds of rephrasing of what a past author said: the first one rephrases, or accounts for, past authors' answers in their own terms, or in the terms of their time; the second rephrases, or accounts for, past authors' answers in our own terms. Blaug at the time unequivocally favored Rational Reconstruction at the expense of Historical Reconstruction, thus upholding his preference for absolutist rather than relativist approaches.

Blaug (1962)	Blaug (1990)		Blaug (1997)
	Blaug's Rorty (1984)	Blaug's Blaug (1962)	
Geistesgeschichte			
Relativism	Historical	Strict Relativism	Historical
	Reconstruction	Moderate Relativism	Reconstruction
Absolutism	Rational	Strict Absolutism	Rational
	Reconstruction	Degenerate Absolutism	Reconstruction
	Doxography		

**Table 1.** The canonical typology

Subsequent to Blaug's 1990 paper, what I have called the "canonical typology" became influential (see Table 1). First of all, obviously, it was influential on Blaug himself: he supplemented his introduction to the 5<sup>th</sup> edition of *Economic Theory in Retrospect* by "sharpening" his position, as he put it, through adding the distinction between Historical Reconstruction and Rational Reconstruction, which he explicitly viewed as "almost the same" as his prior distinction between relativism and absolutism (Blaug 1997, p. 7). *Geistesgeschichte* and Doxography were no longer discussed in the subsequent reeditions. But the 1990 paper also exerted a palpable and long-lasting influence upon historians of economic thought, who turned to it each time questions arose about the legitimacy of their methodology (see, for instance, in the years which followed its publication, Samuelson, Patinkin and Blaug 1991, Backhouse 1992, Dockès and Servet 1992, Khalil 1995, Lapidus 1996).

As we know, of course, this is not the end of the story: for, some years later, Blaug changed his mind, his preferences shifting from Rational to Historical Reconstruction (Blaug 2001). His 2001 paper, in turn, fueled the ongoing and at times schismatic debate on the methodology of the history of economic thought (in the special issue of the *Erasmus Journal for Philosophy and Economics* devoted to Mark Blaug in 2013, see for instance the papers by John Davis and by Harro Maas). But although Blaug did change his mind about the kind of approach he favored, he nonetheless remained faithful to the canonical typology inherited from Rorty. Yet what such persistent faithfulness concealed was that in passing from Rorty's hands to Blaug's, and thus transiting from the history of philosophy to the history of economics, the initial typology had evolved. Rorty's original typology had faded into Blaug's:

yet, as had already been shown (see Shionoya 2009), Rorty's original paper was also open to alternative interpretations, and was not committed to the relativist–absolutist interpretation, as Mark Blaug's interpretation was.

### 3. Back to Rorty

For instance, when Rorty discussed the issue of Historical Reconstruction versus Rational Reconstruction, he didn't view it in terms of the relativist–absolutist opposition. Rather, he borrowed it from a paper by Quentin Skinner published in 1969, and he followed Skinner in defining Historical Reconstruction as an approach in which no one

“can eventually be said to have meant or done something which he could never be brought to accept as a correct description of what he had meant or done” (Skinner, 1969, p. 28).

In contrast, Rational Reconstruction is an approach in which we are not searching for what an author *meant*, but rather for what *is meant* by him or her. That is, we are seeking the outcome of a forced conversation, in alternative contexts, between the past author and us today. Rorty proposed to adopt a restricted use of “meaning”, so as to reserve the words “meaning” and “significance” for Historical Reconstruction and Rational Reconstruction respectively (Rorty 1984, pp. 54-55).

The opposition between Historical Reconstruction and Rational Reconstruction, understood as an opposition between what an author meant (the meaning) and what is meant by his or her works (the significance), seems rather powerful. For instance, it clearly leads us to conclude that asking what Adam Smith would have said about digital economics is not at all nonsensical: we would only say that this is something that falls under Rational Reconstruction, not Historical Reconstruction.

Here I would like to draw the reader's attention to another aspect of the Skinnerian origins of Rorty's distinction between Historical Reconstruction and Rational Reconstruction: in arguing that it rests on the possibility of bringing past authors to accept an interpretation of what they said, we blur the border between the two types of reconstruction. Just think, for instance, of a simple statement which might pertain to Christopher Columbus:<sup>3</sup> “he discovered America”.

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<sup>3</sup> The example continues Rorty's discussion. See Rorty 1984, p. 53.

At first sight it's a Rational Reconstruction, since nothing like the American continent was part of Columbus's knowledge. But assuming we adopt Skinner's criterion, do we really believe that "he could never be brought to accept it as a correct description of what he had meant or done" – that is, that he could not accept the kind of information provided to him? On the one hand, this can be viewed as introducing an alternative context, and hence justifying such a claim would be regarded as Rational Reconstruction; but, on the other hand, how could we be so presumptuous as to imagine that we could in no way bring Christopher Columbus to accept this enlarged, rather than new, context? In which case, "Christopher Columbus discovered America" would be a Historical, not a Rational, Reconstruction.

The existence of a fuzzy, and thus debatable, border between Historical Reconstruction and Rational Reconstruction led Rorty to view his three relevant genres (*Geistesgeschichte*, Historical Reconstruction, Rational Reconstruction) not as independent approaches which might be mixed contingently in the work of such-and-such historians of ideas, but rather as an example of what he regarded as a "Hegelian triad" (Rorty 1984, p. 68), whose working opens the path to a fourth genre – or, better, a fifth, if Doxography is taken into account, despite its exclusion by Rorty, much like Blaug would also do. In the canonical typology this fifth genre is set aside: yet the entire final section of Rorty's paper is devoted to it. He called it "Intellectual History".

Intellectual History is presented by Rorty as raising issues which do not belong to our present knowledge. This is obviously the case with marginal or forgotten figures: taking them into account might result in a revision in the headings of the chapters of canonical histories of economic thought (in the history of philosophy, for instance, he mentions the relatively minor place of Diderot; Rorty 1984, p. 69). But it is also a way of saying that what an author meant, in the Skinnerian sense, some centuries ago, might also become meaningful for us today.

Intellectual History is obviously a way to challenge the fuzzy border between Historical Reconstruction and Rational Reconstruction. It yields an enlargement of the interpretations of a past author which – again in Skinner's words – he "could be brought to accept [...] as a correct description of what he had meant or done". Take as an example one of the main messages which Morishima found in Marx. What he called the "fundamental Marxian theorem" was published in its basic version in his book on Marx in 1973 and in its generalized version one year later. Broadly speaking, this theorem states (in its 1974 version) that in a quite general linear model of production, with alternative techniques and joint production, a



positive rate of surplus-value is a necessary and sufficient condition for a positive rate of profit (Morishima 1974, pp. 621-22). One might discuss at length the question of how we can know whether this kind of relation between the rate of surplus-value and the rate of profit was something which Marx actually meant. Let us assume it was. In 1973, however, despite certain pioneering contributions like that of Okishio (1963), this didn't really belong to the economic knowledge of the time when Morishima published his book. Thus:

1. You could argue that the so-called fundamental Marxian theorem belonged to Intellectual History, being an issue which didn't belong to our present knowledge as economists.
2. Nonetheless, you could also say that it is a Rational Reconstruction, since Morishima used the properties of non-negative square matrices and linear programming, which Marx clearly could not have known about.
3. But if the equivalence between the signs of the rate of surplus value and of the rate of profit was really something which Marx meant, there is no reason to view these formal developments as an unsurmountable obstacle, according to the Skinnerian criterion. (I mean, there are no reasons other than Marx's possible intellectual limitations – which would be a rather odd argument.) Thus Morishima's fundamental theorem could be viewed as an alternative account, meeting the requirements of a Historical Reconstruction, of what Marx had said in some passages from *Capital*, Book 3.

This illustrates the fact that when a statement falls within the sphere of Intellectual History, it might well be a Rational Reconstruction, but it might equally be a Historical Reconstruction. This non-canonical typology, which includes Intellectual History as a fifth genre, could be derived from Rorty's paper; yet it is rather different from – and far less conflictual than – the canonical typology we are used to, which was nonetheless also derived from Rorty's paper.

#### **4. Towards a non-canonical typology**

Independently of Rorty's development of the idea of Intellectual History, there is a longstanding and familiar intuition that what we can learn from the past might, at least, be meaningful for economists today. More than thirty years before the publication of Rorty's paper, in his *History of Economic Analysis* Schumpeter noted that in economics "much more than in physics have results been lost on the way or remained in abeyance for centuries" (Schumpeter 1954, p. 5), such that it was the task of historians of economics to revive them.

And a similar position is evinced by more recent authors, albeit stated from different points of view or in different terms. Illustrating his claim by reference to monetary theory, Cesarano (1983) argued that economic knowledge was increasing in the long run, whereas in the short run its trend showed oscillatory movements, and thus there was room for smoothing them by means of the history of economic thought. In the same way, Hollander (1987) noted that the replacement of one theory by another often appeared as the result of a change in what he called the “focus of attention”, and that it was one of the tasks of historians of economic thought to move the focus back to the theory that had been abandoned. This point, he claimed, was illustrated by the revival of the Smithian distinction between productive and unproductive labour in order to explain the weakness of the British rate of growth (Hollander 1987, p. 1). Drawing on Lakatos, Negishi argued that research programs in economics at times enter periods of hibernation, and that it was the role of historians of economic thought to bring them out of hibernation, as Keynes or Sraffa were supposed to have done when reading respectively Malthus or Ricardo (Negishi 1989).

Even Blaug’s paper from 2001 might be regarded in this way. At first sight it relies on the same canonical typology, but the approach he favoured had changed: he gave up on Rational Reconstruction, and turned to Historical Reconstruction on the basis of an efficiency argument according to which new propositions, which didn’t belong to present economic knowledge, might be found in the works of past authors. The idea had clearly been in the air for a long time – long enough that it was unnecessary to make reference to Rorty’s contribution on this point. Blaug, then, could view it as part of a Historical Reconstruction, enlarged to encompass *Geistesgeschichte* – yet without noting that for Rorty, the approach involved in the efficiency argument fell under Intellectual History, not Historical Reconstruction.

In much the same way, previous Presidential Addresses to the ESHET Conferences have expressed concern about the possibility that the history of economic thought might be able to renew contemporary economic knowledge. These addresses were opportunities to recall the identity of the discipline and its distinctiveness, even when its relation with other disciplines was also praised (like, for instance, economic history for Annalisa Rosselli in 2012; see Rosselli 2013). Among them, some were directly concerned with the kind of approaches which prevail in the history of economic thought. This was the case for Heinz Kurz’s contribution in 2006, Maria Cristina Marcuzzo’s in 2008, and Hans-Michael Trautwein’s in 2016 (Trautwein 2017).

I would like to call our attention not to the differences between these perspectives – for of course there are differences – but rather to what they have in common. They have in common a special ambition of scouring the works of past authors in order to find ideas, propositions, or, as we might more generally put it, statements, which do not belong to present economic knowledge, and which potentially challenge it, and which might even contribute to its transformation.

When compared with the canonical typology, they make its limitations obvious. All of them, in their own ways, promote a special vocabulary to support various typologies of approaches in the history of economic thought which might be related to the canonical typology; but they also all make room for approaches which search the works of past authors for issues which do not belong to our present knowledge, thus also opening the path to a non-canonical typology. This allows us to link them to Rorty’s five genres of historiography (see Table 2).

<b>Rorty (1984)</b>	<b>Kurz (2006)</b>	<b>Marcuzzo (2008)</b>	<b>Trautwein (2017)</b>
<i>Geistesgeschichte</i>		Historical reconstruction =	Exploration of the past
Historical reconstruction		{ Contextual analysis, Historical narrative }	Exploration of the past / Correction
Intellectual history	Heterodoxy	Textual exegesis / Quest for alternatives	Exploration of the past / Blindspots & backtracking
Rational reconstruction		Textual exegesis /	Decoration
Doxography	Whiggism	Quest for ascendancy	Abbreviation

**Table 2.** ESHET Presidential Addresses: towards a non-canonical typology

Heinz Kurz (2006) pointed to *heterodoxy* as an alternative to “Whiggist” interpretations (according to terminology drawn from a 1931 book by H. Butterfield), which I relate to the canonical typology’s category of Rational Reconstruction. Cristina Marcuzzo (2008) noted the role of *textual exegesis* in the search for new ideas, and also of the *quest for alternatives*, though she viewed this as a kind of Rational Reconstruction. And, drawing on the idea that today’s increasing specialization in economics also increases the risk of neglecting the history of economic thought, Hans-Michael Trautwein (2017) found support in what he called *blind spots identification* and *innovation by backtracking*, which both require the highest skill in the

history of economic thought and are of the highest general interest. Clearly, the categories of heterodoxy, textual exegesis, quest for alternatives, blind spots identification, and innovation by backtracking, correspond to the search for new results (Schumpeter 1954), the historical smoothing of oscillations in economic theories (Cesarano 1983), the change in the focus of attention (Hollander 1987), and the awakening of hibernating research programs (Negishi 1991). And, of course, they also correspond to Rorty's Intellectual History, his fifth genre which fitted so badly into Blaug's absolutist–relativist perspective that it found no place in the canonical typology.

This, then, is the outline of the story of how we have done the history of economic thought in recent decades. It is the story of the birth of a canonical typology of approaches in the history of economic thought, and the story of its dissolution, which opened room for an alternative approach.

## **5. A proposed typology**

We can put this point differently by focusing on the opposition between old and contemporary statements (see Lapidus 1996; 2016). I have used the term “extensive approach” for any account of old statements in terms of other old statements, irrespective of their nature, or the discipline in which they are located. This corresponds to what Blaug, after Rorty, called *Geistesgeschichte* or Historical Reconstruction. An obvious example of an extensive approach is Karl Pribram's attempt, in his book published posthumously in 1983, to promote what he called a history of economic *reasoning*. Such an approach amounts to viewing economics as an application of principles of reasoning elaborated elsewhere. By “elsewhere”, he means the discipline which aims precisely at building principles of reasoning – that is, philosophy. Hence, in Pribram's book one finds chapters on Thomist economics, Cartesian economics, and Kantian economics, respectively appealing to Thomas Aquinas, Descartes, and Kant. The intervention of philosophy in a historical account is only one example of an extensive approach: in fact the term covers a range of possibilities which are not usually considered together. For instance, a link might be established between a political situation and economic propositions: such could be the case if we follow Cannan (1892) in interpreting the contributions of Malthus, Ricardo, and West in February 1815, which led to a novel explanation of rent, as an impact in the field of economic theory of debates within the political field about the Corn Laws. Or, in the same way, we might be concerned with such a link if,

following Mitchell (1967–69), we paid special attention to technological and organizational change in accounting for changes in economic theory.

In Figure 1, I have drawn up on the left a set of old statements, irrespective of whether they are by nature economic or not. Areas shaded in medium or strong grey represent the subset of what *we* consider to be theoretical statements in economics. Areas in light grey represent what *we* consider to be other kinds of statements. This reference to “we” is the irreducible mark of present knowledge, of which Rorty said that it could never be cancelled out, in any genre of historiography. Consequently, the extensive approach is simply a way to pass from old statements in economics to other old statements.

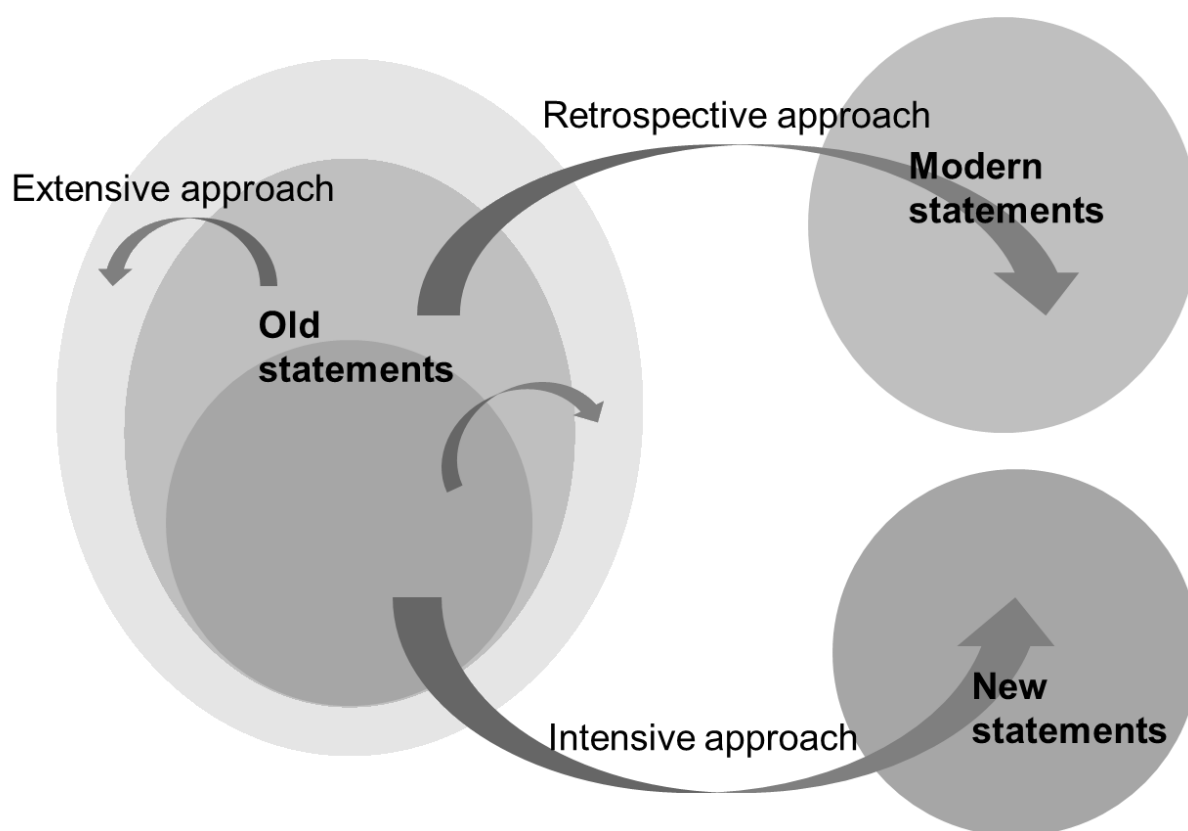
I have used the term “retrospective approach” for any attempt to account for old statements in terms of the present-day economic statements which they prefigure. This obviously corresponds to Rorty’s categories of Rational Reconstruction and Doxography. An exemplary instance was given by Blaug himself when, with a certain sense of humour, he dedicated his famous handbook *Economic Theory in Retrospect* “to David Ricardo, my son”. This shows more clearly than any more sophisticated comment what adopting a retrospective approach involves: Blaug’s naming his own son after the famous economist reflects the manner in which we, like Blaug, view David Ricardo or any other past author much as if they were our children – that is, as people full of promise, who might yet be what we have already succeeded in becoming, and who reflect our current knowledge imperfectly, naïvely, and with less mastery than we now have.

Again in Figure 1, I have set out on the right hand side, in medium grey, modern economic statements belonging to contemporary knowledge. The retrospective approach therefore amounts to linking an appropriate subset of old statements marked in medium grey with the set of modern statements.

Now, it should be clear from Figure 1 that something structural is missing. Specifically, whereas the extensive approach potentially concerns all old statements, the retrospective approach concerns only those old statements which might be viewed as more or less accurate prefigurations of what today is known. Thus there is room to introduce a missing approach, which I have called the “intensive approach”. In Figure 1, this would be another way to relate old to contemporary statements. Old statements (in dark grey) are retained only insofar as

they do not prefigure modern statements, but can still produce new statements (also in dark grey), thus renewing present knowledge.

Like the retrospective approach, the intensive approach *is* concerned with contemporary economic theory; but, like the extensive approach, it *is not* concerned with what we already know. And unlike either of these approaches, it is concerned with the challenges faced by our knowledge in its present state. My contention is that the intensive approach covers Kurz's (2006) building of "heterodoxies", Marcuzzo's (2008) "quest for alternatives", and Trautwein's (2017) identification of "blindspots".



**Figure 1.** A non-canonical typology

## 6. The intensive approach as a heuristic

The intensive approach therefore supports the intuitive view that the history of economic thought is not simply self-sufficient – as it surely is on an extensive or retrospective approach, much like any academic practice in the history of ideas which doesn't require further justification – but is also a way to contribute to advances in economics. But if so, this means that the history of economic thought should be counted among the possible means to feed into

economic theory. Thus besides the direct access to economic theory enjoyed by economists, and the indirect access which also belongs to economists trained in empirical or experimental economics, we should also add the access made available through the history of economic thought, which is no less real for being less commonplace. And access in this instance requires an intensive approach.

Nonetheless, it turns out to be unexpectedly difficult to find evidence not only of the efficacy of the intensive approach, but even of its very existence: as if it was the missing piece of a puzzle which we can only imagine and in no sense hold in our hands. The extensive and the retrospective approaches were illustrated above by the handbooks of Karl Pribram (1982) and Mark Blaug (1962) respectively. Is there any handbook which might also illustrate the intensive approach? At the beginning of his *History of Economic Analysis*, Schumpeter argued that the history of economic thought was a proper means to find lost or idle ideas (Schumpeter 1954, p. 5). But in spite of its scholarly achievements, and of its enduring originality, Schumpeter's book falls far from providing a significant example of an intensive approach, and rather provides its readers with numerous illustrations of the extensive or the retrospective approaches. It is all the more puzzling that, setting handbooks aside, we have to wrack our brains to find convincing instances of the intensive approach actually being employed. I can give only a few examples.

For many of us, Sraffa's introduction to the works of David Ricardo in 1951, and especially his interpretation of Ricardo's so-called *Essay on Profits* (1815), in relation to his 1960 theory of the prices of production, might be viewed as an impressive illustration of what an intensive approach can do (see also Lapidus 1996; 2016). And as already mentioned, Morishima's interpretation of Marx through his Marxian fundamental theorem might be another example (Morishima 1973; 1974), alongside Leijonhufvud's reading of Keynes's theory of unemployment (Leijonhufvud 1968), or Sen's reading of Smith's conception of poverty in relation to the capabilities approach, or of the impartial spectator in relation to open impartiality (Sen 2002). We could also add to this rather meager list some "if only" examples, suggesting that, with a few tweaks, history might have been different and have left more room for the intensive approach: *if only* Paul Romer had read Smith before his 1987 paper; *if only* Daniel Kahneman had read Bentham before his joint paper with Amos Tversky from 1979. I could go on wracking my brain, but I guess that what I intend to ask is already obvious: why, considering its potential importance, are there so few examples of the intensive approach in action?

The answer seems to be that, unlike the extensive or retrospective approaches, the intensive approach is by nature a *transient* approach. I'll give two examples, drawn from those mentioned above.

The first is Sraffa's interpretation of Ricardo's *Essay on Profit* in 1951. Let's engage in the following thought experiment: we are transported back to 1951 and, despite this backward transportation, we remember everything of our lives except what is connected to Sraffa's works from 1951 onwards. What would we think of Sraffa's newly published introduction to his long-awaited first volume of the new edition of Ricardo's works? Some of us would agree with Sraffa's interpretation of the *Essay*, accepting the idea of the structural specificity of agriculture (corn made with corn), so that the profit rate might be viewed independently of prices, and rather as a ratio of homogeneous quantities. Others would not: but this is not my point. I want to draw our attention to the fact that at this time, in 1951, we would regard Sraffa's introduction as a typical instance of what I've called an extensive approach. That is, an approach which accounts for what Ricardo did on the basis of his works, his correspondence and discussions, and on the basis of which we might draw inferences, rightly or wrongly, about what he meant.

Now we are transported forward to 1960, and Sraffa publishes his *Production of Commodities by Means of Commodities*, in which he introduces the concept of a "basic commodity", standing for a composite commodity produced by itself and labour. At this very moment, but only then, when reading and according credit to Sraffa's appendix D on his sources (Sraffa 1960, p. 93), and particularly to his understanding of the role of corn in Ricardo's system, which he has generalized into the idea of the basic commodity, his approach appears as intensive: it was clear to the reader in 1960 that the subtitle of Sraffa's book ("Prelude to a critique of economic theory") was an understatement. He had gone far beyond a simple prelude to a critique, and instead supplied a positive alternative to the standard theories of prices. And he did this through his reading of a text nearly 150 years old,<sup>4</sup> and by giving a solution to a problem he found therein.

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<sup>4</sup> Strictly speaking, when Sraffa reviewed the circumstances in which he made his finding of the idea of a basic commodity, he himself cast doubt on the idea that he might have been led from his historical investigation to his analytical discovery: "It should perhaps be stated that it was only when the Standard system and the distinction



And after this? Continuing our journey through time, let us return to the present. Whatever our interest in the theory of prices of production and in its capacity to challenge other theories of prices, we must agree that it is now a part of contemporary knowledge (see, among others, Kurz and Salvadori 1995) – to such an extent that if we should try to explain, today, that what Ricardo wrote in 1815 was an intuition about a theory of prices of production where corn is the only basic commodity, this would be an example of a retrospective approach. That is, it would be an account of what Ricardo meant by what he said in 1815: a prefiguration of what we now know so well, and so much better than he did, two centuries later.

My second example comes from Amartya Sen's unwavering interest in Adam Smith, especially on matters related to individual behavior, justice, and moral philosophy. Some of his works focus directly on Smith: they were written either alone (Sen 1986; 2010a; 2010b; 2011) or in collaboration with his wife Emma Rothschild (Rothschild, Sen 2006), whose book on Smith and Condorcet (Rothschild 2001) seems to have had great influence on him. Others, although they deal with other issues, deliberately leave room for reminders of the link between what Sen is now doing as an economist and what Smith wrote some two centuries earlier (for instance, Sen 1987; 2002; 2009). As with Sraffa's relationship with Ricardo, from the mid-1980s onwards Sen was giving an interpretation of Adam Smith which contrasted with some major interpretations then extant, exemplified for Sen by Stigler's (1971) account, to which he systematically referred (see, for example, Sen 1986, p. 31; 1987, pp. 17-22), notably in order to call into question the "self-interest" line of reading of Adam Smith which was used, Sen said, to support a "cramped and simplistic theory of human rationality" (Sen 2010a, p. 54). All of this falls within an extensive approach whose conclusions might have been (and obviously were) debated.

Yet although until 2002 Sen's emphasis in his contributions on Smith seems mainly to have been on issues related to individual behavior, rules of conduct, and individual rationality, something different appeared after 2002, where his interest now seems to be morality and justice (see Bréban, Gilardone 2018). In his 2002 paper, he continued his re-interpretation of Smith, extending it now to the impartial spectator which Smith had introduced in the *Theory of Moral Sentiments* (1759). He understood Smith's account of the impartial spectator as

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between basics and non-basics had emerged in the course of the present investigation that the above interpretation of Ricardo's theory suggested itself as a natural consequence" (Sraffa 1960, p. 93).

supporting the idea that public reasoning on justice should include not only people directly concerned by the deliberation itself (the “focal group”), but also people from outside this group (Sen 2002, pp. 449-51). Again, such an interpretation, which Sen set in opposition to the Rawlsian one (Sen 2002, pp. 451-53), might be disputed (Ege, Igersheim, Le Chapelain 2016; Bréban, Gilardone 2018); however, it remains an outcome of an extensive approach directed at reappraising some features of a major contribution by Adam Smith.

On the other hand, however, and as confirmed in further works (Sen 2009), Sen explicitly drew on his interpretation of Smith’s impartial spectator in order to introduce the idea of “open impartiality” in his theory of justice (Sen 2002) – a theory of *comparative* justice which he views as rooted within a tradition that includes Smith and Condorcet, and which had been overshadowed by the *transcendental* tradition rooted in Hobbes or Rousseau, and now exemplified by Rawls’s theory of justice (Rawls 1971). This, in turn, is an example of an intensive approach which aims at challenging the Rawlsian theory of justice through reference to writings that are more than two centuries old.

Sen refined his previous argument in 2009, when he published *The Idea of Justice* (dedicated to the memory of John Rawls). But things had changed since 2002: when discussing the link between Smith’s impartial spectator and open impartiality within a theory of comparative justice, he is now dealing with the relation between an account of the work of an old author and what he claims to be his legacy within contemporary academia. Hence the whole discussion can be viewed methodologically as engaged in a retrospective approach.

For both Sraffa and Sen, what I have called the “intensive approach” therefore proves to be a transient approach in the history of economic thought, poised between the extensive and the retrospective paradigms. And it is because of this transient nature that the intensive approach is too fleeting, too short-lived, to give birth to an object as long-lasting as anything like a handbook in the history of economic thought. Yet the transient nature of the intensive approach is the consequence of its being a heuristic, whose potential effect is a transformation of present economic knowledge by means of historical investigation. No less, yet no more. The intensive approach is born from the extensive; and from the moment it is implemented, it is destined to vanish into the purely retrospective.

On the one hand, for a historian of economic thought whose concern is to contribute, no matter how modestly, to the economic knowledge of her or his time, the transient nature of

the intensive approach might seem unappealing: no handbooks; no firm construction upholding the memory of one's work; a visibility restricted only to major results like Sraffa's, Morishima's, or Sen's. While, at the same time, one's everyday results, all these minute potential changes to economic knowledge induced by close historical investigation, add up to barely any steps away from the field of history of economic thought towards that of economics. From this point of view, the history of economic thought may well appear to be a superfluous and excessively complicated detour, at least to the extent that it aims at the concrete transformation of economic knowledge on a par with empirical or experimental approaches to economics.

But, on the other hand, the history of economic thought also effects an incredible extension of the number of past authors with whom today's historians of economic thought may talk, share ideas, and jointly enter into new projects – at least in so far as they themselves are engaged in an intensive approach. In the introduction to the volume in which Rorty's 1984 paper was published, the editors asked the reader to imagine a thousand-volume edition of a giant *Intellectual History of Europe*. To imagine also that all the authors whose works were discussed therein should be restored to life, and would all then read all of each others' books, engaging in discussion, dialogue, dispute, and common projects (Rorty, Schneewind, Skinner, 1984).

Transposing this to economics and to what I've called the intensive approach, the present implication is that we may consider works which we normally view as belonging to the early history of our discipline, like Jevons's *Theory of Political Economy* or Marx's *Capital*, as having just been written. Moreover, this would also mean that they were written very shortly after Smith's *Wealth of Nations*, which was itself published almost simultaneously with Quesnay's *Tableau économique*, etc.: as if time were being compressed, though leaving history intact. And then we invite the authors to engage in dialogue with each other as well as with ourselves – not only for the purposes of presentation, as if we wanted to enhance the impact of these authors' works, for this is the least we can hope to do as historians of economic thought. No: it is above all a way to take what they wrote and thought seriously; so seriously that we are willing to confront it with what we as economists now know. Through what we write about them, they continue to challenge our knowledge, in continuum with what they did in their own periods.

Keynes, Jevons, Marx, Smith, Quesnay, and so many others: they are still with us. As historians of economic thought, we have often been charged with speaking with the dead. Yet this is precisely not what we do: we bring the dead alive, all of them. And we may be proud of it.

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