Euro. J. History of Economic Thought 13:4 463–488 December 2006 Routledge Taylor & Francis Group

Whither the history of economic thought? Going nowhere rather slowly?*

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Theorien sind gewöhnlich Übereilungen eines ungeduldigen Verstandes, der die Phänomene gern los sein möchte und an ihrer Stelle deswegen Bilder, Begriffe, ja oft nur Worte einschiebt. Man ahnt, man sieht auch wohl, daß es nur ein Behelf ist; liebt sich nicht aber Leidenschaft und Parteigeist jederzeit Behelfe? Und mit Recht, da sie ihrer so sehr bedürfen.[1]

(Goethe 1953: 440)

1. Introduction

My talk today will be devoted to the history of economic thought. If a presidential address delivered at a meeting of economic theorists was to deal with the history of our subject, this would hardly come as a surprise. It would in all probability be considered a sign of good taste and a welcome opportunity for the speaker to display his erudition. It might also be seen 'as an innocuous respite from pushing back the frontiers of economic knowledge' (Winch 1962: 193). And it would, last but not least, be an occasion to recall the advances of the younger generations of economists over and above the older ones and, more generally, to exemplify the 'progress' in economic analysis. Seen from the Olympian heights of modern economics our predecessors look like dwarves, still visible, though, but dwarves nevertheless. They populate deep valleys, where there is little light and much darkness. I hasten to add that this picture is, of course, a caricature and that I have myself attended talks at meetings of economists in which the heritage of the past was not belittled and there were no signs of complacency or even arrogance. However, I have also experienced the opposite and it seems to me that the overall trend points in this direction.

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The European Journal of the History of Economic Thought
ISSN 0967-2567 print/ISSN 1469-5936 online
http://www.tandf.co.uk/journals
DOI: 10.1080/09672560601063929

Against this background, a talk on the history of economic thought at a meeting as this one makes one hear the alarm bells ring. Indulging in narcissistic introspection must be motivated by a bleak future of the subject.

There are indeed signs of this sort and they have been with us for a long time.[2] Historians of economic thought are an endangered species and their natural habitat – faculties of economics – are becoming less and less hospitable. The marginalization of the subject has been going on for quite some time. Yet there are conflicting signals. It was only last year that we were confronted with the shocking news that *History of Political Economy* (*HOPE*), a distinguished journal in the field, was being removed from the Social Science Citation Index (SSCI). Shortly afterwards we learned that beginning with 2005 another specialist journal in the field, *The European Journal of the History of Economic Thought* (*EJHET*), was to be covered by the SSCI. And in late spring 2006 we were relieved to hear that *HOPE* was reinstated for full coverage. If I am not mistaken, then, this is the first time that two specialist journals in the history of economic thought, *EJHET* and *HOPE*, are being indexed. In a discipline in which indexation in the SSCI has almost become the proverbial golden calf around which the profession dances, it is a question of survival to be included or not. It affects especially our younger colleagues without whom there won't be a future for our subject. With two journals getting coverage, the situation now is much rosier than it was.[3] Nevertheless, for reasons that hopefully become clear in what follows, I would not recommend anyone who starts an academic career now to become *only* an historian of economic thought. It seems to me to be much safer to have a second leg in economics. Walking with two legs is not only more comfortable but also less risky than walking with only one.

The composition of my talk is as follows. In Section 2 I begin with a short discussion of the aim of economics and the history of economic thought. Then, in Section 3, I summarize some arguments questioning the usefulness of the history of economic thought by our fellow economists. Next, in Section 4, I turn to arguments in defence of our subject and provide some illustrations. Section 5 concludes that economics is too important a subject to be left exclusively to our fellow economic theorists.

As early as 1962 Donald Winch expressed the view that 'The subject [of the history of economics] can only survive if it establishes itself on a firm and independent footing' (Winch 1962: 203) – independent, that is, from the economics profession. While I have some understanding of, and sympathy for, this view, I believe that there are strong reasons to try to regain lost territory within the profession. The following therefore contains some suggestions as to how this could perhaps be accomplished.[4]

Before I enter into a discussion of the main argument let me point out that the subtitle of my talk derives from a paper by Michael Ruse on how Darwin's *The Origin of Species* was received and his ideas developed. Ruse distinguishes between two trends, one 'bad', the other 'good'. The bad one he traces back to Herbert Spencer who tried to make evolution into a doctrine of progress, from the weak to the strong and from the not so good to the better. The main representative of the good trend is said to have been Darwin himself, to whom evolution was 'a directionless process, going nowhere rather slowly' (Ruse 1988: 97). There is a question mark also to the subtitle of my talk, and while I have views on which directions to search for an answer, I do not pretend to be possessed of one.

2. In quest of light or fruit?

The first question we have to address, albeit briefly, is: what is or should be the aim of economics, what the aim of the history of economic thought? In *The Economics of Welfare* Pigou wrote in 1920:

> When a man sets out upon any course of inquiry, the object of his search may be either light or fruit – either knowledge for its own sake or knowledge for the sake of the good things to which it leads. . . . In the sciences of human society, be their appeal as bearers of light ever so high, it is the promise of fruit and not of light that chiefly merits our regard.
>
> (Pigou 1920: 2–3)[5]

If this is meant to apply to economics, and it is, how much more does it apply to the history of the subject! Recent developments worldwide appear to have shifted the scales even more in favour of the promise of fruit and away from light compared with the time of Pigou. Many universities in Europe are currently facing severe budget cuts and are trying to finance research to a growing extent via grants coming from various sources, both public and private. The reputation of a scholar is measured more and more not only in terms of SSCI citations he or she gets, but also in terms of the funds attracted by him or her. Globalization and intensified competition have led to a redefinition of the role of universities in society. Politicians request universities to purge their study programmes of subjects that are not directly 'useful to society'. These trends apparently work against our subject. Historians of economic thought have generally greater difficulties to raise research funds. And they cannot credibly portray themselves as being able to solve the problems of the day.[6]

The counterposition of fruit and light is, however, spurious. Things are invariably more complex. This has been emphasized by Fritz Machlup in his book *Knowledge and Knowledge Production.* He commented on Pigou's above statement thus:

> I must confess that I am less mission-oriented than Pigou wanted social scientists to be. I would undertake my inquiry even if it promised nothing but light; but I believe that fruit can grow and ripen only where there is enough light, and that most inquiries that shed light on problems, societal or not, eventually prove useful to society. I fear, however, that a requirement to justify each research project in the social sciences by its 'promise of fruit' can become a stultifying constraint.
>
> (Machlup 1980: 11)

We may ask: is the history of economic thought capable of shedding light on societal problems – in the hope and expectation that in this way it can prove its usefulness? The answer to this question given by some of our fellow economists is a resounding no. Let us briefly turn to the main reasons put forward by them.

3. Why bother about 'the wrong opinions of dead men'?

The strongest verdict on the history of economic thought, which according to Mark Blaug expresses 'the philosophical overhang of positivism' (Blaug 2001: 146), comes from those who think that economics is, or should be, shaped in the image of the 'hard' sciences, preferably physics. While opinions to this effect have been sounded from an early time onwards, they appear to have gained weight over the years. Here we need not be concerned with whether the image these scholars appear to have of the natural sciences stands up to close examination or whether it reflects a view that is obsolete by now, as some historians of science maintain.[7] What matters for the purpose of our argument is that the underlying concept of hard science entertained is tantamount to claiming that science is invariably cumulative in the sense that there is progress, progress and only progress; there is never regress. The process of the production and absorption of knowledge is taken to be perfect: whatever is good and valuable will be retained, whereas whatever is weak and erroneous will be weeded out. If this was to be true, there could only be an antiquarian interest in the past: why bother about 'the wrong opinions of dead men', to use Pigou's famous phrase?

Some such view was and still is widespread in the sciences and in mathematics. In the words of Alfred North Whitehead (1926: 162): 'A science which hesitates to forget its founders is lost.' Essentially the same view has been expressed with regard to political economy as early as the first half of the 19th century by Jean-Baptiste Say. In his *Cours complet d'économie politique pratique* he stressed:

> ... l'histoire d'une science ne ressemble point à une narration d'évènements. Elle ne peut être que l'exposé des tentatives, plus ou moins heureuses ... pour recueillir et solidement établir les vérités dont elle se compose. Que pourrions-nous gagner à recueillir des opinions absurdes, des doctrines décriées et qui méritent de l'être? Il serait à la fois inutile et fastidieux de les exhumer. ... *Les erreurs ne sont pas ce qu'il s'agit d'apprendre, mais ce qu'il faudrait oublier.*[8]
>
> (Say 1840: 540–1; emphasis added)[9]

In order to forget the errors made, it is necessary to abandon the history of political economy.

Theorists of knowledge and epistemologists appear to be agreed that it is via making errors and correcting them that man learns (see, for example, Audi 1998). Therefore, Say's argument is not compelling. However, is his view of economics as a science tenable? And is the history of economic thought in the long run doomed to death, as he was convinced? Similar views have been expressed time and again. Are we still in a transitory phase, as David Gordon (1965) argued in a paper published over 40 years ago? In his view, 'Economics lies somewhere between the extremes but surely [sic] closer to the natural sciences than to the humanities, and perhaps further in that direction than any other social science' (Gordon 1965: 122). He went on to contend that 'economics has never had a major revolution [sic]; its basic maximizing model has never been replaced' (ibid.: 124). The latter is considered 'our basic paradigm' which, Gordon contends, has already been postulated by Adam Smith; alas, he forgets to provide any evidence in support of his view.[10] This paradigm is said to have 'created a ''coherent scientific tradition'' (most notably including Marx)' whose 'persistence can be seen by skimming through the most current periodicals' (ibid.: 123). Having become 'very much like a normal science', economics, like a normal science, 'finds no necessity for including its history as a part of professional training' (ibid.: 126). Seen in this way, the decline of the history of economic thought is but the other side of the coin whose front side shows the rise of a science to maturity. As Gordon put it: 'I conjecture that this is a decline from what might be called an ''abnormal'' level' (ibid.: 126).

A similar view has been expressed by Paul Samuelson in several contributions, especially in his keynote address at the History of Economics Society meeting in Boston in 1987.[11] There he proposed a programme for what he dubbed the 'Whig history of economic science' (Samuelson 1987). By this he meant a re-orientation of the history of economic thought 'toward studying the past from the standpoint of the present state of economic science' (ibid.: 52). He motivated his proposal in a way similar to Gordon by asserting that there were no 'Kuhnian breakthroughs in current economic science' and that 'ours is not an age of heady accomplishments and new exciting syntheses' (ibid.: 52). Ironically, he tried to exemplify the cumulative character of the normal science of economics with reference to Piero Sraffa's edition of *The Works and Correspondence of David Ricardo* (Ricardo 1951–73) and his reformulation of the classical approach to the theory of value and distribution. As is well known, Sraffa saw his respective analysis explicitly as a return to the standpoint of the classical economists in the theory of value and distribution and as providing the foundations of a critique of the marginalist doctrine (cf. Sraffa 1960: v–vi). Sraffa thus challenged the cumulative-cum-continuous perspective maintained by the Whig history concept. In numerous papers Samuelson has ever since tried to refute Sraffa's claim (see, in particular, Samuelson 1978).[12]

The Whig concept still leaves some room for the history of economic thought, but a fairly small one. As a colleague told me several years ago: 'Why bother with the history of economics? What is good is all in Samuelson's *Foundations of Economic Analysis*, and what is not there to be found can't be any good.' This view is nicely epitomized by the title of a paper by Kenneth Boulding (1971): 'After Samuelson who needs Adam Smith?' The point of view under discussion is reminiscent of a famous remark made on the occasion of a fire destroying a world renowned library: 'This is no loss, because the books burnt contained either what is in the Koran or what is not in it.'

Before I continue, let me stress that it should come as no surprise that people interested in the history of economic thought often happen to be heterodox economists of various leanings. This fact has been noticed by several observers; see recently Blaug (2001). The negative task reserved to the history of economics by Whiggism of pointing out what from the standpoint of modern economic theory are the errors and misconceptions of past authors, while certainly important is hardly a rewarding enterprise. However, as we shall see below, there is a much wider scope for historians of economic thought than this, and the reason for it is precisely that the foundations upon which much of contemporary economics is built are much less solid than is commonly taken for granted within the profession. By reversing Samuelson's above proposal we could say that a further task of the history of economic thought is to study the present state of economics from the standpoint of past authors. Seen from this vantage point, it should become clear why historians of economic thought are almost *ex definitione* bound to be to a smaller or larger degree heterodox. Ideally, they know better: they know not only contemporary economics, they also know the preceding vintages of economics and thus what has been gained and what lost in the course of time. If you happen to believe in the normal science interpretation of economics, or Whiggism, then you had better believe also in the ultimate demise of the history of the subject and get ready for changing your profession!

After this excursion I now come back to the main argument. A closely related reasoning contra the history of economic thought contends that with the accumulation of 'verified knowledge' ever harder choices have to be made in research and teaching. The history of economic thought has to make room especially for mathematics, statistics and econometrics – indispensable tools without which an access to the superior levels of contemporary knowledge are blocked. The opportunity costs of an hour spent on the history of economic thought are said to increase sharply over time. Therefore the subject is of necessity bound to decline. This is occasionally coupled with a further argument. If studied properly, the history of economic thought is a very demanding and time-consuming subject. To provide only an introduction into it runs the risk of remaining at a superficial and anecdotal level. Therefore it is better to abolish the subject as a whole.

A further reason has not so much to do with the subject as with those who, in the critics' perception, specialize in it. The history of economic thought, the argument goes, is typically studied by people who are not clever enough to do economic theory. It is an abode of the semi-numerates and involves adverse selection. Those striving for intellectual excellence are well advised to avoid the subject. Wrong-headed people are attracted by ideas that have long been shown to be untenable. Young and inexperienced scholars must be prevented from being misled. In order to enhance its status among the sciences, economics ought to leave behind its history. Abandoning the history of economic thought is seen as an act of purification that allows the discipline at large to focus scarce resources on advancing economic knowledge.

While it cannot be denied that some historians of economic thought do not exactly contribute to the reputation of the field, the same can of course also be said about some economists. In this context two things deserve to be mentioned. First, several of the most important economists ever have also been major historians of economic thought. Think, for example, of Karl Marx, Eugen von Böhm-Bawerk and Joseph A. Schumpeter. And some of the most influential economic theorists of the last century have shown a deep interest in the subject. Think, for example, of John Maynard Keynes, Kenneth Arrow, Paul Samuelson or Amartya Sen. Some of the proximate reasons that might explain their interest will be dealt with in the following section. Secondly, a more serious issue is the following one: despite all the effort put into becoming, or at least behaving, like a normal science, economics appears to have suffered from a significant loss in reputation relative to some other disciplines both in the academic and in the public domain over the last few decades.[13] While in the past a discipline that was on the rise gave occasion to great hopes as to its capacity to contribute to solving economic and social problems, economics in more recent times has lost much of its former nimbus and is often regarded as barren and irrelevant when it comes to tackling practical problems. The decline in reputation is reflected by the fact that vacant chairs in economics are no more filled and entire departments have been closed down. Historians of economic thought can hardly be blamed for this development, so what has gone wrong? More precisely, what is wrong with contemporary mainstream economics and why does our discipline lose ground relative to other disciplines? And has this anything to do with the marginalization of fields such as the history of economic thought or economic and social history? Is the mathematization of the subject at the cost of other forms of analysis responsible for the decline? Is the decline due to a growing perception that economic theory is 'ahead of conceptual clarity' (Aghion and Howitt 1998: 435)? Is this development, *horribile dictu* to some economists, an expression of the fact that economics has failed to live up to the standards of a normal science and therefore was bound to lose in importance? And could things be improved if economics were no longer to follow what some other economists consider to be a will-o'-the-wisp?

These are difficult questions and the profession at large will have to face them. For the purpose of this paper it suffices to stress that the real problem is not so much the history of economic thought as the present state of economics itself. The attack on the former by some representatives of the latter may be seen as a mistaken attempt to put the blame for the decline of the discipline as a whole on those whose profession it is to write as dispassionately as possible about its development. It is somewhat reminiscent of the antique habit of decapitating those who brought bad news. To the best of my knowledge this habit never improved the situation of anybody and seriously damaged that of some people.

Finally I should like to draw the attention to an argument that is ambivalent. The growth economist Paul Romer in a contribution to a book about the current state of macroeconomics opined: 'If you devote too much attention to ancestor worship, you can get trapped and lose the chance to see things from a new perspective' (Romer 2005: 679).[14] Romer, to be clear, does not advocate a disregard of the history of economic thought, although he somewhat deridingly speaks of 'ancestor worship'. He rather expresses the fear that too intensive a dealing with it might block one's originality. While there is some truth in what Romer says in the above, it is also true that a thorough knowledge of the history of economic thought is not without value. First, it prevents one from putting forward spurious claims to originality.[15] Secondly, it prevents resources from being wasted on 'reinventing the wheel'. Thirdly, it shows one why certain ideas, or the forms in which they have been presented, of necessity led into dead ends, and how such dead ends can be avoided. Finally, it sharpens one's perception of what could be fruitful avenues of research and what not.

This brings me to a few further arguments in support of the history of economic thought, arguments against 'provincialism in time', as Lionel Robbins (1978: 2) succinctly put it.

4. Against 'provincialism in time'

4.1. General observations

The first argument is that economics is not characterized by a smooth selection process in the sense described above. Scrutiny shows that it neither preserves all that is good nor eliminates all that is bad. The teleological view of our subject cannot be sustained.[16] Joseph A. Schumpeter, a foremost economic theorist and a foremost historian of economic thought, apparently was not convinced by the normal science view of economics. How could he otherwise have put forward the paradoxical claim that studying the history of the subject exposes the student to 'new ideas' and 'insights into the ways of the human mind' (Schumpeter 1954: 4)?[17] A given idea is always new to some specific person or group of persons. Ideas that have been enunciated for the first time years or decades or centuries ago may look strikingly new to the modern economist, and the less the latter knows about the history of the subject the larger the number of ideas that can be expected to look novel to him or her.

There are numerous cases where ideas at first fell flat on the ground and were rediscovered only later. Had there been no interest in the history of economic thought they would have fallen into oblivion forever. They might have been reinvented by some later economist, but why not make use of an idea which, once made available, has the property of being non-rival and non-excludable and thus enhances our knowledge base? The economics of innovation teaches us that upon their discovery inventions frequently are not implemented because the environment into which they are born is not hospitable, especially because it would not be profitable to adopt them. It is only after the environment has changed or after some complementary invention(s) in the same or some other industry has (have) seen the light of the day that the former invention is effectively applied.[18] I see no reason why a similar mechanism should not be at work in the realm of economic ideas and conceptualizations.

It may also be the case that a theory is abandoned, not because it is intrinsically flawed, but because the form in which it has first been put forward does not stand up to close examination and because both its advocates and critics were unable to realize its full potential. The deeper reason for such a possibility is, of course, that in general there is no presumption that the tools available to economists at a given moment of time are always up to the complexity of the concepts entertained by them. In no small part, it seems, debates in the history of our subject reflect tensions between tools and concepts. There is also no reason to presume that such a tension, while a characteristic feature of the past, is absent in the present. Given the complexity of the issues economists are typically dealing with one may even be prompted to conclude that economic knowledge always has been and always will remain in the nature of patchwork.[19]

Here it suffices to illustrate what has just been said with reference to development economics and economic geography. In his Ohlin lectures, Paul Krugman (1995) tells the stories of the rise, fall, and eventual resurrection of these fields and explicitly puts the blame for the fall on 'the limiting nature of our intellectual style' (Krugman 1995: viii–ix). A characteristic feature of this style is that in order 'to be taken seriously an idea has to be *something you can model*' (ibid.: 5). This implies that ideas or areas that the currently available technical tools cannot yet reach are ignored. 'Areas of inquiry that had been filled in, however imperfectly, became blanks. Only gradually, over an extended period, did these dark regions get reexplored' (ibid.: 3). Krugman had to engage in 'dabblings in intellectual history' in order to benefit from the fecundity especially of German contributions to spatial economics from von Thünen to Launhardt and Weber.[20] The fact that something can be 'modelled' does not mean, of course, that the modelling is able to bring to full life a particular idea that has been formulated only verbally. There are cases in which models, while suggestive, are not faithful to the ideas they purport to present in rigorous terms.[21]

While it is possible that a theory or an idea is prematurely abandoned because of the defective form in which it has been put forward, there is also the possibility that a theory or an idea is retained although it is defective in substance, and is known to be so. This is indeed a fact that is much more important than appears to be generally admitted. According to Keynes,

> The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt

> from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back.

Keynes continued:

> I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas. Not, indeed, immediately, but after a certain interval; for in the field of economic and political philosophy there are not many who are influenced by new theories after they are twenty-five or thirty years of age, so that the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest. But, soon or late, it is ideas, not vested interest, which are dangerous for good or evil.
>
> (Keynes 1936: 383)

One can only wonder whether the power of vested interests is indeed as small as Keynes seems to imply, and whether interests and ideas can be counterposed.[22] Vested interests typically use the medium of ideas in order to emphasize their legitimacy; therefore the two are frequently in a symbiotic relation with one another. Be that is it may, the important point made by Keynes is the following. There has been a history of economics, yet this history is never over: for better or worse old ideas and concepts are still with us. What has just been said does not only apply to the gullible laymen but also to our fellow economists including those involved in counselling and advising on policy matters. The spectres of the past are still very much alive. It is a myth reflecting both vested and entirely new interests that economics as a whole has successfully grown out of the dark ages and is dealing only with ideas and concepts that are both true and bear fruit.

Two particularly astounding examples of the continuing employment of concepts that have long been shown to be untenable in general are the aggregate production function and the concept of capital as a factor whose quantity can be ascertained independently of relative prices and thus income distribution.[23] As to the aggregate production function, it has been shown that such a function cannot be derived from microeconomic information other than in excessively special conditions.[24] Macroeconomists are (or ought to be) aware of this and some express unease when they use such functions. In the context of a discussion of the role of non-rival goods in economic growth Paul Romer emphasized: ‘After all, if one is going to do violence to the complexity of economic activity by assuming that there is an aggregate production function, how much more harm can it do to be sloppy about the difference between rival and nonrival goods?’ (Romer 1994: 15–16). Interestingly, the same Romer stated two years later: ‘Only 30 years ago many economists still objected to a mathematical statement of the relationship between output and capital in terms of an aggregate production function and an aggregate stock of capital, *Y=f(K, L)*' (Romer 1996: 202). Things have changed since. How is it possible that many of those who advocate the view that economics comes close to the natural sciences at the same time defend the use of analytical concepts that cannot be sustained?

The answer to this question is methodological, not theoretical. The use of the aggregate production function is defended on the ground that what matters is not the realism of the basic premises of a theory, but its predictive power, the 'realism' of the results that can be derived with its help. One may adopt whichever premises serve this purpose, irrespective of how bold, unreal or even fancy they may look to the observer. The use of such artificial constructions as the immortal 'representative agent' who maximizes his or her utility over an infinite horizon, etc. are all justified in this way. Modern economics is in large parts under the spell of one variant or another of intrumentalist methodology. It was first advocated by young Schumpeter (1908) and then forcefully propagated by Milton Friedman (1953). There are several problems with this point of view that have been laid out in great detail in the literature.[25] Here it suffices to point out two things. First, according to instrumentalism a theory cannot be rejected simply on the ground that all the building blocks it employs are questionable or have even been shown to be untenable. However, if this were to be so, how could 'erroneous' ideas ever be eliminated from the canon of economics? Instrumentalism, it seems, is at odds with the cumulative science conception of economics and thus the Whiggish point of view. This is supported by a second and closely related consideration. Since there is no generally accepted view as to how to assess the 'predictive power' of a theory, economics lacks clear-cut criteria for judging the quality of a theory or deciding which one among several theories dominates over its rivals. This uncertainty as to how to assess the empirical performance of alternative models can be illustrated, for example, by the contention put forward by Mankiw *et al.* (1992) that the conventional Solow approach to economic growth does better than the competing 'new' or 'endogenous' growth models. While on the surface this may well be so, Felipe and McCombie (2005) have shown that there is no reason to become complacent, for the 'good fit' of the Solow model is simply a reflection of the fact that an accounting identity has been tested.

In short, modern economists have no reason to look down on past masters and would often benefit if they knew (or knew better) their works. In the environment depicted, historians of economic thought could perform useful tasks in at least three respects. First, they can be expected to be familiar with the origins and developments of ideas which, while still ruling the world, have ceased to rule the profession. Historians could act as critics, interpreters and intermediaries who bring back to life the historical conditions in which the ideas under consideration emerged, whether vested interests played any role in this, which transfigurations these ideas underwent over time, and what they mean today and why they are still held by some people. Historians could contribute to enlightening both groups of clients, the non-experts and the experts. Secondly, historians of economic thought, at least the generalists among them, should in principle be able to act as intermediaries between economists specializing in different areas within the profession. As Adam Smith knew very well, specialization comes at a price, which he described vividly in terms of the dangers of degradation and deprivation of the 'labouring poor'. In economics something similar appears to happen: an ever more sophisticated division of labour is reflected in an ever increasing fragmentization of knowledge. The economist who knows everything about a vanishing problem and very little or nothing about the rest is almost proverbial.[26] Historians of economic thought, to the extent to which they manage to keep abreast of the broad developments in economic analysis, could perform a useful role similar to Adam Smith's

> philosophers or men of speculation, whose trade it is, not to do anything, but to observe every thing; and who, upon that account, are often capable of *combining together the powers of the most distant and dissimilar objects*. In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens.
>
> (Smith 1976: I.i.9; emphasis added)

While historians of economic thought are perhaps not well advised to battle against the tendencies of specialization in the field, they might effectively contribute to mitigating its negative implications. This necessitates, of course, that a sufficiently large number of them care about what is going on in contemporary economics and actively seek discussions with economic theorists. (This may include conversing with oneself.) I am aware of Donald Winch's warning that this might end up in 'a dialogue of the deaf' (Winch 1997: 401). But it might also end up differently. If it does, it might contribute to overcoming somewhat knowledge fragmentization within each subfield in economics and lead to a better mutual understanding and respect of theorist and historian. Both are badly needed. Clearly, not only the modern theorist is in danger of provincialism in time; the historian of economic thought is exposed to the same pitfall. Provincialism in time ought to be fought on both sides.[27]

Finally, historians of economic thought ought to perform the role of critics of present as well as of past economists. It would be unfair to pin them down to a criticism of the latter only. Since, as we have seen, modern economics is intimately tied up with instrumentalism, a main task of historians of economic thought is to critically assess this stance and compare it with alternative ones. This ought to be done on a general and abstract level and then exemplified by case studies of modern pieces of analysis. There is a huge amount of work waiting to be carried out!

Modern economic analysis may also be used to sharpen an argument in favour of the history of economic thought. The latter, we have said in the above, is a treasure trove of ideas that can be exploited by the modern economist. We have then cited Adam Smith's reference, in chapter I of book I of *The Wealth of Nations*, 'On the division of labour', to 'philosophers and men of speculation' whose main business it is to 'combine' and recombine existing particles of knowledge in order to create new economically useful particles. The combinatoric metaphor as an abstract description of the innovation process is thus already to be found in Smith (and possibly long before him). It recurs in Schumpeter's 'new combinations' (Schumpeter 1934: 66) and in a somewhat different form in Kenneth Boulding's 'tree of knowledge' (Boulding 1956: 95). More recently Martin Weitzman expressed it with the help of combinatoric mathematics in his concept of 'recombinant growth' (Weitzman 1998).

We may draw a parallel between knowledge generated in the technical sphere and knowledge generated in economics. We may begin by paraphrasing a consideration Kenneth Arrow (1969: 29–30) originally put forward with regard to technological progress. Ideally, progress in economic theory consists in the first instance in a reduction of uncertainty as to the working of the economic system. The product of economic research is an observation on the world which reduces its possible range of variation. Progress typically proceeds by recombining and developing known ideas: new ideas are produced by means of old ones. This in itself should be enough to make it abundantly clear that economics could not exist without its history. There is simply no idea put forward today that has no ancestors. Not for nothing Pallas Athena, the goddess of the arts and sciences, was imagined to have arisen from the head of Zeus. One would be inclined to say that the history of economic thought is for economics what Zeus was for Pallas Athena, if one did not know that the former is true whereas the latter is a myth. Therefore, it is utterly ridiculous to see some contemporary economists portray their own work as if it was a construction *ab ovo*. There is no such thing (see also Blaug 2001: 156). When even gods have ancestors, it should come as no surprise that economists have precursors. To pride oneself on quoting only papers published in the last five years is a sign not of scholarship but of ignorance. I do not want to dispute, of course, the possibility that there are new areas of research which cannot look back at a long history, but more often it is not so. I also do not want to insinuate that whenever an author writes on a particular topic he or she must refer to all the precursors of the argument under consideration. Nothing of this sort is being proposed. It suffices to avoid signs of complacency and arrogance.

We may now, following Weitzman, give the combinatoric metaphor a more precise form. As Weitzman stressed, 'new ideas' are not some exogenously determined function of 'research effort' in 'the spirit of a humdrum conventional relationship between inputs and outputs'.

> It seems to me that something fundamentally different is involved here. When research effort is applied, new ideas arise out of existing ideas in some kind of cumulative interactive process .... To me, the research process has at its centre a sort of pattern-finding or combinatoric feel.
>
> (Weitzman 1998: 332)

According to Weitzman what happens may be compared to the activities in an agricultural research station in which pairs of existing 'idea-cultivars' are combined to bring about new 'hybrid ideas' (where the word 'cultivar' is an acronym for *culti*vated *var*iety). With I as the number of idea-cultivars, the corresponding number of different binary combinations that can be got from I is $C_2(I)$, which is given by

$$C_2(I) = \frac{I!}{(I-2)!2!}$$

For example, with $I=4$, we have $C_2(4)=6$; with $I=5$, we have $C_2(5)=10$; and with $I=6$, we have $C_2(6)=15$, etc. In economics new ideas typically result from reformulating and combining several previously known ideas.

The important message of Weitzman's otherwise rather mechanistic argument is that the growth in the number of ideas that results from combining reconfigured existing ideas is remarkable and well exceeds exponential growth. Would the entire potential of recombinatory possibilities always be exploited in full, then the growth of the number of knowledge particles would over time increase almost without limit. If this reasoning were to be applicable to the creation of knowledge in economics, and *cum grano salis* I believe it is, then it follows that it would be self-damaging if economics were to get rid of the history of economic thought.

4.2. *Two illustrations*

I conclude this section with two examples that illustrate some of the aspects mentioned *in abstracto* in the above.[28]

Example 1. The first example concerns Piero Sraffa's rediscovery of the classical surplus approach to the theory of value and distribution, and his demonstration that contrary to the received Marshallian interpretation this approach is fundamentally different from the demand and supply approach of the marginalist authors. Sraffa's work led to a major reinterpretation of the classical economists and a coherent reformulation of their doctrine of value and distribution. This doctrine, he was convinced, had prematurely been abandoned due to a grave misunderstanding: the defective labour value-based form in which it had been handed down by the classical economists had erroneously been taken for a defective substance. Hence for decades the potential of the classical approach had been left undeveloped – a case of crass failure of an allegedly efficiently working selection process.[29]

One of the most intricate problems the classical economists faced when dealing with the dynamism of the modern economy was that of a growing heterogeneity and diversity of things: commodities, services, labours, etc. How to deal with an ever expanding cosmos of goods? Here we can focus only on a single aspect of this problem. Ricardo sensed that in a world characterized by ubiquitous quantitative and qualitative change his fundamental proposition on income distribution had to be formulated with regard to magnitudes that are independent of the *kind* of commodities produced. Such magnitudes are the *share* of wages in the social product (or 'proportional' as opposed to real or commodity wages), on the one hand, and the *rate* of profits, on the other: 'The greater the portion of the result of labour that is given to the labourer, the smaller must be the rate of profits, and vice versa' (Ricardo, *Works*, vol. VIII: 194). Ricardo's proposition was meant to be applicable to a system incessantly in motion, that is one in which capital accumulates, the population grows, some natural resources become scarce, and there is technological progress. However, his proposition was not fully correct, because with a circular flow in which commodities are produced by means of commodities the general rate of profits, r, depends not only on the share of wages, w. As Marx already saw and Sraffa showed, it also depends on a technical property of the system of production in use – the maximum level of the rate of profits, R, which reflects the system's surplus generating capacity. With wages paid *post factum* and wages and prices expressed in terms of the 'standard commodity', the sought expression of the general rate of profits is

$$r = R(1 - w)$$

It applies independently of the heterogeneity and diversity of commodities and is thus the appropriate tool for investigating the development of r over time via the development of its determinants R and w – the level of technical knowledge embodied in the methods of production actually employed and income distribution. And it confirms an intuition of Ricardo's who in a letter to Malthus of 9 October 1820 had written:

> Political Economy you think is an enquiry into the nature and causes of wealth – I think it should rather be called an enquiry into the laws which determine the division of the produce of industry amongst the classes who concur in its formation. *No law can be laid down respecting quantities, but a tolerably correct one can be laid down respecting proportions.*
>
> (Ricardo, *Works*, vol. VIII: 278–9; emphasis added)

This, I believe, is Ricardo's quintessential message: for a system permanently changing from within, an economic 'law' can only be formulated with regard to distributive *proportions*.

The failure of the classical economists to elaborate a coherent theory of value and distribution based on the concept of social surplus was due not to mistaken analytical concepts but to the inadequacy of the tools at their disposal. When Sraffa was eventually able to overcome their difficulties within their own analytical structure, this involved an advance both in economic theory and in interpreting the classical authors: progress came as a joint product.

Example 2. The second example concerns a modern attempt to come to grips with the dynamism of the capitalist economy that parallels somewhat the attempt of the classical economists, especially Smith. The paper I have in mind is Paul Romer's 1990 essay on 'Endogenous technological change' (Romer 1990). How does Romer tackle the problem at hand? He combines

(a) the endogenous production of new 'industrial designs' as in Romer (1986) with
(b) the formalization of the role of human capital in economic growth as in Lucas (1988) and
(c) a product diversity specification of physical capital derived from Dixit and Stiglitz (1977).

Romer's model can thus be said to illustrate the combinatoric metaphor of knowledge creation. Economic theorists are typically synthesizers and there is no reason *per se* to be dismissed as such.

Scrutiny shows that the main ideas underlying Romer's model have ancestors that can be traced far back in the history of our subject. What is new is the translation of what we knew already rather well into the official language of intertemporal equilibrium macroeconomics, today's 'intellectual style'. The idea of the establishment of a research and development industry that generates new economically useful knowledge – element (a) – as an aspect of an ever deeper division of social labour can be traced back to Smith and, of course, many other earlier writers. Also the education aspect of human capital formation – element (b) – is foreshadowed in Smith. Recall his statement: 'A man educated at the expence of much labour and time to any of those employments which require extraordinary dexterity and skill, may be compared to one of those expensive machines' (Smith 1976: I.x.b.6). The generation of such expensive human 'machines' presupposes a sufficiently large market and is thus an integral part of an ever deeper division of labour, which involves new methods of production that enhance labour productivity.

The truly original novelty of the paper, according to Romer's own judgement, is however element (c): 'The unusual feature [sic] of the production technology assumed here is that it disaggregates capital into an infinite number of distinct types of producer durables' (1990: S80). This is motivated with view to the empirical fact, stressed by the classical economists, that economic development is reflected *inter alia* in a growing number and diversity of produced means of production. The hypothesis is close at hand that the productivity of the economic system as a whole is somehow related to this number. Romer expresses this by stipulating the following extension of a Cobb–Douglas function

$$Y(H_Y, L, x) = H_Y^{\alpha} L^{\beta} \sum_{i=1}^{\infty} x_i^{1-\alpha-\beta}.$$

where H_Y denotes the amount of human capital employed in the final output sector, L the number of workers employed in it, and $\sum_{i=1}^{\infty} x_i^{1-\alpha-\beta}$ the employment of intermediate products. Final output is thus an additive separable function of the various intermediate products. Since at a given moment in time there is only a finite number A of them, $x_i = 0$ for all $i > A$. Over time A increases, and this increase involves an increase in total output, labour productivity and income per capita.

A careful examination of Romer's model shows that the heterogeneity and diversity of capital goods is more apparent than real. In fact, all capital goods are taken to be commensurable with one another independently of relative prices and income distribution.[30] However, this problematic aspect of the model need not concern us here. We also set aside other disquieting features of the model, such as the assumption that 'knowledge' and 'human capital' are *cardinally* measurable.[31] We rather compare element (c) which serves as a proxy for the productivity enhancing effect of improved machinery and other instruments of production with earlier conceptualizations in order to see more clearly what has been gained and what lost by Romer's formalization.

The concept of a growing 'product diversity' exhibits some startling similarity with the concept of the 'superiority of more roundabout methods of production' as it was advocated by representatives of the 'Austrian school' of economics. Originally, in Carl Menger, the concept was designed precisely for the purpose of reinterpreting Adam Smith's idea of a deepening social division of labour over time, but in Böhm-Bawerk at the latest it degenerated to the static concept of a description of alternative methods of production at a given moment of time when analysing the problem of the choice of technique. Böhm-Bawerk for this purpose elaborated the concept of the 'average period of production'. It is well known that Böhm-Bawerk's concept cannot be sustained other than in exceedingly special circumstances.

Seen against this background, Romer's treatment of the (growing) diversity of capital goods can be interpreted as trying to kill two birds with one stone. On the one hand it attempts to capture Smith's dynamic concept of the division of labour and the differentiation of the means of production that comes with it; on the other hand it purportedly allows one to aggregate heterogeneous capital goods without knowledge of relative prices and thus the rate of profits. As to the former aspect, one could say that in Romer there is some sort of 'absolute period of production', whose 'length' is given by the number of different kinds of intermediate products, A, actually employed. Over 'time', that is, via an increase in A, production is rendered more 'roundabout' and thereby 'superior', which is reflected in a growing labour productivity and a growing income per capita. Yet, the way in which Romer conceptualizes a 'lengthening' of the period of production is rather peculiar. Since Romer takes each and every intermediate product to be technically and economically immortal, it will never be superseded by some other intermediate product: once produced, it will forever be used. An example may illustrate what is implied by this. If, say, final output happens to be wheat, then wheat in ancient Egypt was produced by means of digging sticks only, whereas today it is taken to be produced by means of digging sticks and ploughs and oxen and tractors and combine harvesters etc., *all employed simultaneously*. This is certainly an extreme conceptualization of a growing capital input diversity which is squarely contradicted by an even casual observation of facts.

These few observations must suffice. The readers are invited to have a closer look at Romer's model and judge for themselves what has been gained and what lost in terms of the formalization provided.

5. Concluding remarks

In this paper it has been argued that economics is not the alleged perfect selection mechanism that preserves each and every economic idea that is valid and useful and jettisons all ideas that are not. The teleological view of the subject cannot be sustained. Therefore the task of the history of economic thought cannot be limited to the study of the past from the standpoint of the present state of economics. Another important and intellectually perhaps more rewarding task is to study the present state of economics from the standpoint of past authors. The history of the subject is a treasure trove of ideas. The potential for growth in the number of ideas that results from combining reconfigured existing ideas exceeds exponential expansion. Would the entire potential of recombinatory possibilities always be exploited, then the growth of the number of knowledge particles would over time increase almost without limit. The history of economic thought may play a useful role in economics by preserving valuable ideas which otherwise would fall into oblivion. To foster the subject is therefore also in the interest of general economists. In terms of a famous dictum of Adam Smith: 'It is not from the benevolence of [them], that we expect our [right to exist as historians of economic thought], but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages' (Smith 1976: I.ii.2).

The paper provides a number of examples to illustrate its main messages. It argues in particular that the classical approach to the theory of value and distribution was prematurely abandoned, because a deficiency in the form in which it had been put forward was mistaken for a deficiency in substance. It is stressed, among other things, that the classical authors were keen to deal with an indisputable fact of development and growth: the growing variety and diversity of commodities. The paper then scrutinizes a prominent recent paper on endogenous growth by Paul Romer that also seeks to come to grips with a growing variety of durable capital goods as an integral part of the process of the division of labour. It is argued that the model exhibits several disquieting features, including the one that homogeneity 'masquerades as variety', as one critic succinctly put it. The model exemplifies well what Krugman has called the 'limiting nature' of the contemporary 'intellectual style' in economics, where an idea gets a hearing only if it is dressed up as a model using an optimizing framework. This, however, every so often turns out to be a Procrustean bed into which well-known and perfectly sensible ideas are forced. The net value added by applying this intellectual style is not always clear, and occasionally it may well be negative.

This should be enough to insist that economics is too important a subject to be left exclusively to our fellow economic theorists.

Notes

* Presidential address delivered on the occasion of the annual conference of the European Society for the History of Economic Thought (ESHET) in Porto, Portugal, 28–30 April 2006. For valuable suggestions in the preparatory phase of this paper I thank Tony Aspromourgos, Stephan Boehm, Giancarlo de Vivo, Gilbert Faccarello, Christian Gehrke, Harald Hagemann, André Lapidus, Axel Leijonhufvud and Bertram Schefold. I am grateful to Tony Aspromourgos, José Luís Cardoso, Volker Caspari, Daniel Diatkine, Gilbert Faccarello, Duncan Foley, Christian Gehrke, Geoff Harcourt, André Lapidus, Antoin Murphy, Nerio Naldi, Manseop Park, Luigi Pasinetti, Neri Salvadori and Ajit Sinha for valuable comments and suggestions on an earlier draft of this paper. The views expressed in this address are entirely my responsibility.

1 'Theories are commonly outpourings of an impatient mind that would like to get rid of the phenomena and therefore replaces them by images, notions and often merely words. One senses and even sees that this is only a poor substitute; but does not passion and faction spirit always love substitutes? And rightly so, because they are so much in need of them.'

2 See, for example, the papers collected in Blaug (1991); see also Winch (2000) and Boehm *et al.* (2002).

3 The SSCI and ISI, the private company that elaborates it, has assumed almost a monopoly position in the field. 'Monopoly', Adam Smith was convinced, 'is a great enemy to good management, which can never be universally established but in consequence of that free and universal competition which forces every body to have recourse to it for the sake of self-defence' (Smith 1976: I.xi.b.5). Some competition, I believe, would have a beneficial effect also in the field under consideration.

4 It goes without saying that I do not dispute in the least the importance of the history of economic thought as the discipline that studies the contributions of economists in their own times and places, of the importance of historical reconstructions, of the study of original sources, archival work, etc. However, there is an intertemporal dimension to the problem at hand that is the focus of the following discussion.

5 Nerio Naldi has drawn my attention to the fact that Pigou had presented his above version of the Marshallian view of what economics is about as early as in his Inaugural Lecture, 'Economic theory in relation to practice', in 1908 as successor to Marshall's Chair.

6 The question is of course close at hand whether our fellow economists fare better in this regard. The loss in reputation of economics relative to other disciplines experienced over the past few decades does not appear to point in this direction. I shall come back to this below.

7 See in this context Ilya Prigogine's (2005: 69) statement: 'In all fields, whether physics, cosmology or economics, we come from a past of conflicting certitudes to a period of questioning, of new openings. This is perhaps one of the characteristics of the period of transition we face at the beginning of this new century.'

8 '… the history of a science does not compare to a narrative. It cannot be anything else other than an exposition of the more or less fortunate attempts … to collect and solidly establish the truths of which it consists. What could be gained by collecting absurd opinions, the rejected doctrines that merit to be so? It would at the same time be useless and boring to exhume them. … *Errors are not those that deserve to be learned, but to be forgotten.*'

9 Cited after Lapidus (1996: 870).

10 For a refutation of the neoclassical interpretation of Smith, see Winch (1997).

11 It is interesting to note that Samuelson in his 1962 paper on 'Economists and the history of ideas', commenting on the idea of cumulative knowledge, was explicit only in the following regard: 'mathematical knowledge has been cumulative' (Samuelson 1962: 5, n. 2).

12 For a recent assessment of Sraffa's contributions from different perspectives, including Samuelson's, see Kurz (2000); for a discussion of Samuelson's views on the classical authors and Sraffa, see the forthcoming exchange between Garegnani and Samuelson in *EJHET*, 14(1) (2007); for a discussion of Sraffa's interpretation of the classical and marginalist economists in his hitherto unpublished papers, see Garegnani (2005), Kurz (2005a), Kurz and Salvadori (2005) and Gehrke and Kurz (2006).

13 The relative loss in reputation of economics is perhaps best seen in terms of how other scientists assess the achievements of some of the so-called 'Nobel Prize' winners; see in this regard Henderson (2005). (While Alfred Nobel funded the prizes named after him, which are awarded by the Royal Swedish Academy of Science, the economics prize is given by the Swedish Central Bank.)

14 I am grateful to Manseop Park who brought this statement to my attention while I was a visiting professor at Korea University in October 2005.

15 A malevolent person might contend that those who advocate getting rid of the history of economic thought are motivated by an interest to constrain competition of ideas only to contemporaries and cut out past economists.

16 This has been argued by several historians of economic thought, e.g. Lapidus (1996). Even if there happens to be advancement in some sense, it is not necessarily linear. Cesarano (1983) has put forward compelling evidence that in monetary theory evolution follows a cyclical pattern, with certain ideas disappearing and reappearing again over time.

17 On Schumpeter's contributions to economic analysis and its history, see, for example, Kurz (2005b).

18 Think, for example, of Ricardo's discussion of the case in which improved machinery can be introduced only after money wages have risen due to rising food prices because of diminishing returns in agriculture (Ricardo, *Works*, vol. I, 395).

19 Paul Samuelson wrote with regard to economic controversies of the past: 'The fact that contemporaries quarrel must be understood against the background of an age in which writers imperfectly understood their own theories' (Samuelson 1987: 56). I fear that there is no reason to presume that this age is or ever will be over in economics.

20 For a summary account of this tradition, see Blaug (1997: ch. 14).

21 As Gilbert Faccarello stressed in private correspondence with me, there is another striking example where ideas we have known for a long time, but wich had been lost

sight of, all of a sudden reappear in new garb and context. Thus, 'new Keynesian economics', championed by Stiglitz, Greenwald, Weiss and others, puts forward an analysis of banking and financial activities in which moral hazard and adverse selection play a crucial role. Much of what is being said now was anticipated by Adam Smith (1976: II.ii).

22 See in this context Samuelson's anti-Whiggish statement: 'One must never make the fatal mistake in the history of ideas of requiring a notion that it be ''true''. For that discipline, the slogan must be, ''The customer is always right.'' Its objects are what men have *believed*; and if truth has been left out, so much the worse for truth, except for the curiously-undifficult task of explaining why truth does not sell more successfully than anything else' (Samuelson 1962: 14–15).

23 Other examples are close at hand and include Say's law and the quantity theory of money. As Lapidus (1996) stressed, there is a tendency to preserve bits of theory that are simple to teach to the detriment of others that are more difficult. Pedagogical preoccupations may involve some sort of Gresham's law in the selection process.

24 Interestingly, Lucas (2002: 56) admitted that if the heterogeneity of capital 'was the issue in the famous ''two Cambridges'' controversy, then it has long since been resolved in favor of the English side of the Atlantic'. Paradoxically, he went on: 'Physical capital, too, is best viewed as a force [sic], not directly observable, that we can postulate [sic] in order to account in a unified way for certain things we *can* observe' (ibid.).

25 For a criticism of instrumentalism in its different forms, see Lawson (1997).

26 See in this regard Hayek's (1937: 49) discussion of the division of knowledge which, in his view, 'is quite analogous to, and at least as important as, the problem of the division of labour'.

27 One is reminded of Keynes's *bon mot*: 'I do not know which makes a man more conservative – to know nothing but the present, or nothing but the past' (Keynes 1970: 277).

28 The following summarizes briefly what in the original version of the paper was laid out in some detail. The reader interested in the full argument is asked to consult the web page of ESHET (www.eshet.net/).

29 In a recent contribution Mark Blaug (2001: 156) maintained: 'There is nothing predetermined about our current theories and if years ago, economics had taken another turn at a critical nodal point, we would today be advocating a different theory.' This can hardly be disputed. One such 'nodal point', and arguably a most important one, is discussed in the above. Had the classical authors not failed to provide a coherent version of their approach to the theory of value and distribution, the history of economics might have taken a radically different path. 'Economic knowledge', Blaug insisted, 'is path-dependent' (ibid.).

30 For details, see the long version of the paper; see also Park (2006), who argues that in all currently available horizontal innovation models homogeneity masquerades as variety.

31 If knowledge is not cardinally measurable, *nothing* can be said about returns to scale, marginal and average products, growth rates etc. (see Steedman 2003).

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Abstract

The paper argues that economics is not a perfect selection mechanism that preserves each and every economic idea that is valid and useful and jettisons all ideas that are not. The teleological view of the subject cannot be sustained. Therefore the task of the history of economic thought cannot be limited to the study of the past from the present state of economics. Another important task is to study the present state of economics from the standpoint of past authors in order to see what has been gained and what lost in the course of time. The history of the subject is a treasure trove of ideas. The history of economic thought may play a useful role by preserving valuable ideas which otherwise would fall into oblivion. To foster the subject is therefore also in the interest of general economists.

Keywords

Classical economics, hard science, history of economic thought, instrumentalism, intellectual style, new combinations, recombinant growth, Whig history