The Future of History of Economics: Young scholars' perspective[1]

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Version of August 20, 2006

Word Count: 2232

This symposium was originally motivated by what we saw as two striking features of the debate about the future of history of economics. First, we were struck by the fact that although many of the participants in this debate take the future to be bleak indeed, significant numbers of young, smart, and ambitious scholars continue to join the field. There is in fact much reason for concern: the decreasing weight attached to history of thought in economics education (especially in top-ranked research institutions), the decline in opportunities to pursue graduate study in this field, the small number of job openings for historians of economics, and so on. Though apparently aware of all this, young scholars continue to make history of economics their discipline; their presence is obvious at annual meetings of national and international organizations, in summer schools, and elsewhere. Presumably, this does not just reveal a preference for a future in the discipline, but also a belief that there will be such a discipline, and that it will contain a place for them. It is possible that many of these young scholars are in for a nasty surprise, but their existence might also signal that the future of the discipline is not quite as depressing as some would have it.

Second, we were struck by the fact that so few young scholars in history of economics participate in the debate about the future of their discipline. Some do, of course, including the five junior contributors to the 2002 *HOPE* supplement (Weintraub 2002). Still, these voices constitute only a small sample, and as a result are unlikely to reflect the full depth and width of opinion among junior scholars. Having participated in a number of informal discussions about the topic - including the discussion group formed for this purpose at the research center PHARE in Paris in 2003 - we know that many scholars entering the field find the terms of the current debate unsatisfactory, and that they would like to take an active part in it. It is hardly surprising that those early in their careers take an intense interest in, and have their own opinions about, the future of their discipline. What is surprising is the fact that those opinions do not get more public expression.

This symposium was organized in order to provide young scholars in history of economics with a public forum for expressing and discussing their views about the future of the discipline. We were hoping to learn more about why, despite the gloomy outlook, some students choose to start a career in the discipline. It is of course possible that these students are simply unable to do "real" economics, or that they suffer from youthful innocence. Yet, we thought it possible that they have reasons for thinking that the discipline has a future, and that it will contain a place for them. If so, we wanted to know what those reasons are. Moreover, insofar as there will be a discipline at all a few years from now, it will inevitably be shaped by those who are now beginning their work in the field. Thus, by listening to young scholars, we were hoping to learn something about the future of our discipline.

While recognizing that it would be impossible to find a truly representative sample of young scholars, we nevertheless hoped to reach as broad an audience as possible. Thus, we formulated a call for papers that did not discriminate on the basis of nationality or of institutional or disciplinary affiliation. We distributed the call for papers widely, by posting it to the HES mailing list and by sending it to a number of individuals as well as to several national and international associations.[2] We also put up a symposium website, and were pleased to see that several other sites chose to link to it.

Our call for papers asked potential contributors to consider, first, whether history of economics has a future at all. Insofar as they answered in the affirmative, we also invited them to explore what they saw as the nature of the discipline. Questions included: How will future historians of economics be educated? What will be the character of their work? What methods will they use? How will they make themselves relevant? How will they disseminate their work? What will their institutional home be, and how will they interact with other fields? We expected participants to suggest a diagnosis of the current state of the discipline, and to identify likely trends for its continued development. Furthermore, because the debate from the outset has had a strong normative component, we encouraged contributors to explore not only what they think is happening and will happen, but also what they think *should* happen. We expected participants to use whatever approach was most relevant for the points they wished to make: historical approaches for historical points, philosophical approaches for philosophical points, and so on. Because of time constraints, we asked for clear, bold statements with brief supporting arguments.

The response was encouraging. We received more submissions than we had expected, and were surprised by their overall quality. The writing suggested that authors were strongly concerned with the problem at hand, and that they had already thought a good deal about the topic. Given the number of papers received, we had to make a selection. Because we wanted to include a broad set of ideas, we chose papers on the basis of diversity of opinion as well as cogency of the argument. Thus, the selection was not based on any preconceived notion of what the "right" answers to our questions might be. Albeit to a lesser extent, we also tried to involve young scholars with diverse disciplinary backgrounds, regions of origin or residence, and professional status.

We ended up with five papers that we felt optimally met these criteria. The panel (which included the present authors) was in fact highly diverse. The eight participants were men and women, of seven nationalities,[3] and currently active in six countries.[4] There were three graduate students, two post-docs, and three recently appointed assistant professors. While most have or are pursuing Ph.D.'s in economics, one had a background in philosophy and science studies, and one in history and philosophy of science as well as in economics. Some see history of economics as their primary research field, while some do not. Incidentally, the diversity of the participants serves as an additional reminder that the contributions do not reflect a joint position. All contributions are expressions of individual opinions, and in fact participants disagreed with each other on multiple points.

In spite of the diversity of opinion, at least two points of agreement emerged. First, participants share the concern that history of economics should maintain close ties to economics. Thus, Ivan

Moscati argues that historians of economics are better off maintaining strong connections (both theoretically and academically) with economics, and he cautions against what he calls “history of economic thought as science studies”, which he sees as moving in the opposite direction. Other participants argue for close ties between history of economics and economic theory by highlighting the potentially fruitful interactions between the two. Hence, Rebeca Gomez Betancourt and Alexander Tobon try to reinterpret and renew the French tradition by arguing that historians of economics have a quite precise role to play within economics, viz. as “generalist economists.” Without claiming that increased specialization is a bad thing, they maintain that increased specialization has generated a need for such generalist economists. Marta Sora argues, among other things, that history of economics helps us not just to understand the meaning and significance of economic theory, but to assess its validity as well. She concludes that history of economics may be particularly important in emerging and transition economies. Nuno Pedro G. Palma reviews four reasons why history of economics is relevant to economics, and maintains that historians of economics would be taken more seriously by mainstream economists if the historians spent more time working on contemporary and mainstream issues. Meanwhile, Eric Schliesser argues that there are reasons internal to the way evidence works in science in general to rediscover the importance of history of economics.

Second, participants appear to agree that it is important for historians of economics to have a solid background in economics. Moreover, several also agree that this background should include contemporary economic theory. Moscati uses the Italian case to develop this point, arguing that it would be useful for historians of economics to acquire a stronger background in the core tools of current economics, instead of receiving specialized training in their primary field only. Similarly, Palma’s proposal presupposes that historians of economics have or are willing to acquire substantial knowledge of cutting-edge research in economics. Although Gomez and Tobon do not explicitly address this issue, their view of historians of economics as generalist economists presupposes that historians of economics master straight economists’ tools.

As indicated above, participants also disagreed with each other on certain points. For example, Schliesser objected to Palma’s claim that it will ultimately be more rewarding to study recent writers such as Robert Lucas rather than older figures like Adam Smith. More significantly, participants did not agree on the question of the most appropriate institutional arrangement. While many participants maintained that historians of economics belong primarily in economics departments, Schliesser suggested that some historians of economics may at least for the time being find homes in other departments, specifically, in philosophy and in history and philosophy of science (HPS) departments, which are likely to be open to some of the questions that historians of economics address. On the latter point one of the present authors strongly agreed. In part because philosophy and economics have a variety of historical, conceptual and theoretical affinities, Angner maintained that philosophy/HPS departments are one natural home - though not the only one - for history of economics scholars.

On the whole, we were delighted not only with the response to our call for papers, but also with the panelists’ dedication. By reading and commenting on each other’s contributions both before and after the symposium, they clearly signaled their deep interest in and commitment to the future of the history of economics. In passing, we were encouraged by the fact that a group of Italian students organized an analogous session in Lecce, Italy, a few weeks afterwards. We also received a positive response from the audience, with several leading scholars attending the session and passionately participating in the discussion that followed. Incidentally, the quality of the debate highlights the important role that international associations and yearly meetings may have in fostering the work of young scholars in history of economics, despite a difficult environment. Conferences, summer schools, etc. provide critically important opportunities to fill in gaps in one's knowledge, to expose oneself to unfamiliar ideas, themes, and approaches, to sharpen one's arguments, and to simply share survival strategies with others working under similar conditions.[5]

The symposium also underscored the benefits of multiple backgrounds among history of economics scholars. In our view, this diversity of perspective expanded the range of positions expressed and improved the quality of the discussion. A community of scholars trained in a variety of fields can should be familiar with a broader literature and master a more diverse set of tools and techniques. Such a community should also be better able to communicate and collaborate - for mutual benefit - with scholars in neighboring fields. This point can be a source of (limited) hope: if indeed history of economics can have multiple disciplinary affiliations, then the fact that it is losing ground in economics departments need not spell death to scholarship in the area. However, we need to acknowledge the fact that scholars from different disciplines may have different aims and purposes, and therefore do history in slightly different ways. This is not to say that anything goes, nor that there are no standards to which historical work can be held; indeed, we believe that maintaining the highest standards is a *sine qua non* for being taken seriously outside of the community. We do mean to say that there are several modes of reasoning, kinds of evidence, and so on, that can count as historical, and that each should be assessed for what it is.

With this symposium, as we have said, we intended to involve young scholars more actively in the discussion about the future of history of economics, in the hope that their contributions might provide important new perspectives on the place of our discipline, with particular emphasis on its future development. Though there have been many disappointing developments in recent years - and young scholars in particular tend to be painfully aware of the decreasing opportunities available to them - there are some encouraging developments as well. As the response to this symposium suggests, there are plenty of ambitious young people who are willing to do a great deal of work to pursue a career with limited prospects, and who are delighted to take the opportunities available to them. With the right kind of institutional support, their existence suggests that the community of historians of economics may not just survive, but prosper.

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NOTES

[1] This symposium took place at the Annual Meeting of the European Society for the History of Economic Thought (ESHET) in Porto, Portugal, on April 29, 2006. We are grateful to ESHET, especially Pascal Bridel and Annalisa Rosselli, for generously assisting us logistically and financially in the organization of the symposium.

[2] Including Association française des sciences économiques, Società italiana degli economisti, Association Charles Gide pour l'étude de la pensée économique, and Associazione italiana per la storia dell'economia politica.

[3] Colombian, Dutch, Italian, Polish, Portuguese, Swedish, and Venezuelan.

[4] France, Italy, Poland, Netherlands, U.K., U.S.A.

[5] In fact, this symposium was conceived at one of David Levy and Sandra Peart's Summer Institutes for the Preservation of the History of Economics in Economics at George Mason University.

MORE ECONOMICS, PLEASE:

WE'RE HISTORIANS OF ECONOMICS

BY

IVAN MOSCATI

I. INTRODUCTION

Prognosticating the future is probably not our strength as historians: our professional skills equip us better to look back, rather than forward. However, in taking stock of the present situation and prospects of the history of economic thought (HET), three convictions emerged in my mind. The first one can be labeled as disciplinary: I deem that the identification of HET with science studies is not a winning strategy for our field. The second is theoretical: I believe that future HET scholars should have a good background in mainstream economics. The third conviction is pedagogical and regards the education of HET scholars. In analyzing the situation in Italy, I argue that the current PhD programs in HET have some structural problems, and I suggest an alternative strategy to involve and train young talented scholars in HET. Though my discussion is limited to the Italian case, I believe that it may be pertinent also for other European countries, especially France.

The basic belief underlying these three points is that the history of economics should maintain a strong connection, both theoretically and academically, with economics.

II. HET-AS-SCIENCE-STUDIES IS NOT A WINNING STRATEGY

The disciplinary flexibility of HET has allowed for its literature to participate in several methodological genres. One is the history-of-analysis approach, which focuses on the internal developments of economic science. By contrast, science-studies narratives embed HET in more general historical, social, political, economic or academic contexts, and tend to explain the developments of economics as driven by external factors. A third approach is the archival, which is based on the search for unknown documents that may highlight some episode in the HET. Yet another approach is the epistemological, which looks at HET as Alexandre Koyré, Thomas Kuhn or Imre Lakatos looked at the history of physics, that is, as a basis for epistemological investigations. Many other approaches to HET are possible and have been practiced.

I belong to the party convinced that this variety of approaches enriches our field. Therefore, I judge negatively the effort made in the last 10-15 years (to be sure, with admirable intelligence and energy) by Roy Weintraub, Phil Mirowski, Wade Hands, Margaret Schabas and other leading North American scholars to identify proper HET with science-studies narratives, and to marginalize other approaches (especially the history-of-analysis approach) as non-legitimate contributions to the field.[1] I share the need expressed, e.g. by Weintraub (1996), to raise the historiographical standards of HET contributions, and I also believe that papers claiming that some past economist got it right should be submitted to a theoretical journal rather than to an historical one. However, I find any strategy that considers HET-as-science-studies the only legitimate HET to be intellectually impoverishing. I must confess that the “old fashioned”, “thin” histories of analysis written *à la* John Chipman, George Stigler, or for Italian readers, *à la* Claudio Napoleoni, often appear to me more interesting, instructive and reliable than conspiracy “thick” tales *à la* Mirowski. Therefore, I would not like to see the histories of analysis disappear from HET’s top journals and other main publications.

Obviously, I do not deny that social, personal, political, economic or academic factors are important for understanding the history of economics. However, I believe that these factors do not exhaust the whole story, nor are they the most important part of it. Like Keynes, I believe that “the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas” and that “the ideas of economists […] are more powerful than is commonly understood” (Keynes 1936, p. 383). Science-studies scholars may label this position as idealistic and oppose to it a tough-guy, realist view of the world, according to which money and power are what in the end really put things forward. I must say that I find this tough-guy view a bit naïve and philosophically out-of-date.

Moreover, it seems to me that in focusing on economists’ quest for wealth and status, on their political convictions or network relationships, on the institutions that fund them etc., science-studies scholars tend to disregard what the economists actually wrote in their articles and books, that is, to disregard the very primary facts on the ground for HET. Indeed, the history of economic science is not like the history, say, of the French Revolution. In economics the main action does not take place in assemblies, streets, palaces, and battle fields but on paper, in economists’ articles and books. A HET narrative that disregards what happens in these articles and books appears to me incomplete and hence, in spite of its alleged thickness, too thin.

Also from an academic viewpoint, I find the HET-as-science-studies strategy detrimental.[2] First, in Europe as well as in the US, historians of economics work primarily in economics departments. Consequently, they are expected to understand economic theory, to have intellectual interactions with their economist colleagues, to teach some economics courses, and to actively participate to departmental seminars. A young historian trained as a science-studies scholar will probably fail to have such skills, making it more difficult for him to find a job in an economics department. With respect to the departments in history or philosophy of science, I must say that in the last years I have seen few job openings in HET there. Hence, it seems that young scholars in HET would be better off leaving the specialization in science-studies narratives to more established scholars with tenure.

Furthermore, in my opinion the HET-as-science-studies strategy has contributed to the isolation of the HET subfield from the main field of economics, and this isolation has weakened the academic relevance of HET. On the one hand, economists seem interested in science-studies narratives even less than in other forms of HET. On the other hand, when economists write some contribution in HET, they typically write histories of analysis, which science-studies scholars tend to disparage as “celebratory”, “whiggish”, “presentist” stories, in brief, as non-legitimate contributions to HET. Certainly, the diminishing production of HET contributions by economists and the decline in their readership of HET journals cannot be entirely attributed to the disparaging attitude of science-studies scholars. Still, I think that this attitude has somehow helped in driving economists away from HET.

Finally, I submit that the HET-as-science-studies strategy is not beneficial to historians’ attempt to let HET journals be included in the Social Science Citations Index (SSCI) and the Social Sciences Edition of the Journal Citation Reports (JCR), both produced by Thomson ISI.[3] At the time I write (June 2006) the situation is the following. Among the main HET journals, only *History of Political Economy* and *The European Journal of the History of Economic Thought* are listed in the SSCI, whereas no HET journal is included in the JCR for the years 2004 and 2005 (the last available ones). To be sure, the poor presence of HET journals in the Thomson ISI indexes largely depends on the self-referential way they are constructed: since a journal is mostly cited by journals of the same disciplinary cluster, if few or no HET journals are included in the indexes, HET journals get very few citations and tend to be removed from the indexes.[4] However, I also think that HET journals should try to be cited by journals of close disciplinary clusters.

In particular, let us consider the JCR index which is the basis for the notorious Impact Factor. According to the Thomson ISI classification, the 2005 JCR database includes 29 journals of history and philosophy of science, 15 journals of history of social sciences, and 175 economics journals (as just mentioned, no HET journal is listed in the 2005 JCR). I believe that the HET-as-science-studies strategy might increase the citations from the 44 journals of the first two groups, but will decrease the citations from the much more numerous group of the 175 economics journals. If this belief is correct, the net effect of the HET-as-science-studies strategy on the HET journals citations is negative, so that such strategy decreases the chances of HET journals to be included in the JCR and have an Impact Factor higher than zero.

An alternative strategy for increasing HET journals' citations may be for HET journals to publish more review essays on recent developments in economic theory, possibly written by economists. At the moment such reviews appear in the *Journal of Economic Literature*, the *Journal of Economic Perspectives*, sometimes in the *Economic Journal*, and also in the handbooks in economic theory published by Elsevier. The supporters of HET-as-science-studies do not consider these review essays as legitimate contributions to HET. However, as an historian I find reviews of recent developments in economic theory very interesting; I have often used them as a starting point for my historical research, and I would like to read more of them in HET journals.

If we historians were capable of convincing economists to publish some of their review essays in HET journals, and if economists became acquainted to look for a survey, say, on the recent developments in contract theory also in HET journals, probably the citations of the latter would go up. I stress that I am suggesting that HET journals publish *more* review essays, not *only* review essays.

III. FUTURE HISTORIANS SHOULD HAVE A GOOD BACKGROUND IN MAINSTREAM ECONOMICS

I think that future scholars in HET should be well trained in neoclassical mainstream economics, and this at least for three reasons.

First, during the last century, and especially after World War II (WWII), economic science has become first and foremost neoclassical economics. Hence, the developments of neoclassical economics during the second half of the 20th century should be an important topic of research for historians. Yet, mainstream theory employs sophisticated mathematical tools and, in order to understand its problems and developments, one must be familiar with these tools.

Unfortunately, many historians of economics – and I include myself among them – have weak mathematical backgrounds. This circumstance may explain why the historiographical interest in post-WWII mainstream economics does not seem to correspond to the actual relevance of the latter, or is mainly limited to science-studies works. In Italy, and probably in other European countries, the modest attention to post-WWII neoclassical economics also has sociological and political causes. In fact, historians trained in the 1960s, 1970s, and 1980s typically support some heterodox economic school, and this has induced them to disregard neoclassical economics and its history.

I think that future (as well as present) HET scholars should be able to go beyond Keynes and Sraffa, and also beyond Arrow and Debreu, and be capable of writing, for instance, the history of game theory from Harsanyi to Aumann, describing the developments of decision theory from Savage to Schmeidler, or narrating what has happened in macroeconomics after Lucas. In order to do this, future historians will need a good background in mathematics and mainstream economics.

Second, a good knowledge of mainstream economics is important not only for research purposes, but also for didactic ones. I actually see two main reasons for teaching HET to students in economics: HET can offer them a panoramic view of the neoclassical science they are taught (let us call this the "compass function" of HET), and, by making the students aware of non-mainstream ideas, HET may stimulate them to a critical study of economics (the "critical function" of HET).

My perception is that most courses in HET in European and American universities currently emphasize the critical function of HET, while they are less effective in advising students on what has been done in choice and decision theory, general equilibrium theory, game theory, welfare economics and social choice theory, experimental and behavioral economics, growth and cycle theory, the political economy of macroeconomics, etc. in the last 60 years. Yet, in order to be able to offer the students a panoramic and orienting view of the recent developments of mainstream economics, future historians of economics should add to their traditional background in classical, heterodox and pre-WWII neoclassical thought, a solid training in recent and current mainstream economics.

Third, I think that economics historians have much to gain from discussions and other intellectual interactions with their economist colleagues. According to my experience (which I consider not so uncommon) it is often the case that some economist in your department has worked as a theorist on a topic you are now dealing with as an historian. If you are able to interest the economist colleague in your work, the research suggestions s/he gives you and the comments s/he makes on your paper generally turn out to be extremely useful. Furthermore, economists often spot and correct some technical or mathematical error we historians frequently make in writing our papers. Finally, as an historian of economics I find it very interesting to understand how economists think, what their problems are, how they try to solve them, and also, from a science-studies perspective, what academic processes regulate the publications of their papers.

There are probably many other reasons why Mohamed the historian has every interest in going to the economists' mountain. My point here is that this typically is a mainstream mountain: most economists are mainstream economists, so if historians want to interact with them they must be familiar with mainstream theory.

IV. FUTURE HISTORIANS SHOULD HAVE A PHD IN ECONOMICS, EVEN IN ITALY

The previous considerations have a clear implication about the education of future historians: in my opinion they should have a PhD in economics. This is what happens for instance at Duke University, where young scholars with an interest in HET are enrolled in the local PhD program in economics. The situation is different in Italy.

In my country there are two PhD programs in HET. The oldest one is at the University of Florence, and since 1986 it has graduated many of the current Italian HET scholars (and me among them). The other PhD program, which began in 2003, is at the University of Macerata. Both programs are quite small: Florence has three or four new students each year; Macerata has three. These small numbers make it difficult to organize structured courses for the students, and the circumstance that professors are generally not paid to teach in these programs renders the organization of regular lectures and seminars even more problematic. In the last years some troubles with the recruitment of new students have also emerged. As Annalisa Rosselli wrote in a recent survey on HET in Italy, “it appears more and more difficult for both PhD programs to attract young scholars of good quality and/or with a good background in economics” (Rosselli 2005, p. 12, my translation).

In this situation, I wonder whether the PhDs at Florence and Macerata are still the best instrument to involve and train young talented scholars in HET. An alternative strategy would be to convince some larger and more structured PhD programs in economics, with faculty sensible to HET issues, to accept one or two HET students in their programs. The scholarships now used to fund students in the programs at Florence and Macerata could be used to fund the HET students hosted in the economics programs (let us label these hospitable economics programs as “partner PhDs”). More precisely, students with an interest in HET should apply to the partner PhDs as other normal applicants do. If enrolled there, they could apply for a scholarship in HET. The HET scholarships should involve two basic obligations: the HET scholars at partner PhDs should attend some extra courses in HET (maybe organized by the faculties of Florence and Macerata), and write two of their three final papers on a HET topic (the third paper should be in economics).

This educational project, whose details obviously need further specification, may have two positive effects. First, it may attract to HET more “young scholars of good quality and/or with a good background in economics”. In fact, because of the scarcity of HET job openings, for a young talented scholar it is highly risky to enter in the PhDs at Florence or Macerata. This circumstance is likely to produce adverse selection effects among the potential applicants to these two PhDs. On the contrary, a talented young scholar with a strong interest in HET could be stimulated to enter a partner PhD in economics: if for some reason things go wrong with HET, s/he can apply for a position as an economist. Second, young scholars in HET could enjoy better training in contemporary economics. As regards their education in HET, I believe that the extra courses in HET required by their special scholarship could offer to HET scholars an adequate background in the history of economics, as well as a proper historical sensibility that should prevent them from becoming mere apologists for the existing theory.

FOOTNOTES

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[1] Shabas (1992) is generally considered the manifesto of the HET-as-science-studies program.

[2] A point similar to the one discussed in the following lines has been made by Moggridge (1992).

[3] For a critical analysis of the way the Thomson ISI indexes are constructed see Klein–Chang 2004.

[4] On this issue see Weintraub 2006.

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IN SEARCH OF A DEFINITION FOR THE HISTORY OF ECONOMIC THOUGHT†

BY

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ALEXANDER TOBON**

Abstract

This paper highlights some important ideas as regards the character, method, and identity of historians of economic thought. They may be considered as "generalist" economists, who hold knowledge of general theories. This knowledge allows them to establish links between different fields of specialization in contemporary economics. Therefore, historians of economic thought have a precise and permanent position within the economics discipline rather than in the field of history. We argue that scholars in history of economic thought need to focus on general theories.

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†We are grateful for helpful comments to Erik Angner, Ivan Moscati, Eric Schliesser and Paola Tubaro.

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I. INTRODUCTION

The development of economics in the last twenty years has been characterized by specialization inside the traditional schools of thought. Some economic concepts (or some markets) became “sub-disciplines”, in particular in Neoclassical Microeconomics. We refer thus to Labor Economics, Health Economics, Public Economics, Welfare Economics, Environmental Economics, Behavioral Economics, Industrial Organization, etc. It is a disarticulation of the various topics of research in economy. Specialization is reinforced by the integration of the ideas of other sciences: sociology, psychology, philosophy, but also medicine, law, etc.[1]

We do not consider specialization bad in itself. It allows the deepening of scientific knowledge and it is also needed in order to provide a solid basis for applied work in various fields. It becomes bad when it encourages scholars to move away from the fundamental questions of economics and when it generates the constitution of poles of specific knowledge in our science - in the majority of cases so distant from one another that they do not manage to communicate any more. It is the case today.

The fundamental questions of the economic science are those which give a general theoretical explanation of the capitalist system or the market economy[2]. It is well-known that there exist, roughly speaking, four great schools of economic thought which gave the most solid arguments: the Classical theory, the Neoclassical theory, the Marxist theory and the

[1]To simplify the debate, we have omitted the developments within macroeconomics (macrodynamics with microeconomic foundations, etc) and monetary theory (Search models, etc).

[2]Also economic theories of non-capitalist systems may fall into the framework of this debate.

Keynesian theory.[3] As shown by Tjalling Koopmans (1957), theses theories give a very important role to the study of prices as a mechanism of allocation of resources.

Our interpretation of the situation is as follows: the study of these four general theories has become the object of study of the history of economic thought and the studies of the specialized theories (or markets) have become contemporary economic theory. There is a disconnection between the specialized theories and the history of economic thought. Consequently, it is not only contemporary economic theory which becomes divided in specializations or sub-disciplines but also the history of economic thought. But the latter is considered out of the whole of sub-disciplines which conform modern economic theory. This situation marginalizes general theories in the contemporary debate in economics and, consequently, the economists devoted to their study.

This marginalization worries young scholars in history of economic thought. Some discussions about the reasons that have brought about this result were held in France in 2004 by a group of graduate students and junior faculty at the *Pôle d'Histoire de l'Analyse et des Représentations Économiques* (PHARE). Our point of view derives, to some extent, from these discussions. The current marginalization has several consequences such as those which are mentioned in the articles of the *History of Political Economy* 2002 Supplement: the disappearance of general theories in university curricula in economics and the notable decrease in research work dealing with them. Nevertheless, this unhappy reality should not

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[3]We disregarded current heterodoxies who are associated these four general theories, for example Post-Keynesian theories, Neo-Ricardian theories (or Sraffian theories), Circuit theories, Neo-Marxist theories, etc. We recognize the difficulty of current heterodoxies of asserting themselves in contemporary debates in economic theory. One of the reasons of this difficulty is the lack of coherence in its contents.

discourage us in our professional careers. While examining these problems, we aim to explain how our “specialization” may be relevant and fundamental for the study of the economics.

It seems to us that the history of economic thought defined as the study of general theories carries an interest to all fellow economists. This interest can be explained by three elements: (II) character of research, (III) method of research and (IV) the search of an identity.

II. CHARACTER OF RESEARCH: ECONOMIC THEORY

Within our framework as defined above, the first element of interest for all economists is that the history of economic thought can contribute to the development of modern economic theory. The history of economic thought should contribute to the development of general models that might act as reference for all sub-disciplines –i.e. models or analyses that make it possible to establish coherent theoretical bonds between sub-disciplines.

However, in this conception, the history of economic thought is not retrospective[4]. In our view, its role is not limited to describing the process of formation or the progress of economic theory in historical time. In addition, it does not only try to establish links between present and past theories, or to show how certain modern ideas are the heritage of ancients authors.

This was the traditional role of the history of the economic thought since Schumpeter’s 1954 *History of the Economic Analyses*. In the current marginal state of the history of the economic thought, this cannot be its role any more. It is precisely this view of the history of the economic thought that is responsible for the actual state of our profession. In

[4] For example, we are thinking here of Mark Blaug’s work *Economic Theory in Retrospect*.

addition, the character of research in history of economic thought that we will propose is not new. It is the context of marginalization that is new.

Lapidus (1996), in an outstanding analysis of the history of economic thought, calls this character of research “the intensive approach”. He mentions the examples of Piero Sraffa’s approach in his *Production of Commodities by Means of Commodities*. Another example is Karl Marx, especially his *Theories of Surplus Value*. Sraffa starts by reading and publishing Ricardo’s works, which enables him thereafter to build his own theory. Then he uses his own result to clear up Ricardo’s texts. Sraffa’s theory helps to better understand Ricardo’s arguments. Marx discusses with past authors as if they were present, and speaks to them as if they were just sitting in front of him. This approach is opposed to Schumpeter’s who does not dialogue with authors. He seeks the logic of concepts, analyzes instruments, and follows a logical time.

We would like, therefore, to safeguard history of economic thought’s openness to the study of theories and fundamental problems in economics, and its close links to contemporary economic science, particularly at the present time, when economics becomes more and more empirical and less theoretical.

We argue that scholars in history of economic thought need to focus on general theories. The only history of economic thought which is interesting for us is that which can dialogue with any other economics specialties. The history of economic thought, that we are claiming, has generalist purposes. It allows for communication among sub-disciplines in economics. In this sense, we share the opinion Professor John Davis of University of Amsterdam.

III. METHOD OF RESEARCH: HISTORY

One of the biggest worries of economists and paradoxically of those who do history of economic thought is to be regarded as historians. The word “history” produces two effects: fear and shame. Firstly, it produces fear because as economists we do not necessarily have the background and the rigorous and specific working methods of today’s professional historians. In addition, this has never been our intention. Secondly, the use of history as a working method may cause shame to economists, who come from a body of knowledge that claims to be a “science”. It can be seen as an easy exit and as an incapacity *vis-à-vis* the new developments of the mathematical tools applied to our science.

It is common to hear among fellow economists that resorting to history means escaping from the rigor of economic science, and from the use of mathematics. It is also heard that historians of economic thought are those who use the great traditional works of 19^{th} and 18^{th} centuries in their research. The more one goes back through the centuries; the more one is a historian and the less an economist. However, it is necessary to reject with force all these stereotypes that have no bases. The historians of economic thought pay attention to the context in which theories are developed, but their analysis is not historiographic. They make use of historical knowledge to better understand their proposals.

Today’s specialists of Game Theory could use Nash’s 1950 text, thus having recourse to history. Economic Development specialists could base their research on Malthus’s texts, thus also making use of history. This historical method should not be exclusive of the history of economic thought, but should concern all the other sub-disciplines. The specificity of this method is derived from its relationship with the character of research that we have underlined above.

In fact, the use of old articles, works and archives of any period of time, without distinguishing them according to their writing or publication date, gives a permanent validity to them. In the same way, Beethoven's partitions are still modern when a musician refers to them as a source of inspiration to compose new songs.

IV. IDENTITY OF THE RESEARCHER: ECONOMIST, HISTORIAN OF ECONOMIC THOUGHT

Having defined character and method of research in history of economic thought, we now just have to assume the role of historians of economic thought in the discipline as economists. For us, the success of a historian of economic thought does not depend only on his/her quality as a researcher and as a teacher, but also on being recognized as a generalist economist. This recognition is opposed to that of specialized economists. As long as there are negative consequences of specialization, it will be difficult for historians of economic thought to have success as generalist economists.

On the basis of this identification to the heart of our discipline, it is perhaps necessary to introduce a new distinction within those whom the profession usually calls today historians of economic thought. On the one hand, the economists who make an alternative history of economic thought with a character and a method that *differ* from those pointed out here. We refer to those who adopt the method of research known as "extensive" and "retrospective"[5]. On the other hand, it is imperative to establish a distinction from those that make history of economics as *history of science*. Roy Weintraub (1996), member of the *History of Economics Society,* defined well the object of this sub-discipline. In this approach, historians of economics, like an historian of science, concentrate on specific fields (history of game theory, history of monetary theory, history of econometrics, etc.), by stressing their study of the

[5]The extensive method refers to the treatment of old economic problems with use of old tools. The retrospective method, as the one already defined, deals with old economic problems in a logic time. See Lapidus (1996).

context of economic theories (sometimes even more than the theories themselves). It seems to us that the title of history of economic *thought* and history of economics are not synonymous and that this confusion is dangerous for the two sub-disciplines. Finally, the historian of economic thought must take part directly in the present debates in economic theory.

V. CONCLUSION

Our main purpose, already stated, is to make feel the need for a general theory in which all specialties could meet in a coherent way. Sooner or later, this will be the spontaneous need in economic science. It is through this participation, without ambiguity, that it will be possible to fight against marginalization and to avoid the disappearance of the history of economic thought in departments of economics at universities.

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THE IMPORTANCE OF THE ANALYSIS OF ECONOMIC PROBLEMS FROM THE PERSPECTIVE OF THE HISTORY OF ECONOMIC THOUGHT: THE EXAMPLE OF THE CONCEPTION OF THE HUMAN BEING IN ECONOMICS

I. THE SIGNIFICANCE OF USING DIFFERENT APPROACHES TO ECONOMIC PROBLEMS

Economic problems fall within the field of interest of many sciences, being analyzed by, among others, psychology, sociology, anthropology, philosophy and human biology. Each of these sciences examines a given problem from its own perspective. They focus, however, on very detailed or specific issues, such as analyzing concrete human decisions or behaviors, measuring specific neural conduction in the brain while making economic decisions, or exploring the impact of societies' economies on early development. Economists very seldom use this kind of knowledge since economics always abstracts from the complications of real economic problems by isolating certain features and examining them from a very specific economic perspective. While the achievements of other sciences can in most cases be treated as an inspiring element which can change the way of thinking about the essence of economic phenomena, it is hardly possible to supplement an economic theory with the discoveries of other sciences in this field. Economists are aware of the complexity of real economic problems, but for the sake of the science they practice, they use models emphasizing a few features that seem to determine economic life.

II. THE HISTORY OF ECONOMIC THOUGHT PROVIDES A COMPREHENSIVE EXAMINATION OF ECONOMIC THEORIES

The history of economic thought provides us with a general picture of a particular theory. It demonstrates how, and for what reasons, certain ideas have been discussed among different schools of economics in the course of the development of economic thought. It shows how prevailing philosophical currents and discoveries in other sciences influence economic conceptions through intellectual fashions or ideologies. This knowledge helps to understand the basis of the emergence and development of economic ideas.

The historical perspective is particularly important for economists from underdeveloped countries. The evolution of economic theory follows the development of a particular economy. Theories that are formed in developed countries, and capture economic interactions there well, often fail to explain economic phenomena in other economies. The historical perspective helps to examine the relationship between an economic theory and the conditions of the economy that inspired its development. It shows the multitude of views on problems considered by economists from countries on various stages of economic development. It helps not only to understand the meaning and significance of assumptions made by a particular theory, but also its validity.

The lack of a perspective stressing the context of creation, the origin, and the limitations of a particular theory can lead to very serious consequences. The acceptance of a particular economic conception often determines the choice of concrete economic policy and the way in which it is implemented. The failure of economic reforms imposed by international organizations on Central and Eastern Europe in the 1990s demonstrates the tragic consequences of applying economic policy based on contemporary mainstream economics to solve the problems of underdeveloped countries. The same reforms that were successful in developed economies not only fail to bring about expected positive outcomes in other countries, but often also contribute to more serious economic crises.

The historical perspective stresses the lack of universality arising from the historical development of economic theories that reflect the conditions of particular economies. This type of perspective can be particularly fertile for economists who seek to model economic processes in underdeveloped countries by helping to overcome the ostensible universality of the neoclassical paradigm.

III. THE CONCEPTION OF THE HUMAN BEING IN THE HISTORY OF ECONOMIC THOUGHT

The analysis of the conception of the human being in economics from the perspective of the history of economic thought provides a good example of the creative and essential contribution of the history of economic thought to understanding economic problems.

The conception of the human being is a constantly recurring topic in the economic literature. Many books, papers, and conferences on the history of economic thought, as well as on economics devoted to this subject, focus on different aspects of this problem. In many cases the reason that scientists become involved in analyzing this problem is due to evident differences between real human behaviour and the model of it accepted by mainstream economics. It provokes attempts to supplement the theory with elements that will bring the model closer to reality, or to prove that the discrepancies between the model and reality disappear with a deeper analysis. Attempts to elaborate existing conceptions of the human being in economics are often also triggered by discoveries or observations of applied sciences connected with economics, such as marketing, management, finance, the theory of insurance, the theory of investment, and the theory of decision-making. Interest in the nature of *homo economicus* has also been stimulated by logical discrepancies that become evident through attempts to combine the achievements of economists adopting differing conceptions of the human being. There have also been attempts to reconcile the conception of the human being in economics with the idea of man in contemporary currents of philosophy, ethics, and, because of the Western cultural basis of economics, Christian ethics.

The historian of economic thought is particularly interested in the changes of conception resulting from developments in dominant schools of thought – that is, from the ageless discussion among different currents of economics – since they largely concern fundamental features of the human being in economics. The concept evolved in the classical-neoclassical school, which supposes the universality of economic theories to explain problems facing real economies, as well as in institutional currents, which stress the importance of context in the application of economic tools. The character of changes in this conception within schools, which accept different paradigms of economics, makes the problem particularly interesting.

A response to the question of the anthropological basis of a social science such as economics is essential. Without a conception of the human being, any description of an economic system is incomplete. The understanding of the basis of human behaviour in economic life is of fundamental significance to any theoretical model of the economic system. The effectiveness of the state's shaping of the economic space – conducting economic policy, establishing economic law, creating economic institutions – which are to some extent based on economic theories, also depends on the proper understanding of the key element of the economic system, namely, human behaviour.

As for the anthropological basis of economics, the conception of man is very specific, taking into account only his economic activity. This conception represents an attempt to distinguish universal characteristics of man that determine his behaviour in economic life in order to recognize economic laws. The perspective from which the historian of economic thought examines conceptions in economics permits us to discern how different currents of economics attach different weights to the conception of man. This conception has been particularly important for neoclassical economists; however, because of the significance of the conception, all currents of economics must make some assumptions concerning human nature. In order to emphasize those characteristics of man that have fundamental meaning for shaping economic laws, the conception of man is reduced to the most important features in all currents of economics. The lack of uniformity of the conceptions results from the differences in the characteristics of the human being recognized as the basic ones by different schools of economics, or from differing interpretations of the nature of human behaviour.

There are a number of reasons why the perspective of the historian of economic thought is essential for pointing out differences in the anthropological conceptions in economics. There is a popular view shared by economists and specialists from related sciences that the only anthropological approach in economics is the model of *homo economicus* characteristic of the classical current of economics. In view of the frequent critical opinions of the conception of human being in economics, it is worth stressing that *homo economicus* is not the only one; in the history of economic thought, other anthropological conceptions are also strongly represented, even though at present they seem to be less popular. Therefore, it is important to analyze the conception of the human being of many economists, from which we can distinguish a number of well defined approaches.

Many authors emphasize that the model of man developed by the classical school has nothing to do with real human behaviour in economic life. This conception has been accused of being completely unrealistic. Therefore, it is important to answer the question of the extent to which this model enables us to understand the nature of the human being in economic life, whether it is possible to maintain the assumption of the universal rationality of the human being when economic and political choices have been most effectively influenced almost exclusively through non-rational motives, by appeals to emotions or instincts.

The *homo economicus* model cannot be used to represent the economic behaviors of people from cultures outside of the Western world. Some scientists argue that a new model of the human being should be formulated that better explains human nature in the Western world as well as for other cultures. Offering an opposite conception to the one derived from the Enlightenment tradition would undoubtedly bring an essential contribution to this discussion. The impact of the conception of the human being on the theories of a particular economist is also an interesting aspect of the analysis. There is a distinct division of these conceptions along the lines of division of different schools of economics. It strengthens the argument that the anthropological assumptions influence the vision of the functioning of the economic system.

From the perspective of the history of economic thought, four basic conceptions of the human being can be distinguished: Classical, Neoclassical, Marxian, and Heterodox. The conception of the human being in economics generally serves one of two functions: it is an important supplement to or a basic element of the theory developed or created by a given economist.

For classical economists, for example, the conception of man is an additional element which helps to explain macroeconomic phenomena. The movement of capital from less to more profitable branches of industry or the anticipation of inflation have been explained as outcomes of rational human decisions, based on objective knowledge about the world, and driven by financial self-interest. The division of labour and its effectiveness have been treated as the product of the innate individualism of the human being. Rationality, ascribed to human beings by authors belonging to the classical school of economics such as Adam Smith, Jean Baptiste Say, John Stuart Mill, and also in the 20th century to some extent by Robert Lucas and Milton Friedman, is based on the assumption of the objective recognition of the world, a recognition untouched by the human psyche. This conception arose at the end of the 18th century in the age of the Scottish Enlightenment, and clearly connects with the philosophical legacy of this epoch. The authors who adopt this conception assume that man is able to objectively perceive the world (or to act “as if “ he could), and act in a logical way to obtain his own material gains according to his knowledge. Classical economists therefore strongly emphasized the significance of education, which allows for the realisation of the potential for objective rationality in all people.

For neoclassical economists, the conception of man has been a basic element of economic analysis. According to the doctrine of cognitive individualism that they adopted, the motives for individuals' acts have been recognized as the foundation of both economic processes and their analysis. They created a conception of the individual who, like Robinson Crusoe, isolated from the world, makes decisions using scarce means to obtain the multiplicity of his goals. They believed that understanding the logic of these decisions would lead to an understanding of social economic laws. They rejected the classical assumption of the significance of recognizing objective reality, viewing human perception of reality as subjective. The neoclassical conception of man created by Lionel Robbins and developed by William Stanley Jevons, Alfred Marshall, and Gary Becker, assumes that each agent seeks to maximize his own utility. Utility – the attainment pleasure or satisfaction – is not limited to material gains. The human being is not able to obtain perfect knowledge of the world around him, but he is equipped with a mechanism of consequent logical calculation which enables him to reach his predefined goals. Neoclassical rationality is restricted to the ability to order goals and to act in a logical fashion on the basis of available information.

The neoclassical conception differs from the classical one in the attitude toward human social relations. The classical economists highlight the significance of human individualism for economic life, whereas the neoclassical authors stress the social nature of man. They accept that the human need for social contacts or belonging to a community influences all spheres of his life.

The heterodox conception of the human being rejects the assumptions of both the classical and neoclassical schools concerning human rationality.

Such authors as Friedrich List, Max Weber, Thorstein Veblen, John Kenneth Galbraith, but also John Maynard Keynes, Joseph Schumpeter and contemporary experimental economists, have argued that human behaviour is seldom an outcome of rational thought. Human behaviour is determined by a wide range of factors such as instincts, spirit, social milieu, tradition, habits, and sometimes reason. For heterodox economists, the conception of man plays an essential role. They contradict the vision of the rational, self-interested individual, claiming that the classical conception is Utopian. They focus on the true character of human behaviour: the tendency to conform to traditional patterns of behaviour; the unusual effort required for rational decisions; and the psychological conditioning of man, which induces instinctive behaviors subordinate to emotions.

Economists of the historical-institutional school have highlighted the influence of the culture and the degree of economic development of a given country on the economic behaviour of its inhabitants. They question the universality of economic laws.

The economists who have employed this concept claim that people fundamentally differ. Only a small fraction of society develops rational thought. Those who are capable of it, according to Joseph Schumpeter, play an important role in society. In a capitalist economy, they become entrepreneurs.

The division of the conceptions of the human being in economics mentioned above does not exhaust the wealth of such ideas in the history of economic thought. It does, however, outline a structure into which other conceptions can be placed. Those of Friedrich von Hayek and Herbert Simon, for example, do not fit within the frame of any of the conceptions presented above, but can be situated between them.

IV. CONCLUSIONS

There are many conceptions of the human being in economics and many perspectives that can be used to analyze them. The historical perspective enables us to identify the essential currents of thought on this problem. It shows how different conceptions in economics interweave and interact with each other, as well as how closely they correspond with the achievements of other sciences and philosophical currents characteristic of the given epoch. Such a perspective permits us to outline the heterodox conception, which emphasizes social features of the human being and non-rational motives of behaviour. It is particularly important in a situation in which economics learns more and more from other social and human sciences. In addition, experimental economics has demonstrated that non-rational motives play an important role in the process of economic decision-making. It also illustrates the continuity of the criticism of the classical-neoclassical economic paradigm. One interesting feature of the heterodox conception of man is not its cohesion, but rather its perennial recurrence as a critique of classical and neoclassical assumptions.

The historical perspective identifies the evolution within classical-neoclassical economics of the understanding of human goals and the attitudes toward the social environment in economic life. This evolution is particularly apparent when contrasted with the dominant conception of man in contemporary mainstream economics, which combines the classical and neoclassical conceptions (rejecting man's non-material goals).

The perspective of the history economic thought depicts the context of particular economic conceptions, thereby helping to understand their limitations.

Milton Friedman wrote that it is not important whether a conception is true, only whether it is possible to make use of it for verifiable forecasts. The ability to forecast rather than conformity with reality justifies a conception.

The heterodox description of human nature agrees more closely with contemporary psychological and sociological knowledge than does the model of man developed by the classical-neoclassical school. It has been particularly useful for explaining economic processes during turbulent times, or in countries outside of Western civilization. In such cases, the influence of culture, spirit, institutions, collective psyche, and emotion on economic processes is evident. The classical and neoclassical models, on the other hand, are most successful in explaining economic processes in developed Western countries. In these countries, as a result of evolutionary processes leading to the prevalence of such conditions, styles of life and work have been promoted, and myths and institutions dominate, that lead people who imitate them to behave as if they were rational. However, the mechanical selection of means to obtain a particular goal, as opposed to reflection on the problem at hand, introduces essential limitations. It explains both the impression that in many cases people seem to behave rationally, as well as the irrational susceptibility of man, for example to advertisements or to the manipulations of the media in seeking to impose certain lifestyles.

The historical perspective also helps to identify the basic assumptions that lead to particular economic policy recommendations.

The opinion that the role of the state in the economy should be minimal is a consequence of the assumption that people are perfectly rational individualists and that the multiplication of private fortunes leads to social harmony.

The authors who emphasize the influence of non-rational motives on human decisions are more inclined to treat the government as important, suggesting that it should coordinate the economic system, guide the direction of development, and enable the establishment of institutions supporting social development.

This paper has presented a division of the conception of man from the perspective of the history of economic thought. It differs from those offered by economists, psychologists, or philosophers, but such a general view deepens the knowledge of economic concepts in a fundamental way by: demonstrating the variety of different approaches to particular ideas in economics; identifying the origin and evolution of economic conceptions, helping to understand their contexts and limitations; and highlighting the consequences for economic theory and policy of accepting certain philosophical assumptions.

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History of Economics or a Selected History of Economics?

Nuno Pedro G. Palma

Abstract:

The goal of the present paper is twofold: first, to argue that research on the history of economics to can be important to modern economics, and second to criticise the way this kind of research is done today. I will argue that today's history of economics is a selected history of economics, because most research on the history of economics is not about contemporary and mainstream issues (or when it is, it is written in opposition to them). This must change if the work of historians of economics is to be taken seriously by mainstream economists.

Keywords: History of economics, mainstream economics.

1. INTRODUCTION

When asked about the importance of research on the history of economics to modern economics, most mainstream economists will argue that it is not important at all. Things written more than ten or twenty years ago are usually assumed to be irrelevant, out-dated, or incorporated into more recent work. The goal of the present paper is twofold: first, to argue that research on the history of economics to can be important to modern economics, and second to criticise the way this kind of research is done today. I will argue that more research on the history of economics should be done along the lines of contemporary mainstream economics. This is not what happens today, when most research on the history of economics is either not about contemporary and mainstream issues, or when it is about contemporary and mainstream issues, it is written in opposition to them. For this reason, today's history of economics can be characterized as a selected history of economics. I will argue that it would be important for this to change, and that this change would enhance the - often lacking - communication between historians of economics and mainstream economists.

In sociology, the "Great Masters of the Past" rule the world. Durkheim, Weber, and others are constantly re-read and re-interpreted by sociology students. In sharp contrast, a physics student does not know where calculus comes from, historically – he just knows how to use it. Economics is half way between, being that the history of economics is sometimes taught at the undergraduate level, but is virtually disappearing from graduate curricula, and historians of economics are on the defensive[1].

So what is the real importance of the history of economics to economics? Long gone are the times of glory of Jacob Viner, Joseph Schumpeter and Lionel Robbins, and it is often heard that the history of economics is dead, at least dying in the mainstream economics departments. Some view this as a good thing. Economics is becoming increasingly scientific, they say, and physics does not know its own history. Does this means that just as physicians regard the history of physics as irrelevant, and something to be studied in a different department such as history, economists should regard the history of economics as irrelevant? I believe not, and the first goal of the present article will be to explain why not (section 2).

The second goal of the present article will be to analyse the present practice of research on the history of economics (section 3). I will argue and present evidence showing that historians of economics have been devoting little time studying contemporary mainstream economics. I will then present some possible reasons for this negligence (section 4). The conclusion (section 5) will discuss some consequences of this fact, and suggest a different course for the history of economics.

2. REASONS FOR THE RELEVANCE OF THE HISTORY OF ECONOMICS

Many reasons have been given for the importance of the history of economics, and at least some of my forthcoming reasons have inevitably been given somewhere else. Their purpose here is however to support a subsequent argument. It is important to note that the reasons that I shall present are theoretical; they argue why the history of economics is important for economics; I do not mean to suggest that historians of economics always do behave according to these reasons in reality.

The first reason why the history of economics is relevant for economics is that it is important not to re-invent the wheel. It is critical not to underestimate what not re-inventing the wheel represents: true scientific progress can only exist if 1) the relevant part of past knowledge is fully taken in account when producing new knowledge, and 2) new knowledge is truly new. Historians of economics should be able to systematise the past, and to judge on what constitutes *the relevant part* of past knowledge. This is very important, as historians of economics play a potentially crucial part on the selection process that determines which work deserves the limited attention of later scholars. Let's take the work of Frank Ramsey as an example. His work on optimal economic growth [Ramsey (1928)] was clearly economics ahead of its time. Although it was recognized by some (notably Keynes) upon its publication as an important contribution, Ramsey's work was subsequently forgotten[2] until it was rediscovered and restated in the sixties. As it is amply known, Ramsey's story is not the only case of its kind. It is the responsibility of historians of economics that this sort of situation does not occur. For this to happen, it is necessary that historians of economics have the necessary tools, namely mathematical ones. A final note I wish to make on this point is that, as argued in Kenneth Boulding's (1971) essay "After Samuelson, Who Needs Adam Smith?", it is possible that there are arguments and insights in past authors that are not fully incorporated in present day analysis. In this case, research on the history of economics can bring new ideas to contemporary economics.

A second reason why the history of economics is important to modern economics is that, in an age of increasing specialization, historians of economics, who are generalists by nature, may be able to "see the big picture". With this I mean two different things. First, historians of economics should be able to look at the present in historical perspective. Modern research consists on a huge flow of information on which fashion plays an enormous role. Analysing recent research in historical perspective helps realizing what the really important questions are. Second, historians of economics (generalists by nature) may piece together different branches of contemporary economic theory[3]. Because historians of economics have often broader interests than mainstream economists, it is even possible that they also intersect topics from different disciplines with economics, for example, law, biology or psychology. I stress that historians of economics are generalists, so their contribution should not focus on very specific details, but on broad theories [see Betancourt and Tobon (2006)].

A third reason is that small "m" methodology, often done by historians of economics (in part due to the last reason advanced) is very relevant, and can even eventually influence mainstream economics. A concrete example of this is Lionel Robbins' early methodological essay.

Robbins' *Essay on the Nature and Significance of Economic Science* [Robbins (1932)] redefined the scope of economics and was a defence of abstract theorizing as well as a critique of marshallian intuitionism. In the essay Robbins attacked the value judgments on which measurable utility rested. His program was soon after taken up by Hicks and Allen, thus influencing mainstream economics in a significant way [see Backhouse (2003, p.311-12)].

It is certainly not irrelevant to note that the contribution of Robbins was part of a larger discussion that was in due course in his time, and his work could never have been published had he not a profound knowledge of contemporary mainstream theory. This point raises a clue to contemporary methodologists about the necessity of analysing recent theory. Some may say that this reason (the importance of small "m" methodology) is only a reason for the relevance of economic methodology, and not the history of economics. However, as I already mentioned, economic methodology is very often done by historians of economics, and cannot be separated from it, as the same classification in the JEL codes (B) suggests: "Schools of Thought and Methodology". And the fact is that Robbins was indeed a historian of economics.

The forth and last reason for the importance of the history of economics that I shall present is that positive economics is always based on a normative choice of assumptions. This has been recognized before, but I suspect its complete implications are not always taken in account when considering the work of historians of economics. This reason is not independent of the third reason advanced: a large part of important methodological work is to identify the normative origins of positive results. Because any simplification inherent to the construction of an economic "model" implies disregarding some issues, the choice on which issues to disregard and which to place emphasis in is always a normative one, and the methodologist has here an important role in pointing out exactly which results depend on which assumptions[4]. Note that this reason has consequences for first reason advanced: the historian of economics has to bear in mind this methodological consideration when performing the selection process that decides what stays on to the future as a legitimate and important contribution to economics.

Some people have argued that studying the history of economics can "trap" someone in old ideas, impeding a scholar to develop new ideas. There may be some degree of truth in this, however I cannot see why it should apply more to the history of economics than to any other area of economics: I can also think of people being trapped in the literature of public finance, for example.

3. PRESENT RESEARCH ON THE HISTORY OF ECONOMICS

The main "policy" objective of the present essay is to argue that more research on the history of economics should be done along the lines of contemporary mainstream economics. This is not to despise the work on Sraffian, Marxist, Post-Keynesian, Institutionalist, and other heterodox theory, nor the work about the not-so-recent past. But face it, just as all that work is important, current mainstream work is important too. Even if we adapt the "dead for thirty years" criterion, which is on itself subject to critique, more research should be done about the work of, say, Nobel prize winners. When the editors of *The European Journal of the History of Economic Thought* asked several Nobel prize-winners about the most important contributions of twentieth century economics, most answered things like game theory, macroeconomics, and the development of general equilibrium theory. Arrow (2001), for example, answered game theory and imperfect competition, the economics of information, general equilibrium theory, national income accounting, and the emergence of economic growth and development as a field of study. Almost everyone recognizes these are important areas of research, however it is difficult to find a single paper on the history of economics about any of these themes, at least not about their early history.

The following table shows some statistics about the articles that have been published in the three main journals of the field from the time the last series starts (1993) until the end of 2005[5]. The sections that follow do not wish to represent any special division of periods by schools of thought, but only to provide a mere temporal division for the sake of analysis.

	History of Political Economy		J. of the History of Ec. Thought		E. J. of the History of Ec. Thought		Total	
	Articles	Rel. nr. of articles	Articles	Rel. nr. of articles	Articles	Rel. nr. of articles	Articles	Rel. nr. of articles
Pre-Classical	67	11.1%	15	5.5%	36	13.6%	118	10.4%
Adam Smith[6]	35	5.8%	16	5.8%	21	8.0%	72	6.3%
1770-1870	74	12.3%	37	13.5%	42	15.9%	153	13.4%
1870-1900	36	6.0%	35	12.7%	28	10.6%	99	8.7%
1900-1945	101	16.8%	49	17.8%	37	14.0%	187	16.4%
1945-today	30	5.0%	16	5.8%	16	6.1%	62	5.4%
Cross-Section[7]	57	9.5%	34	12.4%	35	13.3%	126	11.1%
Commentaries/Replies	62	10.3%	22	8.0%	11	4.2%	95	8.3%
Other/Methodology[8]	139	23.1%	51	18.5%	38	14.4%	228	20.0%
Total	601	100.0%	275	100.0%	264	100%	1140	100.0%

Table 1. Articles in HET Journals

When browsing these journals, I found it intriguing how many articles there are on a series of pre-classical authors, notably on Quesnay, on Smith and Malthus, and even a series of early twentieth century heterodox authors, and above all how little on contemporary (and late) economics Nobel prize-winners and other mainstream[9] authors from the twentieth century. This was the reason that took me to write the present paper. I shouldn't leave here the impression that I think that all that research is unimportant. I do think it is important to pay attention to heterodox theory, as well studying the not-so-recent past. However, I believe that much of the importance of this relies in the impartial comparison with contemporary mainstream theory, if this kind of research is done. And in fact, I consider that the relative attention given by historians of economics[10] to contemporary (especially mainstream) economic theory is simply too small. If the 5,4% dedicated to 1945-today does seem a small amount, it gets only worse when we take in consideration that 1) some of these articles are on heterodox authors 2) there are almost no papers at all about any period after 1970 and 3) the small amount of work that is indeed about contemporaneous mainstream theory, is often in opposition to it. Let's take for example an exception to the first two premises - Sent's work on the history of rational expectations (Sent 1999) - it is not an exception to the third[11]. There are indeed very little exceptions to all three premises. Some good examples do exist, mainly in the "cross-section" section, but they remain a small fraction of the cake.

In a general way then, what it seems to come out from the evidence is that analysing recent mainstream economic theory is not what historians of economics have been doing. Historians of economics seem to be willing to move towards the future much slower than they should be. In recent issues of the journals, it is possible to start seeing a small number of articles on Patinkin, Samuelson, Friedman, and the early history of game theory, for example, but those periods are generally the most far the articles go. Also, articles tend to focus a lot more on one or a small number of individuals than on schools of thought and comparison between them. It would be important that we start seeing articles considering the history of, say, social choice theory, the new classical school, the new keynesian school, or endogenous growth theory, just to name some examples.

4 REASONS FOR THE NEGLIGENCE OF CONTEMPORARY THEORY

The two last sections concluded that historians of economics devote little relative attention to contemporaneous mainstream economic theory, and that when they do, it is almost always in opposition to it. So this begs the question: Why so? Why is it that there exists more research articles on Adam Smith than on all the history of Post-World War II economics taken together? This is obviously a complicated matter that involves a number of different issues. I will divide the question in two parts: 1) Why do historians of economics devote little attention to contemporary mainstream theory? and 2) Why is it that when they do it is in opposition? The answers are clearly related, but nevertheless deserve separate attention. The answer to the first question certainly involves something which can be called the “fog effect”. This means that if a given phenomenon is close to someone in time, it is harder to analyse historically (it is probably harder to analyse at all). But there are other possible reasons. One is related to the trends of publication in specialist journals. As the previous section demonstrated, there are very little articles on contemporary authors in the journals. This fact may imply that further articles on contemporary authors are more difficult to write and publish, and may be a risky career strategy. For example, the fact that there are lots of articles on Adam Smith implies that further articles on Smith may be easier to write and to publish than an article on Robert Lucas. Also, the mathematical apparatus of modern mainstream theory implies that there are barriers to entry in the study of contemporary theory that do not exist in studying other periods. There is one last possible reason. Many people argue that contemporary theory is difficult to analyse because a historical reconstruction would require access to manuscripts that are not currently available. Even if this is true in the short-run, it does not however rule out the role of rational reconstructions. Rational reconstructions have the benefit that they do not require the analysis of manuscripts, and their appearance is not only important on itself, as it can be complementary to the later appearance of historical reconstructions when the necessary papers become available.

The remaining unanswered question is why are the few articles written about contemporary mainstream work in written in opposition to it? This question has certainly to do with the problems discussed by Lodewijks (2003). It turns out that, for several reasons, the history of economics is often an instrument for the defence of heterodox economics and the critique of orthodox economics. This, *per se*, is quite unacceptable, as the historical analysis can have normative consequences, but must be, as far as possible, ideological free.

5. CONCLUSION: CHANGING THE HISTORY OF ECONOMICS

Historians of economics and mainstream economists seem unable to communicate. On the one hand, historians of economics often lament that mainstream economists do not pay attention to the intellectual history of their own discipline, and that publications on history of economic thought journals carry little weight. On the other, mainstream economists often dismiss the history of economics as irrelevant and unnecessary for current research. It is my opinion that a healthy co-existence and collaboration can and should exist between historians of economics and mainstream economists. This is independent of if they work in the same department or do not. The main reason for the exclusion of historians of economics from mainstream departments is practical. The journal rankings that determine the departmental rankings, and consequently their funding, attribute little weight to history of economic thought journals, as they receive few citations from the “top” journals. Historians of economics often complain that the editors of mainstream journals are not open to the publication of articles on the history of economics[12], and so that there are relegated to specialist journals, which receive fewer citations and attention. Yet publishing in mainstream journals is not the only way that historians of economics can gain attention. There is another way, and this is to increase the citations of specialist journals in mainstream journals. For this to happen it is necessary that historians of economics analyse issues that are of interest to mainstream economists. And do they? In the second part of this essay I discussed some reasons why the history of economics can be important for modern research in economics. I stressed the idea that these reasons where theoretical. In fact, most of the reasons given there do not correspond to the work of historians of economics, because, as it was shown in section three, they generally do not analyze contemporary mainstream economics, at least from an impartial position. This means: 1) they aren't able to judge in historical perspective on the originality of new knowledge, 2) they cannot "see the big picture" 3) they do not perform relevant small "m" methodological work, and 4) they cannot judge on the normative origins of recent positive results. As so, their work is of little interest to mainstream economists.

Blaug (2001), and others, noted that the history of economic thought is held in low esteem by mainstream economists. Yet, there is no reason to believe that historians of economics have nothing to teach to mainstream economists, or that they won't listen[13]. I have often heard and read historians of economics lament that mainstream economists do not pay attention to the history of economics. The truth is that if they want attention, it is historians of economics that must change their attitude. To earn the respect of mainstream economists, work in the history of economics should not be intrinsically ideological, cannot eschew formal methods, and must move beyond the frontier of what is usually considered to be the history of economics. The "fog effect" cannot be an excuse forever. Let's face it: a day will come when Adam Smith has nothing new to say. But Robert Lucas has.

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ACKNOWLEDGEMENTS

I would like to thank, first and foremost, José Luís Cardoso. Without his support this paper would not have been written. I would also like to thank Paulo Rosário for comments on an earlier version. Thanks are also due to all participants at the "future of the history of economics: young scholar's perspective" symposium at the 10th ESHET conference in Porto, Portugal, where an earlier version of this paper was presented, especially to Erik Angner, Roger Backhouse, Axel Leijonhufvud, Ivan Moscati, Eric Scliesser, and Paola Tubaro. For all of the above, the usual disclaimer applies.

NOTES

[1] See, for example, Blaug (2001), and Weintraub (2002).

[2] Two reasons are possible for this negligence that took more than thirty years. They are, first, that Ramsey used advanced formal methods, which were unusual for his time and second, that the world depression that would begin in the following year (and that was already underway in Great Britain) diverted the profession's attention towards sort-run business cycle problems and not the long run issues with which Ramsey's article was concerned. None of these reasons was longer valid in the sixties.

[3] This is not to disregard the importance of specific/specialized knowledge, which also plays a part in the history of economics.

[4] Think again about Robbins' *Essay*: it was an analytical critique of the logic and appropriateness of the assumptions underlying economic propositions.

[5] *The European Journal of the History of Economic Thought* started on 1993, and my counting starts on this year, making a total of 12 years of analysis. A longer counting would decrease the relative weight of recent contributions, which are the most important to analyse. The table excludes book reviews. Articles on economic history are included in the relevant sections.

[6] This section includes articles on Adam Smith (even if jointly with other authors). It excludes general articles on political economy.

[7] This section includes articles concerning more than one of the other sections. For example, an article on both Ricardo and Sraffa.

[8] The relatively large size of this section is explained by the inclusion of the papers discussing the history of economics as a discipline.

[9] For the sake of concreteness, let's consider mainstream theory current graduate microeconomics, macroeconomics, and econometrics as taught in the top departments.

[10] At least as measured by the current measure ,i.e., number of articles on scientific journals.

[11] Louçã (2004) is a further example.

[12] It is not completely unusual to see articles on the history of economics in mainstream journals such as the *Quarterly Journal of Economics*, however these are written by mainstream economists and inevitably never cite specialist history of economic thought journals. Many of these articles focus on recent issues.

See Blanchard (2004) for a recent example. I leave here the hypothesis that they do not cite specialist journals, not because of some sort of dismissal of research on the history of economics, but simply because due to the fact that historians of economics do not write about recent issues, they have nothing to draw on.

[13] It is not difficult to imagine a world where mainstream economists consistently cite works on the history of economics, so as to provide a historical pedigree to their own theories.

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[T]he most reckless and treacherous of all theorists is he who professes to let facts and figures to speak for themselves, who keeps in the background the part he played, perhaps unconsciously, in selecting and grouping them, and in suggesting the argument post hoc ergo propter hoc"—Alfred Marshall, quoted as epigraph to *A Monetary History of the United States 1867-1960*, Milton Friedman and Anna Schwarz Jacobson.

Philosophy and a Scientific Future of the History of Economics

I Summary and Introduction

In this essay, I argue that economists have reasons internal to the way that evidence works in the sciences to re-discover the importance of the history of their own discipline. For it is a constitutive element of science – here conceived as an ongoing research practice (as opposed to as an explanatory activity) – that evidence is never discarded forever and is thus historical in nature. Moreover, while drawing on the history of economics and the history of physics, I argue that the history of a discipline can be a source of important evidence in ongoing inquiry. Along the way, I attack a too rigorous distinction between the history of economics and economic history. I distinguish my approach from two closely related positions that emphasize learning from the past for scientific purposes. In my conclusion, I argue that that if economics departments continue to discard the history of economics (and economic history), one of its natural homes is in (history of) philosophy departments, where it can be nurtured among many other theoretical enterprises potentially relevant to the sciences.

II On the Role of History in Scientific Research

Since Thomas Kuhn's *Structure* there has been an explosion of interest in the history of science. Yet, this has not resulted in a better understanding of the significant evidential role of the history of science in ongoing scientific research. This lack of understanding is not surprising because science textbooks and graduate training in the sciences continue to prefer to emphasize either the timeless nature of scientific truth or some natural path toward current science. Moreover, philosophic interest in the rationality of science has tended to emphasize the so-called context of justification and, thus, rational reconstruction and with it a focus on explanation. In contrast, much recent history of science has shown more interest in cultural history, a strain especially important to historians of economics who have wished to introduce departmental and methodological affiliation with science studies and history of science programs (see, especially, Schabas 1993 and 2002).[1] While these have produced many fine studies of the context(s) and even practice(s) of, say, measurement, standardization, modeling, and precision within the sciences, they have not been especially interested in the substance of evidential arguments within the practice of science as an ongoing research process—one in which theory is (to speak with Marshall) a 'research engine.' Here I call attention to a neglected way in which the history of a discipline matters to the practice of science, especially understood as it pertains to ongoing, empirical enquiry.

Of course, Kuhn is correct to note that "systematic reconstruction" of the past is pervasive in the sciences and science textbooks and that they often "badly misrepresents

[1] Margaret Schabas (1992) "Breaking Away: History of Economics as History of Science, HOPE 24: 187-203, and also Margaret Schabas (2002) "Coming Together: History of Economics as History of Science" in *The Future of the History of Economics*, Annual Supplement to Volume 34 *History of Political Economy*, edited by E. Roy Weintraub (Durham: Duke University Press), 208-225.

the past." It appears that even Kuhn's phenomenal and enduring success has had little influence on the practice of textbook writers and (under-) graduate education within the sciences. But it does not follow that "the sciences cut themselves off from their past." (Kuhn, quoted in Weintraub 2002: 2)[2] I do not mean just to make the logical point that badly misrepresenting the past is not the same as cutting oneself off from it. I dispute Roy Weintraub's claim that "in neither mathematics nor physics does history have a place." (Weintraub 2002: 2) Leaving aside the special case of mathematics, the (real!) past does play a very important, principled role in science, but it is obscured even if (and especially because) history is not part of the teaching curriculum (in context the concern of Weintraub).

In particular, history enters in the current research practice of science in at least two ways: first as a source of "the data." Here "the data" is not so much understood as the "tribunal" in front of which theory is held accountable or which it is "tested" against; it is seen rather as a source of the generation of theories and their continuous, ongoing development. Second, as my sometime collaborator, George Smith, argues, "when theory is embedded in ongoing research, the history of the development of the research can itself be a telling form of evidence, especially on questions about what the research has really accomplished." ("Testing Newton, Then and Now," paper delivered at University of Pittsburgh, 11/11, 2005) This second perspective is crucial if one wishes to evaluate the

[2] E. Roy Weintraub "Will Economics Ever have a Past Again?" Introduction to Weintraub 2002: 1-14. Of course, Kuhn did not deny that a distorted/Whiggish history serves various pedagogical functions in science-training and can create a sense of 'maturity' about the science.

success or failure of theories over time. Given the nature of the present occasion, I focus exclusively on the first.

Instead of offering many examples from the practice of science, I take advantage of a coincidence. While preparing a draft of this paper, I received the following description of a doctoral program through a listserv I subscribe to: “The Historical Astronomy group [at the University of Durham] conducts research into two overlapping areas: (i) the history of early astronomy, in particular the development of astronomy in ancient Mesopotamia, Greece and China; and (ii) the use of historical astronomical data in problems of modern science.”[3] I was (pleasantly) surprised that there is a group that explicitly conducts research in both areas. (I was even more pleased to learn that this group has a home in a department of physics.)[4]

It is, of course, no surprise that in astronomy we find some concern with history. Important data in astronomy spans several civilizations: from “ancient Mesopotamia, Greece and China,” mentioned in the advertisement, to the medieval Arabs and efforts in modern “science.” As Noel Swerdlow taught me, “from Copernicus on

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[3] On HOPOS listserv, 26 January 2006. A copy of the advertisement can be found at: http://luddite.cst.usyd.edu.au/archives/galileo/2006/000055.html, accessed on 25 March 2006. The research group’s web-page is: http://www.dur.ac.uk/physics/research/groups/?mode=centre&id=199, accessed on 25 March 2006.

[4] After some further research I learned that the American Astronomical Society has a Historical Astronomy division and that the International Astronomical Association has a subdivision (Commission 41) devoted to the history of astronomy. (The latter, however, seems to be exclusively devoted to the history of astronomy rather than “the use of historical astronomical data in problems of modern science.”) Of course, there are very few job-openings for historians of astronomy in astronomy/astrophysics departments.

people use ancient observations for deriving parameters of mean motions, which require observations as far apart as possible," (personal communication, March 25, 2006). Without this extensive data many important cyclical "phenomena" would be unknown (e.g., the precession of the equinoxes; the orbits of comets; the great inequality in the motions of Jupiter or Saturn; sunspot-cycles, etc), and the Newtonian *synthesis* of astronomy and physics would have been impossible to achieve (or, perhaps, contemplate). Many of the "phenomena" that Newton's theory set out to account for, and many of the striking predictions – which led people to accept Newton's theory, at least as a useful research tool, despite potential metaphysical and methodological qualms about the plausibility of action at a distance – involve appeals to such long running data. (One could do worse than to consult Adam Smith's "The History of Astronomy" on this matter.)[5] The history of astronomy is essential to the discovery of universal gravitation. If one removes the astronomical context to Newton's theory, one is left with a science of mechanics that would have no reason to suspect the existence of or investigate universal gravity as a fundamental force of nature. (I am not suggesting that astronomy or its long running data made this discovery inevitable.)

Some part of science will have a historical component in so far a) it aims to make claims about the behavior of models, systems, and institutions (etc) over time and b) it has a norm that encourages inspection and checking or revision of "the data." For

[5] It is well known that Smith offers a psychological account of the origin of scientific activity with particular emphasis on aesthetic considerations. However, for Smith's adoption of Newtonian realism see Eric Schliesser "Realism in the Face of Scientific Revolutions: Adam Smith on Newton's Proof of Copernicanism," (2005) *British Journal for the History of Philosophy*, 13(4): 697-732. In particular, I call attention to Smith's endorsement of Newtonian predictions and physical explanations.

even if some sciences do not require time-series data (because they are not trying to explain changes over time), the evidence that supports a theory becomes historical in nature as time passes.

At this juncture one may be tempted to make a distinction (well known in economics) between the history of astronomy or economics and astronomical or economic history. It may appear that I have not shown that the history of the discipline or other antecedent relevant practices matter, but only that for some pertinent parts of science history can be relevant to inquiry. However, in order to preserve and help (re-) interpret the historical "data," a set of skills and practices are required that are often not emphasized in the training of scientists (so much so that Kuhn and Weintraub come to their conclusions that history appears to have no place in science). This is why in my example I focus on data derived from entirely different civilizations. In the sixteenth and seventeenth centuries, an element of ongoing science was to recover and assimilate data from past practices; this required major linguistic effort as well as attempts to translate the data from different paradigmatic practices. (I am understating the situation: through the 17th century "history of astronomy is living astronomy and to be taken seriously."[6])

[6] Noel Swerdlow, personal communication, March 26, 2006. The remainder of Swerdlow's position is worth quoting: "Through, say, the middle of the seventeenth century, not only were ancient observations of use, but ancient astronomy, meaning mostly Ptolemy's *Almagest* was still of use, that is, it wasn't just history, it was astronomy worth reporting along with, say, Copernicus or Tycho or Kepler, as it was still taken seriously, still very much alive. It didn't really matter if one was heliocentric or geocentric, Ptolemaic, Tychonic, or Copernican, the models for the motions of the sun, moon, and planets in the *Almagest* still had something to teach, and even more so did Ptolemy's methods of deriving parameters

The results of these labors are achievements that can continue to be refined and questioned. So in order to be able to recover the historical record for present evidential purposes, the academic division of labor must encourage the existence of some specialized historians with their own intellectual infrastructure.

Thus, I deny that there is always a firm distinction between economic history and the history of economics. Adam Smith's long "Digression on Silver" at the end of Book I of *Wealth of Nations* (WN) is as much an exercise in economic history as it is history of economics. The same is true of Milton Friedman's and Schwarz Jacobsen's *Monetary History of the United States*, which admits in its preface that the "[accompanying] analytical narrative…significantly affected [the] statistical analysis." (xxi) Economic history (whether it contributes to ongoing research in economic theory or not) will always require considerable knowledge of the history of economics because the data of the former are often embedded in the practices and doctrines of the past. Moreover, as Adam Smith recognizes in the "Introduction" to *Wealth of Nations*, the economic policies of "princes," and economic activity more generally, are often influenced by theories of political economy past and present. Finally and not insignificantly, rational expectations may differ if theory differs.

This reliance on history can go very deep. For example, the announcement of the famous 43 arc-second per century anomaly in the procession of Mercury's perihelion (in the middle of the nineteenth century) was itself based on observation of Mercury's transits across the Sun dating back to 1631. It is useful for the argument I am developing

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from observations. The work that shows this the most is Gambattista Riccioli's *Almagestum novum* (1651)…"

that the 43 second anomaly was itself a revision, based on a new look at the data and theory, of an earlier estimate of 38 seconds. This anomaly is not something that can itself be ‘observed’ in the data. It is not curve-fitting. *It presupposes commitment to Newtonian theory and the evidence supporting it and enabling revisions to it accumulated over two centuries.* (George E. Smith, 2005.) It is often not appreciated that the ‘observation’ that led to the demise of Newtonian theory *was made possible by its historical development and its history of success.* (The anomaly would not have been ‘observable’ when Newton first formulated his theory.) As I indicate above, Newtonian theory had by this time itself relied on historical data in its development and subsequent entrenchment over time as the most promising theoretical research program. This example is significant in a further way; it shows that one cannot always insulate the a-historical part of science (mechanics) from the more historical part (orbital astronomy).

Furthermore, a widely shared epistemic principle is that it is always available to a scientist to re-open discussion of an established theorem, principle, or “fact.” A crucial and not un-common way to do this is to re-analyze “the data” or the theories that allowed one to distinguish data from noise. (Sometimes re-analysis of data makes a supposed problem or objection disappear.) When an issue is of great importance to theory as ongoing research, there can be constant return to source-data. Given this commitment, there is a place for historians of a discipline (even or especially if the ‘data’ was not understood as such or in the same way in different civilizations or epochs). Sometimes the historian’s skills are needed to help recover, uncover, (re-)interpret, and contextualize (theoretically or in terms of measurement) the relevant data. This is so because data are not just discovered in the wild, as it were, and available to inspection. Instead they are themselves the product of complex interaction between instruments (and -- as Duhem pointed out long ago -- the theories presupposed in them),[7] theories (models, assumptions, etc), observers (and their environments), and 'nature.'[8] So my argument follows from a fairly innocent assumption about the nature of scientific enquiry: that data are embedded in theories and measurements. This gives rise to a certain form of practical or psychological incommensurability between knowledge presupposed in current research practice and past data gathering strategies and recording. Special historical skills allow the gap to be closed.[9] My argument does not require commitment to the stronger assumption of so-called theory-ladenness of data.[10]

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[7] Pierre Duhem (1991; Reprint edition) *The Aim and Structure of Physical Theory* Translated by Philip P. Wiener Princeton: Princeton University Press, especially chapter 4. My whole argument is, of course, deeply indebted to Duhem's insights.

[8] I discuss more of these issues pertaining to economics in Schliesser "Milton Friedman, Positive Economics, and the Chicago Boys," (in progress). For a very sophisticated treatment, see M.J. Boumans (2005) *How Economists Model the World into Numbers* London and New York: Routledge.

[9] This is Adam Smith's view (Schliesser 2005), who also nicely described the phenomenology of this kind of psychological incommensurability in his fragment on "The History of the Ancient Logics and Metaphysics" in *Essays on Philosophical Subjects* (1982) Edited by W.P.D. Wightman and J.C. Bryce, Indianapolis: Liberty Fund, ¶10, 128.

[10] In the body of the paper, I focus on sciences that deal with long-range historical phenomena (one can add examples from archeology and climatology). But, as Sabina Leonelli first reminded me, the day is quickly approaching in some of the experimental sciences that rapid technological progress can make (tacit and theoretical) knowledge about instruments/experimental set-up obsolete within a 'generation' of one's PhD training. Sciences that use sophisticated computer-modeling techniques/calculations may face the same problem in recovering insight in properties of underlying code. So in such sciences historians of the discipline may become increasingly required to ensure continuity of research and theory.

Of course, not all past data relevant to modern inquiry is exclusively or even primarily influenced by past (proto-) economic theory. So, a cultural history of science can be necessary and useful to the evidential practice(s) of ongoing science, in so far as this history is also concerned with the 'science' of the past. To put this in terms familiar from the so-called 'science wars' familiar from science studies in the 1980s and 90s, if historians of economics wish to offer products to be consumed by economists they needs to offer both 'internal' and 'external' history of economics that might be useful to current and future economic enquiry.

My argument implies – pace Weintraub – that historians of economics need not "insist on their autonomy from science." (Weintraub is quoting and endorsing the historian of science, Theodore Porter.) I reject the view that the study of the history of economics ought to retreat "into a home…within departments of history or history of science programs," if that entails that "the standards by which a piece of work in the history of economics must be judged are…specifically, the standards…employed by professional historians to evaluate and appraise historical writing"[11] (emphasis added).

[11] E. Roy Weintraub "What Defines a Legitimate Contribution to the Subdiscipline "History of Economics?" Editorial posted: 1996-09-09. http://www.eh.net/HE/hes_list/Editorials/weintraub.php Accessed 25 March 2006. I don't want to overemphasize the differences between Weintraub's position and mine. We both take the relative professional success of economic historians as a model worth emulating by historians of economics. However, on my model economic history is a partial intellectual *failure* if it does not even aim to contribute to the improvement of the analytic tools, the accuracy of the data of and/or conceptual apparatus useful to (mainstream) economic theorizing. Mark Blaug reminds that some economic historians have worked hard to be relevant. See, besides the well-known work by Fogel, for example, Joel Mokyr *The Gifts of Athena: Historical Origins of the Knowledge Economy* (Princeton, 2002).

This dooms history of economics to irrelevance within economics departments. Thus, I reject the mutual rejectionist position: mainstream economics rejects the relevance of history of economics, and in turn history of economics folk should reject the mainstream and take their work to a department that appreciates them for who they are: historians. To be clear: I do not wish to exclude those that treat history of economics either as an autonomous or critical-of-the-'mainstream' enterprise from history of economics.[12] My argument, presupposes that there will be an intellectual division of labor among historians of economics, some of which may find a home in history and science studies.[13]

In order to avoid confusion, the position I defend should be distinguished from two related uses of history in ongoing practice.[14] First, it is well known that history can

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[12] Moreover, some historians of economics are not so much critical of the mainstream as wish to broaden its discussion, make it more self-aware/reflexive, or shape its future. For very valuable comments on the social, institutional, historical, political, and conceptual origins as well as the implications of the mutual rejectionist position, see Steve Fuller (1996) "Who's Afraid of the History of Contemporary Science?" in *The Historiography of Contemporary Science and Technology*, edited by T. Soederqvist Reading: Harwood. Monthly Guest Editorial for HES, http://www.eh.net/~HisEcSoc/Resources/Weintraub/Fuller.shtml, accessed March 7. 2006.

[13] It should be noted that there are only few science studies programs (some of which have rather fragile institutional support), and most of these are devoted to supporting research about either the medical/life sciences and/or the physical sciences. It is to be doubted that science studies programs can support the professional careers of historians of economics at more than a fraction of the existing number of historians of economics that at present have employment in economics departments.

[14] I ignore other, potentially more important roles that history can play in shaping the aims and structure of evidential practices in the sciences (cf. Paul Feyerabend (1989) "Realism and the Historicity of Knowledge," *The Journal of Philosophy* 86:8: 393-406.)

be a source of inspiration to develop new hypotheses to improve present practice. Adam Smith's emphasis on sympathy, for example, has been cited in this regard in recent literature on experimental economics, behavioral economics, and neuro-economics.[15] In late twentieth-century economics, Adam Smith plays a special authoritative role in such 're-discoveries' by people across the intellectual spectrum: Hayek, Buchanan, Sen, Vernon Smith, Coase, etc. (I leave aside how 'historical' their favored version of Adam Smith is.) My view is not incompatible with this practice, but it cannot be reduced to it.

Second, Hasok Chang, a historically inquisitive philosopher of science, has advocated what he calls "complementary science," that is, the use of history and philosophy of science to "generate scientific knowledge in places where science itself fails to do so." Chang's argument follows from his explicit acceptance of Kuhn's diagnosis that "normal science" can "only function if certain fundamentals and conventions are taken for granted and shielded from criticism." The success of paradigms and their displacement of certain questions sometimes entails the "loss of knowledge, actual and potential." Chang, thus, not merely explains why in response to this state of affairs "heterodox"[16] approaches may find a refuge in the history and philosophy of a particular science, but also advocates their practice as a form of "complementary science" (Chang 2004, 236-7).[17]

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[15] See also the work by Sandra Peart and David Levy (2005) *The "Vanity of the Philosopher"* (Ann Arbor: The University of Michigan Press) which mixes history and theorizing.

[16] See the essays by Sheila Dow, Peter Boettke, and Antony Brewer in Weintraub 2002.

[17] Hasok Chang (2004) *Inventing Temperature: Measurement and Scientific Progress* Oxford: Oxford University Press.

Chang's account of a "complementary science" offers an exciting model for those drawn to the history of economics with a critical perspective on the so-called mainstream. It is, however, unclear how the 'results' of such a heterodox complementary science can be accepted as "scientific" by the mainstream.[18] Chang and I agree that the practice of history (and philosophy) of science can be more than just conforming to a historian's task and standards. It can aim to uncover lost knowledge. But my position also allows for an important role for history within the mainstream, even if it will require better marketing and superior products to make the mainstream receptive to it.[19] Even if economics is a so-called mature, normal science (in this essay I bracket what it would it would take to be a 'normal' science in Kuhnian sense)[20] then in the context of regular

[18] In private correspondence (20 April, 2006), Hasok Chang argues that "we need a distinction between neglect and suppression. Specialist science will *suppress* some things because they conflict with its own premises, and *neglect* other things simply because they are uninteresting or unimportant. In the latter category, complementary science may produce results that orthodox science can happily allow to stand. This is slightly similar to the difference between "alternative" medicine and "complementary" medicine, though the meanings of those terms are not always clearly distinguished. An orthodox Western doctor may freely grant that all sorts of traditional or exotic herbal remedies can work, and may even grant that acupuncture relieves pain, but will not accept the homeopathic claims about the "memory of water", etc. I think complementary science should embrace both the neglected and the suppressed."

[19] My position is not far removed from the spirit of a "scientific philosophy" proposed by Michael Friedman (2001) in *Dynamics of Reason*, Stanford . Friedman emphasizes the positive role that philosophy has played and can continue to play in formulating novel principles for ongoing and future science(s) and helping in consolidating and interpreting new paradigms. Not unlike Friedman, I also offer this as a positive vision *for philosophy*.

[20] It is not so long ago that Marshall called for a positive science of 'economics.' (I thank Leon Montes for discussion of this point.)

ongoing inquiry there is an untapped demand for history of economics that is neither Whiggish, anachronistic, nor heterodox. If it is still immature, then my vision for the history of economics can help change that. Of course, like in all entrepreneurial activity, it is not clear that there will be a market for the kind of product I advocate; economic theoreticians and econometricians may feel they can go on just fine without the kind of comparative, historical data that I am promoting. Knowledge of economic history and history of economics may never again be thought relevant for ongoing inquiry, or be fairly marginal in it (e.g., the status of history of astronomy in astrophysics).

III Conclusion and Disciplinary Consequences

The current decline of interest in the history of economics among economics departments suggests (among other things) that historians of economics are not offering the right kind of product to economics as an ongoing form of inquiry. Though I offer a diagnosis of the future potential of the history of economics that competes with that offered by Weintraub, Schabas, et al, I agree that the history of economics may need to find at least a partial and temporary home in other departments. I emphasize the partial nature of such a situation because my vision requires that at least some historians of economics continue to be trained to understand the cutting-edge tools and questions of mainstream inquiry. I emphasize the temporary nature because history of economics should aim, in part, at (a return to) relevance to ongoing science. History departments are unlikely to be welcoming to this stance. In my view, philosophy departments and history and philosophy of science departments are more promising avenues.

Moreover, many philosophers instinctively agree that even if we assume, for the sake of argument, that economic theories can be genuinely scientific (objective, true, falsifiable, reliable, etc), they cannot escape being philosophic in the sense of being enmeshed in values and, thus always open to philosophic reconsideration. So *one* of the natural homes of the history of economics is in (history of) philosophy, where as the study of (the history of) political economy it can be nurtured among many other theoretical enterprises potentially relevant to economics (and other sciences), in particular, Bayesian decision theory, game theory, and normative (utilitarian) ethics. Various social factors incline philosophers to accept work on formal theories as philosophical (and not as a sign of lack of professional competence). My proposal also takes advantage of philosophers' desire to accommodate social and institutional demands for 'contemporary relevance,' and their wish to interpret this differently than the recent vogue for applied ethics (which has dubious intellectual status in the philosophy profession). This helps explain interest in establishing so-called PPE (i.e., Philosophy, Politics, Economics) programs.[21] I call on historians of economics to explore this option.

So my approach offers a promising professional path for those who wish to combine theoretical interest in economics with historical and philosophic sensitivity. If they have heterodox inclinations Hasok Chang offers them a promising vision; if they have more constructive intuitions they should follow my advice. In competition and collaboration with those inspired by Schabas and Weintraub, these three approaches can find spaces in economics departments, history and history of philosophy of science

[21] As the mention of PPE programs also suggests, many of my arguments about philosophy carry over into political science as a potential home for history of political economy.

programs, political science departments, business schools, and philosophy departments (etc). (One should not fear the inevitable competition over scarce resources.) They will do so by building on existing intellectual infrastructures. Thus, I offer a vision that can appeal to the self-interest and high ambition of those that wish to make the future of the economics and its history.[22]

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[22] I thank Erik Angner, Hasok Chang, Jerry Evensky, David Levy, Leon Montes, Ivan Moscati, Robin Neill, Warren Samuels, Margaret Schabas, Abe Stone, Noel Swerdlow, Paola Tubaro, audiences at DEMUS at the Free University of Amsterdam and ESHET, Porto (2006), and, especially, Sarah Brouillette for helpful comments. The usual caveats apply.