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The Atlantic divide: methodological and epistemological differences in economic history

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ABSTRACT

In the paper the development of economic history will be placed within the evolution of Western thought and culture. Therefore an analysis of the connections between economic history and contemporary epistemology will be carried out. In this perspective an analogy with the traditional division between analytic philosophy and continental philosophy would appear to be useful for economic history too: the first had long prevailed in Anglo-Saxon, the second in continental, culture. This partition evokes and embraces the antithesis between scientific and humanist culture, between logic and rhetoric, analysis and interpretation, conceptual clarification and visions of the world. The paper suggest that the opposition that loomed large over the post W.W.II decades between Anglo-American and European economic histories can also be conceived as a specific form of the wider opposition between ‘analytic style’ and ‘continental style’.

Keywords: *economic history, methodology, epistemology, cliometrics, business history, economic thought*
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1. Introduction

The purpose of this paper is to offer a contribution to the explanation of the differences among the various schools of economic history. This will be done by placing the development of the discipline within the evolution of Western thought and culture of the last century².

Since the nineteen-forties, economic history, still quite a young discipline, has been characterized by a rich methodological debate and, at times, by harsh controversies. These saw, on the one side, the supporters of rigorously scientific research programs with precise and explicit analytic boundaries, a problem-solving approach and the use of tools and methods borrowed from economics; on the other, those championing a hermeneutic approach and 'humanistic' conception of economic history whereby a comprehensive vision of history prevails and the analysis of specific (economic) aspects is only conceived as part of the whole. The first position has been distinctive mostly of the American school, the second largely of the continental schools, with the British one (and the Polish and the Scandinavian) somehow in between.

More specifically, an analysis of the connections between economic history and contemporary epistemology will be carried out. In this perspective an analogy with the traditional division between analytic philosophy and continental philosophy would appear to be useful for economic history too: the first had long prevailed in Anglo-Saxon, the second in continental, culture. This partition evokes and embraces the antithesis between scientific and humanist culture, between logic and rhetoric, analysis and interpretation, conceptual clarification and visions of the world. The paper will thus try to verify whether the opposition that loomed large over the post W.W.II decades between Anglo-American and European economic histories can also be conceived as a specific form of the wider opposition between 'analytic style' and 'continental style'.

This is only one of the possible keys to approaching the history of economics, of course. Other factors, possibly even more determining, have influenced its destiny on both sides of the Atlantic, factors that have often had a more mundane nature and less noble aims: access to research funds, desire for prestige and academic power, conflicts between schools - by no means insignificant issues in academic sociology, which we must restrict to a few rapid references.

The paper is organized as follows. In the next section I will try to show briefly how - before 1940 - no definite approach had yet emerged in economic history. In the third section, the origins of the dichotomy between continental and analytic styles is traced along epistemological lines, while the fourth is devoted to the question of the different analytic languages that might explain the differing results obtained by the same approach in the U.K. and U.S; the fifth makes a brief incursion into business history; the sixth introduces the theme of the crisis of the

² This topic has been already discussed in Toninelli 1999 and 2002

neo-empirical model and discusses its consequences on the discipline; the seventh briefly tackles matters of academic economics and sociology. Finally, a few words of conclusion will round off the paper.

2. Economic history at its beginning: methodological problems and approaches

In 1987 a distinguished proponent of the scientific approach to economic history - 'cliometrics' or, as he herself suggests, 'historical economics' – observed that the *methodenstreit* of the early years between new (scientific) and old (traditional) economic histories had come to an end. Any 'highbrow' (i.e. philosophical and theoretical) issue had shown itself to be 'fruitless' and 'irrelevant' viz. practical ones such as the scientific achievements and proliferation of the 'new discipline' (McCloskey 1987: 16-18). Still, there is a question – «all good history has a question», to quote Deirdre McCloskey again – the answer to which may require a few 'highbrow' reflections. Why, since the Fifties, has economic history, in spite of having quite a homogenous matrix, split in two clearly diverging branches, one almost exclusively pertaining to the Anglo-Saxon world, the other to the European (continental) one?

Economic history may be seen as the youngest of the social sciences. It would certainly appear to be so, at least from the point of view of autonomy and subject specialization - that is formal independence of other branches of knowledge, and acknowledgment of academic dignity – traits that can be traced back mainly to the inter-war years. At the same time, there is almost unanimous consent in tracing some of its real origins back to the inductive approach and thematic strongly associated with the economists of the German historical school.

It was in particular from some exponents of the second generation (the so-called 'young' historians) and certainly from Gustav Schmoller that the proposal came to re-establish economics on a historical and institutional basis, in a clean break with the then dominant abstractions of the classical and marginalist school. However, if «the historical method, applied to economic studies, seems largely to have failed » (Gras 1920:210), its failure was offset by the opening up of a new discipline, that is Economic History. This in fact, in its early decades of life, in Europe as in America, grew up under the influence of the German school (Scott 1928, Gras 1942) and therefore in the conviction that the abstractions of economic theory were of little use to a study of history (Ashley 1893:127).

Some decades would pass before economic history was freed from the excesses of the inductivism that had marked its origins, and before it addressed in a more balanced, less polemical way the question of the relationship between fact and theory. Of course, the problem would not disappear completely and has surfaced again and again, right up to the present, as the methodological crux of the

discipline. In the inter-war years a compromise did seem to have been reached, making it possible to celebrate the union between History and Economics, but it was still a vague compromise at a methodological level. William Ashley's tardy recognition of a sort of rediscovered harmony between the two disciplines (Ashley 1927) appeared essentially driven by a desire for academic recognition of the discipline, which was slow in coming³. It was therefore a compromise more apparent than real, and it concealed a plurality of positions and wealth of initiatives (e.g. publishing) which certainly did not have a single character⁴.

If we observe the evolution of the discipline in countries like France, Germany or the United States, we can see emerging in the inter-war period many different lines of research, which seem to have had little in common except scant sensitivity to the question of conceptualization. Yet they were extremely productive economic historiographies, attracting attention for their descriptive accounts, large number of facts (sometimes massed together without any logical criteria other than *post hoc ergo propter hoc*) and minutely detailed reconstructions. All this work was based on incessant recourse to archives in the two European countries and, where the United States was concerned - due to the obviously limited availability of historical sources - to copious statistical documentation produced since the early 1800s by the Census Bureau (Sèe 1927, Gras 1927, Brodnitz 1927). It is no coincidence that in the States the history of economics made its impact in close association with the history of enterprise, a field of study developed early on, partly thanks to the huge amount of business archives available⁵. To some extent, then, the scarce concern for conceptualization can be explained by the relative lack of absorption by French, German and American culture of British-style economic theory⁶.

Yet in France and Germany, as well as in the United States, the calm waters of 'the traditional' approach were being churned up by innovating research programs and stimulating methodological proposals, though not always pointing in the same direction. In France, for example, Lucien Febvre and March Bloch, continuing the tradition of the *Revue de synthèse*⁷ created with the *Annales* a 'global' history program that set out «to establish a kind of hegemony of History over the other human

³ Not by chance the program of the new "Economic History Society" (1926) –the first president of which was Ashley – did have just a practical trait, without any reference to methodological or theoretical issues, being finalized just to the promotion and diffusion of economic history (*Economic Journal*, 1926:322-3).

⁴ See f.i. the diverging remarks of Clapham (1922a and 1922b), Pigou (1922) and Hecksher (1929).

⁵ As shown by the appointment of Edwin Gay to the position of Dean of the Harvard Business School, and by the title of the first, short-lived (1928-1933), periodical devoted to economic history in the U.S., the *Journal of Economics and Business History*

⁶ The situation in Germany was obviously connected with the lasting influence of the German Historical School. In France, on the contrary, at the turn of the twentieth century marginalism had to compete with the clearly empirical line of positivist economics, which with S. Simiand was attempting to combine statistical form and economic analysis. In the U.S. then, the branch of political economics prevalent at the beginning of the century institutionalism, represented an original native mixture of Peirce-style pragmatism and Marshallian echoes, but paying attention to the social-historical question.

⁷ See for instance Bloch 1949, Febvre 1953, Braudel 1969, Aymard 1972, Stoianovich 1978, Burke 1978). On the *Revue de synthèse*, cfr. Gemelli 1978

sciences». On the basis of this, 'history was given a privileged position, especially over Social Psychology and Sociology, and even more over Economics' (Braudel 1992:301). In Germany instead, with Max Weber and Werner Sombart, there was an attempt to react against the excessive historicism of the History school⁸. In particular, the position of Sombart - the search for dynamic models for History (Sombart 1929)- would not remain isolated and, though remaining a minor part of economic history, would produce some of the most meaningful and long-lasting contributions to the discipline, starting from the 1930s, when in the United States an economist *sui generis* like Simon Kuznets, with his dynamic analyses of the secular movements of production and prices, independently came to make this choice (Kuznets 1930).

The panorama of economic history between the two wars thus presented a plurality of positions and study lines that were ill suited to defining a precise methodological set of rules. This may help to explain why academic acceptance of the discipline was so slow - a problem most rife among historians –making it an object of discussion in the pages of the first (for some years only) periodical exclusively devoted to economic history, the *Economic History Review*.⁹

3. Analytic economic history vs. continental economic history

From the Forties onwards the plurality of positions and fervor for initiatives of the inter-war period on both sides of the Atlantic seemed to concentrate more and more around two main methodological protocols. For the moment I will call them *strong* economics protocol, with reference above all to the American schools, and *weak* economics protocol, to which the greater part of the continental schools can be ascribed. Both schools seem to have become aware that it was necessary to offer economic history a role and a specific task, so that it could develop as an independent discipline. The problem of the bounds, limits and methods that should characterize it, however, was mainly felt by supporters of the first kind of protocol, that is within the U.S. Here an important role was played by the creation of the American Economic History Association in 1941 - which in that same year produced the *Journal of Economic History*.

It is meaningful to re-read with the eyes of an American economic historian of today the objectives the founders of the first association set themselves, because

⁸ Sombart proposed that the economic historian, taking advantage of his specific preparation in the two disciplines, should attempt to fuse them in an original conceptualization (Sombart 1929). The model Sombart had in mind was obviously his own, that of "modern capitalism", which with each ensuing edition became more and more complex and obscure, to the point of making Einaudi complain of "the waste of so much learning and mental effort" (Einaudi 1936: 204).

⁹ This, in 1931 featured an inquiry into the teaching of economic history in the universities, producing a picture that was by no means encouraging. In most continental Europe countries, chairs devoted expressly to the discipline could be counted on the fingers of one (De Mann 1931/2; De Rosa,1990:40 and *passim*).

in this hindsight reinterpretation (with an inevitable forcing meant to anticipate what was to follow), it is already possible to read the future of American economic history and its landing on the shores of cliometrics:

«Fifty years ago our founders established the Economic History Association to convert economic history from a topic into a discipline. Looking back, we can see that their objective was formidable. A discipline requires much more than self-identification, mutual recognition and a set of by-laws. A discipline requires standards, a productive skill, and a place in the larger world of human endeavors» (Sutch 1991: 272).

At a first reading of the original interventions presented at the inaugural meeting of the Association, held in Princeton in September 1941 and published in the supplement of the first annual *Journal*, the new research program of American economic history seems less easy to predict than it actually was¹⁰. More than one intervention focused on the tasks and methodological rules for economic history, in particular on the need for greater quantification and closer links with economic theory¹¹. However what turned out to be really meaningful and revealing was the publication in the first number of the review of an article by Simon Kuznets¹². In it were highlighted the thematic and methodological specificities of the discipline, specificities that made necessary the recognition «of the need for more extensive use of generalization and analysis in economic history». In fact the great new challenge for the historian was to be considered the search for uniformity in the amorphous mass of facts that history proposed: and this could only be done with the guide «of hypotheses and categories evolved by theoretical and statistical analysis in the foreground of economic inquiry» (Kuznets 1941:39).

Therefore, even if emphasized with hindsight by the cliometric lens of (McCloskey-style) ‘modernism’, the guidelines for the future path of the newly institutionalized discipline can be traced here: a precise set of rules and

¹⁰ In particular, in his opening address, John U. Nef – in underlining the risks of “isolation from the rest of learning” the newly created association was going towards – said he was convinced that the scant interest recorded so far by American economic history for its European equivalent was to be attributed to the fact that the latter was mainly the work of scholars of “general culture” and particularly versed in philosophy (Nef 1941: 6-7).

¹¹ Particularly those of Edwin Gay (Gay 1941) on the tasks of economic history, and E. A. J. Johnson (Johnson 1941) on the importance of the use in the discipline of new analytical instruments borrowed from statistics and theory.

¹² Kuznets’ contribution to the development of a more rigorous and systematic approach of economic history should actually not be undervalued. A pioneer in the use of retrospective national income accounts as a tool to study economic growth, his interest in the organized comparative study of structural change dated at least as far back as 1937, when he created the *Conference for Research in Income and Wealth*, and was able to obtain the financial support of the Rockefeller Foundation. His claiming for a conceptual comparative framework to make sense of the large amount of historical data created, while creating a tension with his colleagues at the NBER, opened a new research agenda to economic historians. Later (1947) he was among the founders of *The International Association for Research in Income and Wealth* he chaired the Committee on Economic Growth which recruited leading scholars in eleven countries (such as P. Deane, J. Marczwesky and J.C.Toutain) to study long-term growth. His emphasis on measurement and quantification greatly influenced the first generation of cliometricians, as among his post-graduate students one can enumerate champions of the new economic history such as L.Davis, R.Easterlin, R.Gallman, R.Fogel and S.Engerman. See e.g. Fogel 2000, de Rouvray 2004, Syrquin 2006.

methodological standards, the refining of working techniques and, above all, a rigorous identification of the discipline's boundaries. It was an ambitious and innovative program, the realization of which demanded much resort to economic theory, in order to identify problems and give them analytic treatment using sophisticated quantitative techniques. This would make it possible to attain «professional standards to set its practitioners apart and above charlatans and dilettantes» (Sutch 1991:272).

On the continent, instead, economic history seems to have found it harder to take its place as an independent discipline, clearly identifiable among other fields of historical research. It is obviously impossible here to provide a panorama of all the branches and schools of study (a task for which I would be totally unprepared in any case). In the decades on either side of the Second World War, economic historiography in Europe was strongly influenced by the studies of E.J. Hamilton, whose hypotheses on the impact of the price revolution on the birth of capitalism obviously had profound implications for modern history on the continent (Hamilton 1928,1929,1934). The plethora of quantitative studies, often of an essentially compilative character, ended by causing increasingly bitter criticism, however¹³.

The methodological assumption shared by many of the lines taken by continental research was the conviction that it was foolish and artificial even to attempt to divide human endeavor into separate compartments. It followed that any rigorously exclusive treatment of the economics question was considered, apart from being misleading, meaningless. Since economic aspects are closely interconnected with social, political, cultural and anthropological ones, any study of economic history was expected to take this into account. Paradigmatic of this approach was the research program of the *Annales*, especially in the first two generations of historians recognized as belonging to it (from Bloch to Braudel, to be exact): as we know, it was characterized by the ambitious attempt to encompass the social sciences in a history conceived as 'total history', as all-embracing knowledge of social reality and its development.

Albeit with different accents, aims and awareness, many continental schools developed similar lines of research: in particular, the various lines of economic and social history established in Europe after the Second World War and above all, though not only, those of Marxist inspiration (think of the famous *Studies in the development of capitalism* by Maurice Dobb, 1946). Indeed, the influence of Marxist thought on the evolution of economic historiography in Europe must not be underrated - an influence much more strongly felt on the old continent than in

¹³ Even those, like Gino Luzzatto, who had shown themselves more sensitive to the problem of identification and delimitation of the discipline, reacted with proposals that were not fully in harmony with the lines previously expressed: "Economic history" said Luzzatto in 1951 "is always and above all the history of mankind, and the field of our studies is so vast and varied that it is only on a tiny part of it that full light can be shed with quantitative research (Luzzatto 1951: 349).

North America¹⁴. In France this contributed not a little to scaling down the admittedly promising beginnings¹⁵ of a cliometrics school on that side of the Alps (Grantham 1997). In Germany «the failure of cliometrics to gain many converts...despite the strong position of economic history in German universities» (Eddie and Gray 1999:1) can be explained by a number of causes, particularly the German historical tradition and the weakness of its econometric school¹⁶. In that very Britain which would have served as the ‘main bridgehead’ for the diffusion of the innovations coming from the new U.S. economic history, the radical tradition of social and economic history pointing at the adverse social consequences of industrialization had been long prevailing (Mathias, 2006)

Why this dichotomy, therefore? How can we explain the different directions taken by the discipline on opposite sides of the Atlantic? Is it enough to refer to the diverse influence of Marxist thought? Or is it due to the different ways the economic schools developed?

There is no easy answer. Naturally, the two reasons given above are important, as is the fact that in order to follow the evolution of economic history, it is essential to understand its relationship with the economic theory and micro- and macro-economic models from which it has continued to draw inspiration (we will return to this point later). However, if we choose this kind of explanation (however unsatisfactory), we simply downgrade the problem: why did neoclassical synthesis enter the mainstream so quickly and get to dominate the economic scene in post-war America? How can we explain the lasting success of critical political economics in Great Britain, or the choice of introspection as methodological criterion for the Austrian school and its consequent distrust of econometric models?

My efforts will therefore be addressed to attempting to place the history of economics in the wider context of the evolution of Western thought and culture in the last century. More specifically I will try to analyze its links with contemporary epistemology and to identify the conscious or subconscious influences and conditionings deriving from it. This is an operation that has often been proposed in economics, and it shows quite clearly how the structure of economic science has been modeled on study of the natural sciences (Pheby 1988). Economic history, on the contrary, seems to be less sensitive to the problem, even if some time ago,

¹⁴ Significant in this regard are the considerations made by William Parker in the opening speech of the 1970 meeting of the Economic History Association: «We have really very little experience outside the south with societies that are class structured in any obvious and fundamental way – class structured as a matter of theory and principle as well as of practice. So we tend to think that social classes are a devilish invention of Marx. We have really no intuitive grasp of why backward societies are backward, why defeated people are defeated, why people torn by social antagonism are so torn ... »(Parker 1971: 13-14).

¹⁵ The obvious reference is to the work of Toutain (1961, 1965) and Marzewsky (1961a, 1961b, 1965). They raised an intense debate that saw Marxist historians and “*Annalistes*” arrayed on opposite sides. Cfr. for example, Vilar 1965, Mandrou 1962, Le Roy Ladurie 1968, Chaunu 1964 and 1968, Furet 1971.

¹⁶ However according to Richard Tilly the situation of historical economics in Germany has been improving, (Tilly 2001).

while the controversy between cliometricians and traditionalists was raging, some important words were written about it (McClelland 1975). It is not very surprising, however. Where the conviction has grown that economic history should, as it were, fall under the rules of the 'more scientific' economics, questions of an epistemological and methodological nature have often tended to overlap economic ones. This is the case of cliometrics, with its ample and explicit recourse to economic theory, not only to establish causes between determined initial conditions and the historical event under scrutiny, but also to offer an exercise ground for the application of sophisticated theoretical models¹⁷.

On the other side, those who consider economic history to be no more than a single, if important, aspect of a general, all-embracing history, and who believe in the uniqueness of a historic event, do not even ask themselves whether there is an analogy with economics, not to mention with the natural sciences. Which means that they too would be likely to end by denigrating any attempt to go deeper into the epistemological bases of economic history. These are the two extreme positions, however, and between them lies a wide range of combinations between history and economic theory, or between history and the social sciences, including in particular the position of those who claim the need for an autonomous methodological 'charter' of economic history, including its own independent theorizations¹⁸. As I see it then, the decades-long debate between the various branches of the discipline can only benefit from epistemological openings.

Let us now try to connect the evolution the discipline underwent before and after the Second World War to the philosophical and cultural context of the age, when two different streams of thought became two opposing schools. A comparison with what happened in that context looks promising as a line of investigation. The new institutions and innovative research proposals coming from the world of Anglo-Saxon economic history, and in particular from the States, seem to fit perfectly into the climate of scientific rigor and discovery of objective analytic instruments that was taking hold. The new impetus also came from the drive given by different but converging philosophical trends, logical empiricism and language analysis. On the contrary, it can be claimed that the survival in much of western Europe of a 'humanistic' approach to history, including economic history (that is an approach in which narration and subjective interpretation enhance the pure analysis of the facts) may well reflect a widespread 'anti-economic' non-rationalistic ethos. Above all it is an obvious feedback from what Dilthey called the sciences of the spirit - the humanistic disciplines - and with the 'critical' philosophical and cultural

¹⁷ As is implicit for example in McCloskey 1985. Solow's observations are revealing here (1985: 330): «As I inspect current work in economic history, I have the sinking feeling that a lot of it looks exactly like the kind of economic analysis I have just finished caricaturing: the same integrals, the same regressions, the same substitution of t-ratios for thought. Apart from anything else there is no fun reading the stuff any more. Far from offering the economic theorist a widened range of perceptions, this sort of economic history gives back to the theorist the same routine gruel that the economic theorist gives to the historian.»

¹⁸ I have already discussed these issues in Toninelli 1987 and 1997.

climate in which the main branches of continental philosophy converge: phenomenology, hermeneutics, the Frankfurt school, etc.¹⁹

I mean to say that, with all due caution, the contrast that has developed in the last fifty years between US and, partially (as we will see), British economic history and that of old Europe highlights the difference, however superficially, between analytic philosophy and continental philosophy or, better, between 'analytic style' and 'continental style' (D' Agostini's 1997:16), thus meaningfully encapsulating the difference between the two approaches in contemporary culture. It simultaneously evokes and embraces the antithesis between scientific culture and humanistic culture, between logic and rhetoric, between analysis and interpretation, between clarifying concepts and working out 'visions of the world' (Preti 1968, Wallerstein 1997).

The contrast between the two styles is to be seen as a useful but necessarily rough classification: on account of its clear-cut and comprehensive character all the schools and lines of enquiry can be classified accordingly. It should not be forgotten, however, that for more than one author, such a rigid scheme has in recent years been partly overtaken by the facts: divergences have dwindled, conceptual barriers have broken down and a dialogue has got underway (Dummett 1997, 2001). The same might be said for economic history, as we will see, but the fact remains that in order to trace the origins and developments of the two main lines of research, such a taxonomy is a useful heuristic instrument.

A quite recent synthetic account of the different styles of writing and reasoning that separate the analytic from the continental helps in enlightening the respective essences of the two methodologies, American economic history and the continental variant:

«...the first type of philosophy [the analytic one] has the following characteristics:

- it makes use of *formalisms* and specialized language; it demands checkable reasoning at every point, so that it tends to deal with somewhat circumscribed issues;
- it has a prevalently conceptual or thematic slant and does not deal so much with authors or texts as with concepts and problems.

The second [the continental style]:

- excludes the use of specialized language, makes use of reasoning that is not always exactly reconstructable;
- has a prevalently historical or textual slant, makes reference to authors, texts, particular moments in the history of thought, to great historical and conceptual units (ontology

¹⁹ A partial exception is represented by the Scandinavian countries and Poland, where positivism and neo-empirism can boast a deep-rooted tradition. It is no coincidence that in these countries even economic history has followed a different course from the continental one, being more similar in many aspects to the Anglo-Saxon analytical approach. Think for example in the case of Sweden of the studies on mercantilism by Eli Hecksher, and above all his methodological contribution to a closer link between theory and history, and a wider use of quantitative instruments (Hecksher 1929, 1939), and in the case of Poland, of the studies on the economic theory of the feudal system, and the methodological contributions of Witold Kula (1970, 1972).

before or after Plato; the vicissitudes of modernity; rationalization in the modern age, etc.)» (D'Agostini 1997:58).

Let us try to make some small substitutions in the text, above all with reference to typically philosophical themes: to say, instead of authors or texts, the protagonists - men and their enterprises - of economic experience in the past (e.g. «Colbert's economic policy» or «Ford's multinational activity»). Instead of «ontology before or after Plato» let us insert «the first and second industrial revolution in Europe». What can escape no one with even a smattering of the thematic and methodologies adopted by the two 'schools' of economic history is the amazing similarity with the developments of philosophy just recalled

In synthesis (and carrying matters to extremes) the two methodologies of economic history can be compared and contrasted for some key elements. The first, the 'analytic one' from across the Atlantic, for the problem-solving approach, the conceptual slant (hence its links to economic theory), the employment of a specialized language (hence the frequent use of econometric and statistical techniques). The American cliometric school has represented it paradigmatically: to use Robert Fogel's words, it has set out «to transform economic history from a theory based on implicit, weakly specified and untested theories to one based on rigorously specified and empirically warranted theories». Further on in the same essay the 1993 Nobel Prize winner maintains that economic historians «must search ... for constructs which, while simple, are nevertheless capable of describing the realities with which they are concerned». In fact «simple models contain few parameters and are therefore 'efficient' with respect to the available data» (Fogel 1967: 297-8). Analogy with the 'analytic style' is thus reinforced where these last considerations are concerned: «Trust in schematization or at least in the establishment of some kind of theoretic model remains a typical requirement of the analytic style, and a crucial element in distinguishing it from the continental style» (D'Agostini 1997: 211).

Conversely the key aspects of the other current in economic history - 'continental economic history' - can be introduced precisely by the distrust in the above-mentioned schematization. Since several schools come under that heading, however, it is more difficult to identify common connotations, though there would seem to be some basic features present in the most representative contributions of each of them, with a different specific weight from case to case: holistic approach, narrative language, chronological slant, interpretative tension. For example within the Marxist tradition, the already mentioned Dobb's volume was emblematic of such an approach, as well as later on Eric Hobsbawm's main contributions would have been. Even more representative were the methodological positions taken first by Lucien Febvre in the *Annales* school, then by Fernand Braudel in his studies on the *longue durée* and world economy. For Febvre, «*histoire totalitarian* is expressed as a cognitive synthesis of all the social aspects carried out in 'experimental' form, that

is by integrating various disciplinary points of view, according to the idea that science must reproduce as indeterminate a character as society does». For Braudel, instead, «globality is more of a fringe idea that is expressed as an appropriation process by historians of the methods and techniques of fringe disciplines» (Gemelli 1987: 17).

An entire generation of historians and social scientists would have grown under the combined influence of Marxism and the French school, which reached also the other side of the Atlantic: great personalities such as Paul Bairoch, Immanuel Wallerstein, Jurgen Kocka and Giovanni Arrighi have been addressing historical matters with a broad comparative approach and a strong sensitivity for social and environmental issues. They have greatly contributed to nowadays blooming interest for world history and/or global history.

4. Economic History and Economic Theory

A brief reference has already been made to the connective function between the two sides of the Ocean exerted by British economic history. As a matter of fact the British soil was fertile and well suited to the American seeds. Its long-standing tradition in economics strongly contributed to the development after WW2 of an empirically well sounded and conceptually solid school of economic history in which stand out authors such as, e.g., Alec Caincross, Brinley Thomas, Phyllis Deane, H.J. Habbakuk and Ronald Hartwell (Feinstein 2003). In the same climate was educated Angus Maddison, a former outstanding professional economist and OECD consultant, who for some time has been producing valuable comparative historical growth accounts, largely used both by economist and economic historians.

Yet, simply bracketing British and American economic history together, particularly during the rise of cliometrics, would be misleading. There is quite a disparity between the strongly analytic American model and the weaker British one. This – once again – has interesting epistemological analogies and can be partly explained by differences in the analytic styles of the two schools. Recalling the previous schematization, the differing elements refer exclusively to the first point: the use of formalisms, specialized language and reasoning that can be checked at every point. What the two schools have in common, instead, is an essentially problem-solving conceptual slant. Within the analytic current a contrast emerged in the Thirties between the mainly American logical-neopositivistic component and the more properly analytic one diffused above all in England. In the Fifties this developed in the contrast between two types of language: *ideal* language in America, and *ordinary* language in the United Kingdom.. The first type of analysis aims to purify and clarify the language, «relating it to (or translating it into) a *formal and regimented* language, that is a language whose logical form and procedures are

controllable, clear and defined». On the contrary, the various currents that make up the analysis of language in Britain reveal themselves «somewhat averse to formalization and the use of formal procedures for the analysis and clarification of the natural language, since for (...) them the essentially interactive communicative nature of the language seems (...) to advise against this kind of procedure.» (D'Agostini 1997: 216-17).

Perhaps it is no more than a problem of language that seems to divide - within the analytic kind of economic history - the American school, in search of a 'pure' analytic language and the British one, more reluctant to formalize²⁰. This problem emerges clearly when reading what I consider one of the most important contributions of British economic historiography, *American and British Technology in the Nineteenth Century* by H.J. Habakkuk (1962) and of the decades-long debate that followed its publication. It was a debate that involved some of the most passionate exponents of cliometric history. This is not the place to enter the fray, however. What interests us here concerns the methodological aspects. In a 1997 interview Habakkuk clearly alludes to the differences between the economic languages of the U.K. and the U.S when he says:

«The most influential economists [of Cambridge economics in the period when it influenced me] were not interested in large-scale empirical research on the National Bureau model. ... They concerned themselves with a relatively limited range of established facts, and devoted a considerable amount of intellectual effort to discovering and analyzing why on this basis the system worked as it did. ... And also there ... [was] an absence of the formal statements of propositions and then the statistical proof of their truth» (Habakkuk 1997:29-30).

Indeed, in his attempt to re-read the different technological development in the United States and the United Kingdom, Habakkuk explicitly refers to «the union between history and theory» as something fruitful, especially with regard to «those parts of theory» that he considers relevant for the case under scrutiny²¹. Hence the heterodox approach that characterizes his text, where the neoclassical accents are tempered by a robust presentation with classical and Keynesian echoes²². But precisely from here moves the attack made by the 'new' American economic historians, who criticized him for not having specified his conceptual model sufficiently, so that it lacked analyticity and prevented the adoption of a formalized language capable of leading to precise and definitive measurements. So it was that Peter Temin, taking into consideration only the part of Habakkuk's

²⁰ Hence also a lesser reliance on technicalities as shown by the following statement by R. M. Hartwell (2001: 8) when saying that technique "can also be a dangerous tool for the unwary. History should always determine technique, but too often the use of technique limits history".

²¹ More specifically, as he will write later on (Habakkuk 1971: 320), theory should be used «to suggest hypotheses, pose questions, offer clues, and provide a test of logical consistency».

²² «The intellectual habits which my Labour Scarcity essays betray are much more those of Cambridge economics than they are of Cambridge history» (Habakkuk 1997: 30).

reasoning that can be formalized in neoclassical language (thus omitting the dynamic aspects connected with technological change), reached opposite conclusions to Habakkuk, namely that labor costs were lower in the United States than in the United Kingdom: but these conclusions contradict all the documentary evidence on the subject. However Temin, instead of renouncing his 'language', rejected the British historian's theses (Temin 1966, but see also Fogel 1967, Temin 1971, David 1975: 19-91).

This digression on the Habakkuk debate takes us back to the question of the relationship between economic theory and economic history, a question that for reasons already given surfaces above all within the stream of analytic economic history. It seems to be a long-accepted fact that economics can often provide history with decisive input in its attempt to explain past economic events, as shown by the quotations of Kuznets, Habakkuk and Sutch already recorded in this text.

The problem seems to be much more complicated and, all things considered, unresolved where the continental stream is concerned. Notwithstanding Braudel's sensitive approach to economics, the subject is placed by the *Annales* school on the same level as the other human sciences (anthropology, sociology, psychology etc.), which find their ideal laboratory in global history. In the Marxist school, on the contrary, it is clear that the relationship is enhanced by infusing it with social, political and ideological significance, properly defined by the term 'vision of the world'.

It is therefore only in the 'analytic area' where a tentative schematic subdivision of tasks into economic history and economics is taking shape. Though sharing the same nature, the two subjects differ in so far as the basic function of economic history is seen as explanation while the basic objective of economics is prediction (Friedman 1953). A grasp of the single problems provides all the elements, essential pieces to explain how the dynamics of socio-economic systems evolved through the ages, before and after the birth of the present, capitalist system.

As already mentioned, the nineteen forties represented a crucial turning point where the change of attitude towards economic theory was concerned. At the same time intense stimuli were being felt for closer co-operation between economic history and economic theory in the field of methodology also. The merit for this goes especially to Carl Gustav Hempel, a German philosopher, member of the Berlin circle and in close contact with the Vienna circle, i.e. the two founder groups of the logical empiricism current that went on to merge with the analytic one. Moving to the United States, where he taught at Princeton and Pittsburgh, Hempel published, among other things, various highly relevant methodological contributions to the social sciences. In particular, in his seminal essay of 1942, *The Function of General Laws in History*, he declared the principle of the «methodological unity of empirical science», from which he derived «the necessity in historical

research to make extensive use of universal hypotheses of which at least the overwhelming majority come from fields of research traditionally distinguished from history» (Hempel 1942: 48). The ‘different fields’ must therefore resort to their corresponding sciences, and ask them for the necessary laws to explain events - economic history, in particular, should turn to economic science. In the original 1942 version, Hempel, though admitting it was possible «to construe certain explanations offered in history as based on the assumptions of probability hypotheses» (p. 41), actually ended up identifying those laws (*covering laws*) with the universal generalizations typical of a nomological-deductive explanation. This model proceeds by deduction from universal (and deterministic) laws, comparable to natural law, valid everywhere and at all times.

Following Hempel’s theory, the central problem for the economic historian thus became which part of economic theory to choose, or which laws were suitable to explain the subject in question. The answer arrived with the rationalist, ‘scientific’ shift that had characterized the American economy since the Thirties (McCloskey 1985). Coinciding with the spreading of the ‘analytic style’ as a result of immigration into the country of some of the leading representatives of the European neo-positivist circles, the dominant institutionalist and pragmatic tradition was rapidly overshadowed by the impact of the marginalist-neoclassical current. This is known to have been involved from Walras onwards in the search for and purely theoretic demonstration of the existence of the ‘mythical’ general economic equilibrium, beyond any historically determined context. The economic models getting their cue from it are, however, constructed on axioms and postulates that take on the form of abstract, universal, theoretic assumptions. They have long dominated the mainstream of theoretic economics, finding an emblematic epistemological clarification in Friedman’s famous essay of 1953, in which economics was seen as an objective science on a par with classical physics (Pheby 1988).

There is no doubt that for the first generation of cliometric historians the most relevant historical theory was the neo-classical one, with its static models focused on the function of production, capable of tackling an essentially micro-economic topic (Toninelli 1987). Conversely, the other line of research was kept in the shade for a long time, in spite of its strong impact on both practice and Keynesian and post-Keynesian theory, which in its original, Cambridge school version, was completely ignored²³. This latter way of reasoning was more in line with British philosophy and language, another nursling of Cambridge (where Bertrand Russell lectured up to 1918 and Ludwig Wittgenstein taught in the Thirties and Forties)²⁴.

²³ But see Jeffrey Williamson’s observations in this regard (1979: 251-3).

²⁴ The epistemological approach underlying this kind of model is of the inductive probabilistic kind that draws inspiration from the experimental sciences from Bacon onwards. Through it one proceeds by trial and error and mutual feedback in search of probabilistic generalizations and valid rules inside a determined historical and temporal

5. Economic history and business history

From the late Fifties, therefore, the neo-classic analytic model made a growing impact on American economic history, and cliometrics was to become increasingly identified with it.

However, the conquest of the discipline was quite controversial: since the beginning the «Harvard wing» of cliometrics, formed primarily by Alexander Gerschenkron's disciples, appeared quite skeptical of the excessive reliance on the neoclassical price theory²⁵. Most importantly this position would have soon been endorsed by one of the most authoritative founders of the new school, Douglass North (1981, 1990) who, dissatisfied with the increasing reductionism of history to equilibrium models, was exploring a new approach which revitalized the role of institutions, somehow reverting to the origins of the discipline, i.e. the old historical school (Freeman & Louça 2002: 16-20).

Besides, in 1971 the growing dissatisfaction of business historians towards what was becoming the dominant paradigm in economic history led to quite a dramatic break between the two disciplines²⁶. From the late Forties the problematic cohabitation between economic history and business history seemed to have found a *modus vivendi*: under Schumpeter's influence, the bunch of young scholars associated with the Harvard's Center for Research in Entrepreneurial History tried to bridge the gap between the two disciplines and laid down the foundations of what was to become the dominant paradigm of business history: Chandler's organizational synthesis (Galambos 2003; Hausman 2003). By emphasizing structure vs. agents such an approach appeared *in se* congruous with the Anglo-American conceptual modeling, even if in the light of the mentioned subsequent partition this can sound almost a paradox.

As a matter of fact also with regard to the evolution of entrepreneurial history quite a clear dichotomy emerges, which emphasizes on the one side, the Anglo-Saxon, the centrality of the firm and on the other, the Old Continental, the role of the entrepreneur. Once more different epistemological backgrounds are likely to explain the diverging conceptualizations that lied behind the two traditions of research (Toninelli 2006).

context. In the Sixties Hempel himself, correcting his initial position, would come to admit the usefulness of such an approach in explaining history (Hempel 1965)

²⁵ Of course I am not discussing here of the primary controversy, the one which opposed cliometricians to "traditional" (institutionalist) economic historians: the latter included other distinguished Harvard professors, such as Fritz Redlich and David Landes.

²⁶ In spite of the opposition of Alfred Chandler, who did not want to abandon the Economic History Association to the cliometricians, in that year the Business History Conference was transformed into the formal organization of professional business historians fleeing the EHA (Chandler, 2004)

In the classical economics – from Smith to Marx – the entrepreneur was hardly mentioned, if mentioned at all. Production and the investment of capital was regarded as a sort of an automatic process, which involved no critical decision-making, no risky judgment or imagination of any kind. In this tradition capitalist was the only term which seemed to appeal the scholar. On the continent, instead, a long-standing tradition going back to the late medieval and early Renaissance Italy, as well as to Minorities Economics and the late Scholastic thought, pointed at *periculum* (risk and uncertainty) as the fundamental element which legitimated merchant (business) profits. Not by chance it was a French contemporary of Smith, Richard Cantillon, who introduced the term entrepreneur, only much later popularized by J.S. Mill among British economists (1848). And Mill however failed to break the Smith-Ricardo tradition of the entrepreneur as simply a ‘multifaceted capitalist’ (Blaug 2000: 80).

Theoretical attitude would not change much, but for a few major exceptions²⁷, in the next century. With regard to the continental side, on the one hand the Austrian Economic School coherently developed a line of research that emphasizes individual action and hence deliberately appreciates the role of the entrepreneur (Kirzner 1973); on the other, social scientists/economists like, again, Weber and Sombart produced major efforts to offer typologies and/or taxonomies of all the characters acting in the economy, therefore comprehensive of entrepreneurs. As for the analytic front, within the neo-classic static modelling entrepreneurship kept on being considered an useless false glitter or, at most, a residual – i.e. the elusive world where the growth accounting theory quarantined all that could not be explained through its classic analytical tool, how much important it could be. However, all over the century institutionalism kept on exerting its influence on a minor but nevertheless not negligible stream of thought. As a consequence a growing attention to the firm as an organized institution began to be paid (Veblen 1904, Berle & Means 1932, Coase 1937, Penrose 1959): the dynamics of such an institution was to become a major field of research in business history but, as said, not sufficient to avoid the split between economic and business history.

6. The crisis of the neo-empiricist model

A 1991 study on the characteristics of the articles published in the first half century of the *Journal of Economic History* offers some quite impressive results: the percentage of rigorously cliometric articles published by the journal grew from 10.2% of the total from 1946 to 1950; 42.8% from 1966 to 1970; 78.9% of from 1976 to 1980 and finally to a maximum of 80.5% from 1985 to 1990 (Whaples 1991): no need to say that the greatest part was based on neoclassic theorizing and modelling. The heyday of the neoclassic-based economic history corresponded to

²⁷ Such as e.g. Marshall (1890) and Knight (1921)

the moment of the largest gap and the strongest dichotomy between the analytical and the continental schools.

Therefore the belief in Hempel's *covering laws* and in the heuristic capabilities of explanatory models based on the natural sciences and positivistic logic remained unchallenged for a long time across the Atlantic. But the social disciplines, being younger than the scientific ones, are exposed to the risk of subordination to them. Therefore any analogical transposition of physical science models into social science would result in the well-known tension between theoretic models and historical reality: a tension sparked off by concepts that until a few decades ago were the very nucleus of science - equilibrium, determinism and reversibility - with accidental, unbalancing factors pushed into the background.

For some time, however, even in American analytic circles, some critical reflections have emerged with regard to two basic dogmas of the 'scientific view of the world' associated with Carnap: separation in the cognitive process between subject and object and the fragmentation of experimental facts. It is a belief that was confirmed even in front of the aims of going beyond the neo-positivist epistemology as exerted in the Sixties by Kuhn, Lakatos and Feyerabend (and even earlier by Popper). Here, to some extent, even irrational elements seem to have wedged themselves into the rational foundations of scientific doctrines. Nor can we ignore the fact that more or less simultaneously, in the British analytic camp, the 'post-empiricist' epistemological shift was maturing. This shift radically questioned the dichotomy between human sciences and natural science, given that such a dichotomy presupposed an empiricist vision of natural science almost universally discredited. The very language of the natural sciences is irreducibly metaphorical and inexact, and can be formalized only by distorting the dynamics of its historical development (Hesse 1980: 171-3) [Actually, we can note in this shift the premises for an end to the dichotomy between analytic style and continental style that has up to this point acted as guideline to this paper]. Indeed, rather than discuss the differences between the two epistemologies, the latest scientific-philosophical developments seem to indicate they have been overcome, through the discovery of a new epistemology based on a possible 'new alliance' between natural science and human sciences (Prigogine - Stengers 1979) as would appear to be taking place in the sciences of complexity (Toninelli 1987).

It is a new alliance which the old continent's social sciences have also been urged to take into account - thus including the various streams of economic history. At the outset of the new millennium the complexity challenge has no geographical, sectorial or disciplinary boundaries; on the contrary, its transversality is its characterizing trait. (See e.g. Prigogine et al. 1986, Nicolis - Prigogine 1991, Morin 1993, Bocchi-Ceruti 1985). Certainly the continental school, less conditioned by the epistemological neo-empiricist model, seems an easier path to

follow²⁸. In fact on the side of "hard" science and the theories aspiring to it (in which I would include general economic equilibrium, thus cliometrics too) this possible coming together of different fields of knowledge demands some hitherto unimaginable sacrifices - the reversibility principle, for instance - and the acceptance of time and space variables as constitutive parts of analysis. However, even the "weak" human sciences - in which I would also include continental economic history - are obliged to make a quantum leap: thus both to ask themselves the question of conceptualization and theory (as humanist scholars *par excellence*, such as literary and art historians, have been trying to do for some time) and to realize that "complexity" is something different, more difficult to grasp, than "totality". Besides, the Sixties and Seventies were years of great reflection and enrichment also for the continental philosophical schools, and this has had important effects on the various streams of continental economic history as well. In this respect we only have to think of the changes that have occurred in the *Annales* school, the developments of the so-called "third generation" and the influence exerted on it by Levi-Strauss and Foucault in the heyday of French structuralism

The epistemological developments of the nineteen sixties and seventies have not been entirely ignored by economic theory, even inside the mainstream identifiable in the several schools deriving from the marginalist-neoclassical current (Barrotta-Raffaelli 1998). If in some heterodox currents, criticism of the epistemological approach inspired by the physical sciences comes slightly after the crisis of the neo-empiricist paradigm (see f.e. Georgescu-Roegen 1971, 1976), towards the middle of the Eighties even Robert Solow, one of the leading proponents of 'pure' economics, on the occasion of a meeting of the American Economic Association, somewhat unwillingly agreed that «one will have to recognize that the validity of an economic model may depend on the social context» (Solow 1985:331). Certainly more provocative for its strong Feyerabendian echoes was the essay published in 1985 by D. McCloskey (McCloskey 1985, but see also McCloskey 2001): an outright manifesto against the positive economics method, written no less by a representative of the Chicago school who for years, by his own admission, had fought in the 'positivist brigade'. Reacting to the excesses of positivism, this appeal in defense of rhetoric in economics and economic history can also be read as an attempt to bridge the gap between analytic style and continental style in the two disciplines. But, generally speaking economic theory appears today more diversified and pluralistic. As noted by Avner Greif (1997, p.401), «theoretical developments and empirical evidence led both micro- and macroeconomic theory to recognize the futility of seeking a single

²⁸ In this regard one has to agree with Immanuel Wallerstein when he says that "the conceptual picture offered by complex evolutionary systems, as developed by the natural sciences, presents the social sciences with a coherent set of ideas that goes well with visions long present among the social sciences, particularly among those that have resisted the forms of nomothetic analysis inspired by the science of linear equilibria" (Wallerstein 1997: 59).

universally applicable economic model». While institutions, asymmetric information, bounded rationality, multiple equilibria represent new formidable challenges for the theorist, even among mainstream economists there is less consensus about the very nature of economic laws (Demeulemeester-Diebolt, 2007).

Indeed, the American scene appears more diversified today even where economic history is concerned. The most novel changes come from the history of technology, where for some time the contributions of Nathan Rosenberg (1976, 1982, 1994) and Paul David (1985, 1992, 2007) have been directed towards a 'strategic repositioning' of time and space variables, so as to restore dynamicity to the studies of historical topics. Innovations in a pure Schumpeterian spirit are significant here (Freeman-Louça 2002). To the concepts of technological convergence, learning by doing and path-dependence - developed by the two authors and now become common property for the discipline - can be added that of 'punctuated equilibria' imported from the sciences of complexity in the analysis of technological change (Mokyr 1990). This concept attempts to reconcile continuity and discontinuity in the processes of technological (and economic) transformation, reinterpreting in an original way the models of modern evolutionary macro-biology in which phases of gradual, sequential and adaptive evolution (Darwinian, therefore) are punctuated with profound changes (bifurcations generating new species). Apart from epistemological analogy, such studies in my opinion run along the lines of that minor but most meaningful research for the future of the discipline, which has always striven to find an autonomous set of methodological rules for economic history, that is a set of rules that would reconcile the requirements of deep conceptual study with historical explanation, and thus walk the tightrope between history and economics, proposing models and original generalizations 'typical' of economic history. Alexander Gerschenkron, Walt Rostow, Ronald Hartwell and H.J.Habbakuk contributed in the past to the development of the discipline along this line. Today, apart from the authors of the Schumpeterian tradition, also the economic historians of the neo-institutional school can be located within this perspective.

Yet something seems to be moving on the 'hard' economic history front, too. In the very periodical that has long been the standard-bearer of cliometrics - the *Journal* - as I have said, the last fifteen years saw a few contributions by authoritative representatives of cliometrics, favorable to 'humanistic' openings in American economic history. For example, in the essay already cited, Sutch places as a future objective of the discipline that of integrating economic history back into the discipline of history (Sutch 1991:277-8). On his part, in his opening address to the 1996 meeting of the Economic History Association, with the intriguing title 'Is it kosher to talk about culture?' (Temin 1997), Peter Temin recognizes that the different cultures had a determining role in forging the different way the Anglo-Saxon countries approached industrialization compared with Japan. He

recommends therefore that both economists and economic historians should devote more attention to the cultural aspects in their analyses of economic development²⁹. These range from McCloskey' s (1998) plea for encompassing "S variables" in economic (history) analysis to Wright' s (1999) questioning about the Civil Rights Revolution as an economic revolution for the South; from Neal' s (2000) proposal to address economic history research more to shocks than normal change, to Goldin' s (2001) emphasis on vices and virtues of the US process of human capital formation.

Besides, at a more general level, a number of methodological contributions reflecting on the nature and the boundaries of the discipline tend to suggest the viability of a more eclectic approach able not only to bridge the divide between history and economics but also to transform economic history into «a cure for economics» (see e.g. Greif 1997 and 2006; North 1997; North-Wallis-Reingast 2006; Wright 2001 and 2006).

Finally a timid re-approaching between economic history and business history seems to go on, although the researchers really working on both fronts are still quite few. However it should not be underrated the fact that the NBER is on the front line in pursuing this interdisciplinary effort and that «...recent developments in economic theory provide a historic opportunity for greater communication» between the two fields (Lamoreaux *et al.*, 1999: 5).

7. Academic economics and sociology matters

It would be misleading to link the explanation of the different paths followed by economic history on the two sides of the Atlantic, as well as recent changing attitudes in the US, exclusively to S factors (stretching McCloskey' s language to encompass cultural and philosophical aspects in S). As I have said, issues concerning academic sociology and economics most probably lie behind theoretical and epistemological issues – above all where the academic chair market is concerned. This emerges quite clearly from most interviews given by outstanding exponents of the discipline and published in recent years in that mouthpiece of economic historians, *The Newsletter of the Cliometric Society*. According to them, in comparison with the greater dynamism of a university system like the American one, open to market forces and private financing, and ready to embrace novelty, is counterpoised the sluggishness of continental universities, mainly public, where the rigidity of the curricula and academic careers tend to perpetuate consolidated traditions of research (Temin 1999:42-4; David, 1999: 25-7; Ransom 2000:10-2; Engerman 2000:7-8; Williamson 2000; Hartwell 2001:9; McCloskey

²⁹ In a recent interview Temin carries his point even further: «I would say that the mid Seventies may be the high point of, let's call it, pure econometrics, in the sense that all you needed to know was the economic theory and that, if you just understood the competitive model, you could deal with any problem at all. I think that since then there has been somewhat of a retreat from this ... It was not a decline in the use of economic models and evidence, but rather it was erosion of the belief that these were the only kinds of evidence that were relevant. It was the beginning of a notion that it's not Us and Them, but maybe that it's together that we need to do this» (Temin 1999: 41).

2001:9-10; Sutch 2001:10; Rockoff: 2003,9). Sam Williamson has something significant to say about this when he recalls how, just before the 'cliometric revolution', economy theory itself was rapidly replacing economic institutionalism, which had been dominant until then. A consequence not to be overlooked was that «traditional economic history was regarded as one of the most non-rigorous parts of the field, and was being dropped from many graduate departments» (Williamson 1991:21). With their readiness to enter into dialogue with economists, whose stress was on a more rigorous analysis based on theory and who were able to set up of wide-ranging researches, cliometricians were immediately able to answer the needs of the economy departments. Besides, starting from 1958, they had shown their versatility in using the computer (Hughes-Reiter 1998), an innovation that had soon won the attention of scholars in America, even outside rigorously scientific fields of research. So it was that from the late Sixties onwards, the 'new' economic historians were easily able to find employment in rapidly expanding economics departments. In the Seventies, while 'a terrible depression' fell on career possibilities for young researchers with traditional post-graduate specialization in history, demand for cliometric historians remained high, with the result that they got the lion's share of new appointments, quickly obtaining tenure (on average two years ahead of other historians - Fogel 1996:7). Academic success was accompanied by success in obtaining public and private financing both for research programs with a strong quantitative stamp, and for congresses and seminars.

For a few years, on the contrary, some signs of tiredness and disillusion have begun to appear also where 'academic' success is concerned³⁰. In his reconstruction of the history of cliometrics, Williamson recalls the high costs the discipline had to bear in order to be accepted in economics departments and to be able to share the same status as other branches of applied economics. The most serious of these costs came from considering cliometrics nothing more than a new branch of applied economics. For Williamson this comparison meant that since for a cliometric 'when it comes time for promotion and tenure, the rest of an economics department may not appreciate the difficulties of working in the archives, or for that matter even working in the library, the cliometrician's work is judged on the basis of what the rest of the department knows - the theory and quantitative tests used' (Williamson 1991:26) This has at times led new historians to publish works capable of competing with their economist colleagues but deficient in historical grounding. Instead, for Williamson, a cliometrician's ability should be evaluated on the basis of his knowledge of the instruments and methods of history,

³⁰ According to Temin, «the market for economic historians has not been a particularly buoyant one in recent years. There is a steady demand for economic historians but, while the demand for economists has been growing, the demand for economic historians has been pretty stagnant, with one or two economic historians at each school and, in fact, we like economic historians who can also teach money and banking or macro or labor or econometrics or whatever. And so it has been hard to attract people into economic history as a professional activity» (Temin 1999: 43)

geography and demography, not only on his ability to converse with other economists.

8. Conclusions

This long parenthesis devoted to cliometrics, and the epistemological model that inspired it, has necessarily overshadowed the contrast between analytic style and continental style that was the guideline in the first part of the paper. Yet it can be justified by the fact that, as I have tried to demonstrate so far, this line of study can not only be identified as the paradigmatic transposition into the discipline of the analytic style but also because it can at least be given the merit of having raised extremely important methodological questions. Perhaps not always accepted (or understood), they have at least set in motion a process of critical reflection inside the whole of economic history.

It is also true that, as I have already underlined, the discussion concerning method, especially when carried on beyond the borders of the North American continent, has taken on tones of a clash and rigid opposition rather than a dialogue and comparison. It is also true that to a large extent such rigidity can be ascribed to the sectarianism and aggressiveness of the ‘new’ economic historians, who as Fogel himself recognizes *ex post*, «were young, inexperienced and often did not respect the etiquette of scientific debate»³¹. [It is worth noting how such attitudes find a correspondence in ‘certain unpleasant characteristics that tend to thrive in analytic circles: scientism, sectarianism and a refusal to contemplate other forms of philosophy’ (Marconi 1997:6).]

It may however be worth pointing out at least a couple of points for further study: first of all the disappearance of what had been on the whole also a geographical identification in the opposition between analytic economic history and continental economic history. It is without doubt that in various continental countries (Spain and Germany, above all, but also in Italy and France itself) important lines of study have recently developed in historical economics that might be capable of giving further stimulus to the process of self-awareness set in motion by American cliometrics.

Secondly, as mentioned, since the Seventies an intense phase of rethinking and redefining objectives and methods of continental economic (and social) history seems to have opened up. A maturing blend of Marxist and Braudelian approaches has fueled a stream of studies of mega-economic history, i.e. of economic history seen as a world process in the long run, in which the long-prevailing Euro-centric bias has been challenged by a novel emphasis on the

³¹ Fogel 1996:6. In Peter Temin’s words: «...we were St. George and the conventional historians were the dragons, and we had to slay them. It was very definitely set up a contest between Us and Them, very much a feeling of Them and Us. Other people have talked about the problems with the *Journal of economic History* and how it was going to bridge these cultures. But within the cliometrics meetings, there was no feeling that we had to bridge the cultures: we were the true believers» (Temin 1999: 5)

development of the rest of the world (O'Brien 2006). Besides, the contemporary outburst of globalization and of its related economic and socio-cultural matters has attracted a growing interest from a number of dynamic Anglo-American economic historians such as Jeffery Williamson, Alan Taylor and Niall Ferguson: the 'new global history' tries to combine successfully the cliometric methodology with wide-ranging objectives of social, cultural and political nature.

It may be impossible to claim for economic history what was claimed for historiography in general some time ago, that is, that «mutually fruitful relations» now prevail «completely ... over the specific characteristics of the historical culture of the single countries» and that «orientations and research methods» in history «have ceased to be a national peculiarity» (Rossi 1987: XVIII). Perhaps, more than fertilization and historiographic *koiné*, what is projected for economic history, as for many other fields of knowledge, from philosophy to economics itself, is a cohabitation, a pluralism of methods and many different lines of study, able to overcome any rigid epistemological and methodological opposition from within.

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