Economic history has always been quite a peculiar department both in the domain of history and that of economics; dealing with change, institutions, collective rationality, and conflicting strategies of economic agents, privileging descriptive and non-formal analytical tools, economic history remained for long outside the scope of formal neoclassical economics.

This chapter describes and discusses the story of the incorporation of economic history into the mainstream of economic theory through the cliometric revolution, a powerful intellectual movement emerging by the late fifties, which encapsulated this reconstruction of economic history from the point of view of marginalist price theory and the postulates of individual rationality; Meyer and Conrad were the major drivers of this radical vision, and challenged the ‘old
historians’ school’, today best represented by the response of David Landes.

Yet the coherence of the cliometric movement was soon jeopardized by internal contradictions: Paul David issued the most powerful challenge to the seminal building block of the new approach, Fogel’s ‘Time on the Cross’, a revision of the traditional approach to the economics of slavery in the pre-Civil War USA.

Douglass North is another example of a dissident from cliometrics, and Alfred Chandler provided alternative arguments for a reasoned history approach to societal change.

The cliometric analysis of the British Industrial Revolution, using counterfactuals, namely by Crafts and Hawke, is discussed and contradicted in the chapter.

Keywords: chance, cliometrics, counterfactual, Paul David, economic history, economic theory, Fogel, Industrial Revolution, David Landes, Robert

1.1 Introduction
This book starts with a thorough discussion of methods in history, in economics, and in the social sciences generally, because this is essential for the theme of the whole book. Our approach to the Industrial Revolution and to the information revolution depends on a distinctive method, which is developed in contradistinction to the prevailing dominant methods in both economics and economic history. The best way to demonstrate this difference is to analyse that curious intellectual movement known as ‘cliometrics’, which was so important for economic history in the second half of the twentieth century.

Cliometrics erupted into the intellectual arena of economic history at the end of the 1950s, and in a short while became one of the dominant forces in the field. In the economics profession, used to scorning epistemology despite proudly claiming to be united by a specific method, supposed to single it out among the social sciences, cliometricians discussed the philosophy and the consistency of their endeavour and provided new teachings and surprising applications. They generated new rules of investigation, achieved increasing returns for publication, created novelty, and assured conquest, often through the harsh polemics sometimes thought to be necessary for achieving brilliance in academia. In a science
balkanized into several schools, cliometrics brought together a heterogeneous group of researchers and disciplined their efforts to the performance of a single major task: to redefine and quantify economic history.

Last but not least, the cliometricians applied cosmopolitan neoclassical economics, invading the resistant small village of history—where Schmoller, and later Schumpeter, Mitchell, and so many others, had taken refuge in order to publish manifestos in an outspoken reformist vein. These refugees had also conducted a great deal of plebeian work in favour of regarding economics as part of the social sciences, as a realist science, and even as a science at all, but the invaders suggested a new and fashionable approach to economic history.

This chapter briefly discusses some of the epistemic foundations of cliometrics, its methods and accomplishments, and the divided legacy of economic history after it had gained primacy. The next section summarizes the (p.10) history of cliometrics, arguing that the movement itself was split after the first skirmishes with ‘old’ economic history, while Section 1.3 focuses on the method and on some examples of its application. This provides the opportunity for a general discussion as to which kind of statistical tools are adequate for economic research, which is one of the major purposes of this book. Finally, the conclusion restates the case for history to be included as a part of economics, so that it can deal with its very object: real life economies in evolutionary, irreversible, and complex processes.

1.2 Cliometrics: How Quantification and Economic Theory Conquered Economic History

As the tale goes, cliometrics was born in September 1957, at the National Bureau of Economic Research (NBER) Conference on Income and Wealth, in Williamstown, Massachusetts, and John Meyer and Alfred Conrad were its progenitors (e.g. Fogel 1966: 642; Davis 1966: 658). Within a few years, it had become the epitome for the close integration of economic history into mainstream economics.

Indeed, this conference proved to be a major turning point. Two years earlier, Solomon Fabricant had sent an open invitation to economists and historians to attend a joint meeting; the Economic History Association accepted, and
Gerschenkron acted as a co-organizer. During the meeting, two papers constituted the manifesto for the new current, one prepared by John Meyer, himself a director of NBER later on, and one by Alfred Conrad, both authors of which were from Harvard. And their new ideas inflamed the field.

The offensive against ‘old’ economic history was apparently prepared as a rigorous campaign, as far as one can surmise at the distance of one generation. If the order of authorship reveals their share of the job, Meyer took the lead in epistemology, while Conrad, acting as the first author; delivered a major example of the new approach, conducting a study of the profitability of slavery in the Antebellum South. The final result was a consistent case for the new approach: the papers were simultaneously challenging and daring, powerful and insinuating, concrete and theoretical, exploring new and old hypotheses in a new light.

Twenty years later, Meyer stated, in correspondence with Bob Coats, that they did not intend to revolutionize the métier, and that they identified themselves as disciples of the quantitative tradition in American economic history, as represented by Schumpeter, Kuznets, and Gerschenkron (4 October 1977 letter to Coats; quoted in Coats 1980: 187). Reading the material from the conference, however, one cannot avoid feeling a sense of confrontation and scepticism amidst novelty.

Rostow, who presented a paper on ‘The Interrelation of Theory and Economic History’, rephrased the old resistance of historians to traditional economic theory, arguing that static assumptions and methods were clearly unsuited to the subject-matter of the historian:

For a theorist it is fair enough to say, as Marshall did, that a case of increasing returns is ‘deprived of practical interest by the inapplicability of the Statical Method’, but this is a curiously chill definition of practical interest for an historian. . . . In short, if the work of the economist is to be relevant, he must work to an important degree outside the theoretical structures that have mainly interested him since, say, J. S. Mill. (Rostow 1957: 515, 517)
Moreover, Rostow identified the nature of the problem as the inappropriate application of the ‘Newtonian methods’ to economics, conceived as another branch of mechanics, whereas biological metaphors were more appropriate—as Mitchell and Schumpeter had suggested in due time (Rostow 1957: 514, 519). Yet, Rostow did not hide his lack of interest in methodology, a domain of irretrievably personal and subjective choices: ‘I do not hold much with ardent debate about method. A historian's method is as individual—and private—a matter as a novelist's style’ (p. 509). Following this argument, the whole discussion was a waste of energies.

But Rostow's colleagues did not think this way. The panel of discussants of both Rostow and Meyer–Conrad's papers on method included Martin Bronfenbrenner, Raymond de Roover, Evsey Domar, Douglass North, P. G. Ohlin, and Arthur Smithies. Simon Kuznets performed the task of summarizing, commenting on, and concluding the discussion, and his notes are highly indicative of the interest aroused by the topic and of the intellectual climate of the meeting.

According to Kuznets, one of the putative inspirations of the new movement, the discussants shared some preoccupations, but disagreed on a number of points. Curiously enough, one of their major points of agreement was the fear of an excessive reliance on economic theory, one of the ideas stressed by Meyer and Conrad, since ‘the discussants were concerned chiefly with the specific theory to be used, the implication being that there is no single accepted body of economic theory. . . . [Furthermore] in view of the great complexity of the problem faced by economic historians. . . . allowance must be made for various approaches in formulating hypotheses’ (Kuznets 1957: 545–6, 547). We will return to this argument against a theoretical bias in the integration of history and economics, but it is important to emphasize now how these economists and historians at this stage devalued the theoretical corpus they were offered and instead stressed pluralism. Following Kuznets's testimony, the participants at the conference did not rate the announced contribution by economic theory very highly:
Traditional economic theory is of little help [for the choice of variables] since it is devoted largely to drawing implications from a sharply defined and correspondingly abstract system of market relations, whereas the economic historian perforce emphasizes the institutional changes that affect the scope and conditions within which the market system operates. . . . It would be dangerously confining to demand that each (p.12) of these causal sequences be expressible as a mathematical equation with all the variables quantified and with the selected functions simple enough to permit the kind of testing called for in this paper. (Kuznets 1957: 547–8)

Of course, economists engage in such imaginative castle building in the belief that there will be sufficient resemblance to reality for the model to be useful in attempts to deal with explanatory or policy problems. In fact, in the designing of these tools a fair amount of empirical substance is incorporated; and all too often without explicit proof of its validity. (Kuznets 1957: 551)

As a consequence, there were two grounds for suspicion in relation to the new approach. Some economists at the conference—indeed, most of them—rejected with outspoken contempt the applicability of the dominant methods of theorizing and modelling to history: Kuznets indicates that Bronfenbrenner, Smithies, and Domar were ‘rather sceptical of the value of greater integration of economic theory and economic history, and particularly of the use of econometric models and statistical tests’ (Kuznets 1957: 550). And the historians, at least some of them, feared that the new approach could imply a reductionist vision of history. In spite of this, other historians were more receptive to the new approach. In particular, Douglass North and Gerschenkron welcomed the new development, and the former played a crucial role in it.¹ Kuznets explained this paradoxical fascination in the following terms:
It is easy to exaggerate the impression, but it did seem as if almost all economic theorists participating in the [Williamstown] discussion were doubtful of the value of theory in work on economic history, while at least some economic historians felt that it is needed. And perhaps there is a simple explanation for this somewhat paradoxical situation. Scholars working in the field of economic analysis are all too aware of the limitations of their tools, [while] those working in the field of economic history in which some of these tools have not been widely used, tend to appraise highly the possible returns from such use. (Kuznets 1957: 550)

Although the economists remained sceptical, and some of the historians rather worried, the conference marked the emergence of a constituted body of thought, of a mature methodological approach, supported by some well argued empirical applications. That was all that was needed for inaugurating a school; and a school was indeed inaugurated.

The publication of the volumes of the conference, and the rapid diffusion of the twin manifestos (the slavery paper appeared in the *Journal of Political Economy* in the same year and the methodological one in the *Journal of Economic History* in 1958), paved the way for the ‘new economic history’, or ‘econometric history’, as it was called. From 1960 on, the seminars at Purdue (p.13) (with Lance Davis), the Washington group (led by North), the Harvard team (Gerschenkron), and Robert Fogel, as well as other research units at Yale (William Parker), Wisconsin, Pennsylvania, and the Stanford-Berkeley joint Economic History colloquium, were the major poles for the school (Fogel 1966: 643; Coats 1980: 197). According to Lance Davis, Douglass North was by that time its ‘chief propagandist and entrepreneur’ (Davis 1966: 659).

The counterattack was messy and timorous. The elders were intimidated by the mathematical paraphernalia of the young and did not challenge either their method, which they could barely understand, or their empirical work, which they could scarcely recognize. Indeed, ‘many of them withdrew into a kind of internal exile within the profession’ (Landes 1978: 5). Furthermore, a controversial editorial decision sparked the confrontation. When a paper by Lance Davis was rejected by the *Journal of Economic History*, the young economic
historians summoned their troops in order to claim justice: petitions were circulated, meetings were organized, and the paper was eventually published in 1960. Shortly afterwards, the editors of the journal were replaced. When North and Parker were later appointed as editors of the *Journal of Economic History*, the signal was made quite clear.

Yet, evidence suggests a mild interpretation for the result of the whole process: in spite of some accusations of ‘quantomania’ and ‘numerology’ levelled against the new economic historians, they were left free to conquer the intellectual terrain almost without battle. In a matter of a few years, cliometrics had become a powerful new orthodoxy in economic history.²

Meyer and Conrad: Opening the Pandora's Box of Methodology

It is now time to present and discuss the content of Conrad and Meyer's arguments. The paper set as its purpose the discussion of the ‘concepts of historical causality and explanation in a stochastic universe and to suggest how the analytical tools of scientific inference can be applied in economic historiography’ (Meyer and Conrad 1957: 524). This was by itself quite new. The prevailing wisdom was the opposite vision to that of Samuelson, (p.14) established in his path-breaking 1947 Ph.D. dissertation: causal models and historical models belong to two distinctive and unrelated classes of approach (Samuelson 1983: 272). But the effort to challenge this common assumption and to rebuild economic history in order to model historical processes by stochastic causal systems was shown to be highly profitable.

Based on Hempel (1942), Meyer and Conrad provided a strong version of the positivistic approach: theories should be compared to predictions in order to increase the explanatory value of theoretical assertions through their eventual confirmation (Caldwell 1982: 20, 26–7, 175 ff.). This vindication of logical empiricism was quite exceptional in economics, not because its implicit mode of legitimization was not shared by most, but because the majority of the profession were all too innocent of epistemology.

Denying Popper's distinction between historical disciplines and natural sciences, Meyer and Conrad established an ambitious programme for the integration of economic history into economics—through the assumption and acceptance of
the neoclassical framework—and for the corresponding
definition of economics as a science, following the Newtonian
atomistic mould. Again, this was pure Hempel: all sciences
were definable by the Hypothetico-Deductive Model, and
therefore all could resort to a causal ordering system.
Consequently, science— all sciences— should be able to equate
explanation and prediction through the formulation of a
general law: ‘general laws have quite analogous functions in
history and in the natural sciences . . . they even constitute the
common basis of various procedures which are often
considered as characteristics of the social in contradistinction
to the natural sciences’ (Hempel 1942: 345, 348).

In particular, the ‘irretrievable pastness’, or the irreversibility
of historical events, would not prevent the use of the proper
scientific methods and the definition of causality, provided two
conditions were met. The first was the invariant— either
sequential or simultaneous— conjunction of two properties,
and the second was its asymmetrical character. Now, as the
first condition is rather demanding, since it requires no less
than the perfect conditions for establishing a covering law,
Meyer and Conrad rephrased it in probabilistic terms: all that
is required is that we should be able ‘to assign some
probability to the assertion that the “first” set of conditions
will be followed by a “second” . . . and that this process should
be irreversible’ (Meyer and Conrad 1957: 528). In that case,
causal ordering, or the identification of exogenous and
endogenous variables, could be established. Of course,
identification is relative to a theory: ‘in history, no less than in
any other branch of empirical inquiry, scientific explanation
can be achieved only by means of suitable general hypotheses
or by theories, which are bodies of systematically related
hypotheses’ (Hempel 1942: 352).

But it is not only the reference to the general theory that
suggests a difficulty. The crux of the matter is the stochastic
element. The structure of the model is deterministic, whereas
the random events refer to changes in the course of history:
(p.15)
In formal terms, this implies that while historical explanation does presuppose regularity, it must be assumed that the random elements will dominate the causal system and that the random elements are differently distributed at every moment of historical time. . . . But explanation in a historical system can be interpreted as the estimation of probabilities of transition from one state to a succeeding state, given the initial conditions and a causal law or generalization. In that interpretation the task of the economic historian is to search out the variations in the exogenous variables, that is, to add to the set of empirically realized independent conditions. (Meyer and Conrad 1957: 530–1)

This ‘stochastic causal system’ should therefore be interpretable following the three main elements: (1) the ‘causes’, the exogenous variables, (2) the ‘effects’, the endogenous variables defined by a specific structure, and (3) the random shocks, giving the ‘proper meaning of the uniqueness of historical events’ (Meyer and Conrad 1957: 532), and the hypothesis should be stated in terms of the equations representing the three classes of variables. It should be emphasized that all three classes must necessarily co-exist in this framework: it is the characteristics of the random element that allows for the probabilistic inference, as it is the exogenous-endogenous dichotomic distinction that allows for meaningful causal assertions in this framework.

At the same time, the hidden assumptions of these methods did not frighten the cliometricians; they even depicted themselves as objects of jealousy in the eyes of their fellow economic historians, who were unable to prevent a general movement towards a positivistic revolution in social sciences. This is how Fogel and his associate Engerman presented the barbaric challenge:
To many humanists this effort to treat man as if he were an atom is the ultimate folly. It takes no great effort on their part to ignore such prattle. And that is what many humanists do, except for an occasional snicker in the privacy of their studies when someone mistakenly sends them a reprint of a paper containing a mathematical model of the French Revolution. . . . It was not only the economists who invaded that field in the late 1950s; they were joined by sociologists, political scientists and others. All were armed to the teeth with statistical methods, computer programs, and mathematical models of human behavior. The main body of historians attempted to ignore this incursion, on the assumption that the invaders would flee in retreat when they realized the strength of their opposition, or else, as was true of so many previous barbarian intruders, they would have become assimilated. (Fogel and Engerman 1974: 8–9; emphasis added)

Fogel and Engerman were careful enough to distinguish themselves from previous attempts to mathematize the discipline, arguing that these early efforts were wrongly based on nineteenth-century physics and the presumptions of the First Law of Thermodynamics, asserting the stability of equilibrium as the solution for the system of equations. Apparently, they ignored the fact that neoclassical economics itself was born precisely as the incarnation of energetics in economics, and that its revival in the 1930s under the spell of the probabilistic revolution did not cut such an umbilical cord. As a consequence, agents were still modelled as atoms, as Fogel and Engerman clearly state. One may, however, ask what kind of behavioural assumptions are possible for these atoms, and how a necessarily limited mathematical model can represent the French Revolution.

It comes as no surprise, therefore, that participants at the Williamstown conference met these claims with great scepticism, particularly in relation to the usefulness of the stochastic approach to economic history.
A Major Shift in Economic History

In spite of such opposition, cliometrics won the day. In retrospect, two reasons may help to explain this outstanding result. For one, it represented the emergence of positivism in economic history, and positivism had been regarded as the hallmark of the scientific character of any discipline since the end of the nineteenth century. Indeed, both in economics and in history, and *a fortiori* in economic history, many had resisted positivism and its overemphasis on measuring and counting for a long time. Adam Smith was an historian as well as a moral or political economist, and it was only afterwards, with Ricardo and Say, that axiomatics took over. But for many years the prevailing view in economic history was that it was designed for the task of counteracting the abstraction and irrelevance of mainstream economics, and Schmoller, Toynbee, and others considered their work as a refuge for realistic economics and as the condition for rehabilitating history. Schumpeter, the most heterodox of the orthodox, proclaimed history as an intrinsic part of the programmatic task of economics and, in his afterthoughts following his first survey of the Methodenstreit, asked for a truce with Schmoller (Louçã 1997: 239 ff. and see also Chapter 2 below).

In general, the allegiance of economic history to realism, and its ambitious concern with the overall reform of a science undercut by reliance on neoclassical behavioural assumptions, led economic historians to take preventive measures to preserve their work from the influence of economics (Schabas 1995: 198). On the other side of the coin, modern economics was defined primarily in opposition to history and its pluralistic tradition: in his ‘Plea for the Creation of a Curriculum in Economics’, Marshall argued in 1902 for the positivistic criteria of a mathematical science as opposed to the still dominant narrative techniques (Kadish 1989: 200). Consequently, the methodology of economics was taken from physics, and economic history was kept separate, to the benefit of both parties.

This situation was challenged when the epistemic primacy and theoretical corpus of neoclassical economics were well established, after Debreu and Arrow defined the rigorous conditions for general equilibrium. The powerful movement of
cliometrics represented that very wave of integration: economics claimed history back into its province.

A second reason for the victory of cliometrics, and not a minor one, was that it challenged young researchers to use new and fancy techniques, and (p.17) generated increasing returns to the investment in the sophisticated ‘new economic history’. They proclaimed the obsolescence of the elders (Landes 1978: 4), and abandoned their programme: history was no longer seen as a form of research into evolution and institutions, but rather as the story of a macroeconomic process resulting from the action of innumerable essentially identical and well behaved atoms. This allowed for computation and fancy papers, yet it was soon obvious that the school itself was sharply divided into several undercurrents.

As early as 1965, Fritz Redlich, a retired Harvard professor, wrote a critical assessment of the cliometric school and distinguished its three main undercurrents. First, as he put it, were the ‘data processing’ scholars, such as Davis, Hughes, and Fishlow, mainly interested in gathering information as the quantitative historians did before, although using more sophisticated statistical tools. Second was the work of North, whose product ‘is economic history’ and a thorough discussion of hypotheses. Third, there was the ‘quasi-history’ group: Conrad, Meyer, Fogel—essentially model builders working with figments of imagination, with hypotheses that were neither refutable nor verifiable, and simply required justification (Redlich 1965: 491).

Furthermore, Redlich noted that the latter used two distinctive classes of models: ‘models by reduction’ (based on empirical material, as the production functions in Section III of the Conrad–Meyer paper), and ‘models by construction’ (as in the remainder of the Conrad–Meyer paper and in Fogel’s work). In both cases, ‘a model is never a piece of history, because it is conjectural or subjunctive or, in Max Weber’s language used for all ideal types, a distortion of reality’ (Redlich 1965: 490).³ One may therefore wonder (and this was Redlich’s line of attack) what clarity can be gained from obviously false assumptions.

This difference widened as time passed. As a consequence, the founders were forced to accept a shared judgement: cliometrics led both to good results and to ‘sheer disaster’ in
its application (Davis 1966: 662) and to ‘dull and unimaginative’, ‘imprecise and fuzzy’ research of ‘distressing poor quality’ (North 1965: 90). These differences eventually led to an internal civil war, under the form of aggressive reviews and other more personal attacks (McCloskey 1978: 23).

These different approaches to cliometrics reveal a great deal about the varied intentions of the respective authors. Originally, Fogel claimed that cliometrics distinguished itself by its being a specific method for the rein-corporation of history into economics, subsumed to its general theory, i.e. the basic neoclassical assumptions: ‘The methodological hallmarks of the new economic history are its emphasis on measurement and its recognition of the intimate relationship between measurement and theory’ (Fogel 1966: 651).

McCloskey was even more conclusive, stating that cliometrics was defined by a ‘deep agreement’ about the neoclassical equilibrium price theory:

It is the possession of this method that distinguishes the cliometricians from other economic historians. . . . Not counting but economic theory, especially the theory of price, is the defining skill of cliometricians, as of other economists. A cliometrician is an economist applying economic theory (usually simple) to historical facts (not always quantitative) in the interest of history (not economics). (McCloskey 1978: 15)

This was indeed widely accepted: when a major figure in the econometric movement, Arnold Zellner, proposed the candidature of Fogel as a new Fellow of the Econometric Society, his argument was that ‘Fogel is a recognized leader in the movement to introduce econometric methodology in the field of economic history’ and ‘his work has been an important factor in the encouragement of the use of economic theory in economic history’. Yet, Fogel argued that the impact of the new economic history was ‘primarily due to the novelty of its substantial findings’ (Fogel 1966: 644), and, implicitly, was not due to its method. Indeed, given both the reasons of the economists at Williamstown and the limited scope and stringent assumptions of the methods in question, one may even wonder how they had succeeded in obfuscating so many bright scholars. From the very first days, the method was
accused of inadequacy, even by some who had flirted with it: ‘[econometric methods in history represent] a form of intellectual tunnel vision. It manifests itself most frequently in a penchant for focussing exclusive attention upon the confrontation between a terribly restricted set of observations and an equally narrowly specified hypothesis concerning some facet of economy behaviour’ (David 1971: 464).

But the most important dissidence was that of Douglass North. This was not new. As early as 1965, North had claimed that the ‘new economic history falls short of the mark in remedying this situation [the very poor situation in the discipline]’ and has been ‘generally disappointing’ (North 1965: 86, 90)\(^5\). But in 1978, although claiming fidelity to the original purpose of cliometrics, North went much further and criticized his fellow cliometricians for ingenuous belief in the virtue of neoclassical economics: (p.19)

Emphasis upon the systematic use of theory—particularly price theory—[has been] the most decisive contribution of the new economic history, which is the attempt to develop a more scientific history. The explicit use of theoretical models and the systematic use of statistical inference in testing procedures are the most distinctive contributions of this approach. Where McCloskey and I part company is that I think most of the new economic history simply applies neoclassical economics to the past. That is a contribution, but one that quickly runs into diminishing returns and leaves the economist with the conviction that we are marginal if not dispensable to the profession. (North 1978a: 78)

McCloskey challenged this critique: ‘[North is] a cliometrician who complained (mistakenly) that cliometrics uses economic tools uncritically’ (McCloskey 1978: 28). Nevertheless, a different explanation runs as follows. North oriented his research for a long time to ‘the central puzzle of human history [which] is to account for the widely divergent paths of historical change’ (North 1990: 6), and, in particular, to the study of the formation and evolution of institutions. This vicinity to the old historians' theme forced him to suspect the usefulness of the neoclassical assumptions and finally to conclude that they were the ‘fundamental stumbling block preventing an understanding of the existence, formation, and evolution of institutions’ (North 1990: 24). Indeed, the
consideration of institutions requires the abandonment of the over-simplistic rationality principle and the study of environmental complexity. North did not hesitate in either instance: he replaced perfect information and rational maximizing behaviour with procedural cognition and bounded rationality, and replaced the postulate of simplicity with the discussion of non-deterministic outcomes of social interaction. Consequently, his previous views of institutions as efficient economic units were dropped, as was his representation of institutions through a simple model of transaction costs. As a consequence, North came to be closer to the old historical school than to his fellow cliometricians, and did not hide the impact of his own transformation:

What difference does the explicit incorporation of institutional analysis make to the writing (and for that matter the reading) of economic history and of history in general? . . . A brief answer to the question is that incorporating institutions into history allows us to tell a much better story than we otherwise could. The precliometric history actually was built around institutions, and in the hands of its most accomplished practitioners, it managed to provide us with a picture of continuity and institutional changes, that is, with an evolutionary story. But because it was built on bits and pieces of theory and statistics that had no overall structure, it did not lend itself to generalizations or analysis extending beyond the essentially ad hoc character of individual stories.

(p.20) The cliometric contribution was the application of a systematic body of theory—neoclassical theory—to history and the application of sophisticated, quantitative techniques to the specification and testing of historical models.

However, we have paid a big price for the uncritical acceptance of neoclassical theory. Although the systematic application of price theory to economic history was a major contribution, neoclassical theory is concerned with the allocation of resources at a moment of time, a devastatingly limiting feature to historians whose central question is to account for change over time. Moreover, the allocation was assumed to occur in a frictionless world, that is, one in which institutions either did not exist or did not matter. These two conditions gave away what economic history is really all about:
to attempt to explain the diverse patterns of growth, stagnation, and decay of societies over time, and to explore the way in which the frictions that are the consequence of human interaction produce widely divergent results. (North 1990: 131-2)

This goes back to the origin of the argument: the specific contribution of cliometrics was the method it implied and used in order to reincorporate history into economics. Let us turn to it now.

1.3. Clio at Work
McCloskey, one of the devotees of the new economic history, and clearly one of the main defenders of its positivistic trend, lauded the novelty of the new approach on two grounds: first, it represented the theorization of economic history; second, it set the pace of quantification. One could not take place without the other, since quantification depended on the ‘lunatic’ assumption of some functional forms and on the strict capacity to measure them: ‘The limits on curiosity about the economic past set by the available facts are few, and cliometricians—bemused by production functions and demand curves and the lunatic belief that they can actually measure them—have led the way in pushing the limits further’ (McCloskey 1978: 21).

In comparison with traditional economic history, ‘bemused’ by complex causal systems determined mostly by qualitative features, namely institutional evolution, this represented a major shift. But at least the early generation of cliometricians was quite aware of the foundations and implication of cliometrics: to put economic history à la page with economic theory meant essentially to adopt the general criterion of science as it was conceived of in positivism. As a consequence, history should be indistinguishable from empirical or natural sciences, in the sense that explanation equated the formulation of a general covering law, subsuming every instance under its rule. Carl Hempel (1942) was, for Meyer, Conrad, and Fogel, and eventually for others, the major reference for this reconstruction of history: ‘In history as anywhere else in empirical science, the explanation of a phenomenon consists in subsuming it under general empirical laws; and the criterion for its soundness . . . [is] exclusively whether it rests on (p.21) empirically well confirmed
assumptions concerning initial conditions and general laws’ (Hempel; quoted in Fogel 1964: 1).

And therefore, ‘The fundamental methodological feature of the new economic history is its attempt to cast all explanations of past economic development in the form of valid hypothetico-deductive models’ (Fogel 1966: 656).

Although Hempel argued that historical explanation consisted of ‘explanation sketches’ rather than positivistic conclusions, and that historical models were ‘law-like’ rather than universal laws (Fogel 1964: 246), he was read as stating the exact conditions for empirical research on a covering law, a set of initial conditions and each particular instance of the events. Those were the ‘Hempelian lines’ that the cliometricians had sworn to follow (p. 248).

The hypothetico-deductive model was based on a crucial assumption permitting its positivistic formulation (the definition of the rationality of the agents), and was calculable, since all types of event could be represented under the general specifications of the model or of the random shocks allowing for statistical inference. Therefore, it also allowed for predictive and subjunctive conditional statements about non-observed phenomena. These three characteristics will now be discussed in turn.

The acceptance of the rationality principle and of methodological individualism in economic history was a major sign of its espousal of orthodox economic theory. Indeed, most of the previous investigation dealt precisely with exceptions to and refutations of these principles. But conformity with the canon claimed sacrificial victims, and institutional and social history was one of them.

As a consequence, rationality was assumed further and further back in history as a constant in human behaviour, and *Homo economicus* was erected as the intrinsic self of *Homo sapiens*: ‘The tales of the adventures of *Homo economicus* in unlikely places are beginning to accumulate, in nineteenth-century India, for example, or medieval Europe, or declining Rome’ (McCloskey 1978: 24).
Yet, this is far from consensual. For decades now, the work of anthropologists and historians has shown that different civilizations organized modes of production, and of social interaction, that were alien to individual profit maximization and were based on co-operative action. From Margaret Mead’s research on the Arapesh of New Guinea (Mead 1962: 37) up to much recent work, this thesis was demonstrated time and time again. Contemporary research, namely innovation, firm and institutional theory, emphasizes the very same idea. The notion of a Parmenidean world where all flux is illusory and where *Homo economicus* never evolved over the millennia (Schabas 1995: 198), behaving under the same criteria in Ancient Rome just as he does in the Hong Kong stock market at the beginning of the twentieth-first century, is at least a delightful joke.

But, just like any simple idea, it has a crucial role that can scarcely be distinguished amidst its roughness. It allows for the use of powerful accounting and statistical tools, based on linear or linearized systems, and for the (p.22) common use of regression analysis as the most frequent device (Fogel 1966: 652). Furthermore, it allows for even more stringent assumptions, such as the choice of Cobb–Douglas production functions and all that they imply—constant returns to scale, marginal productivity factor pricing, unbiased aggregation of supposedly independent factors of production, continuously differentiable production functions (e.g. for the study of British economic growth from 1856 until 1973: Matthews *et al.* 1982: 590).

Some of the technical implications of this mode of investigation and computation, and its adequacy for use in historical series, will be discussed in the next section. But we must add that there is also another powerful implication of this assumption of self-interested and maximizing rationality as the pattern of human and social behaviour, which is its ideological *a priori*. Once again, McCloskey voices this not so hidden assumption: ‘Here again economic theory dominates the research, giving it coherence, not conclusions. True, the conclusions have often been variations on the theme, “The Market, God bless it, works” . . . ’ (McCloskey 1978: 21).

This ideology is quite obvious in one of the major pieces delivered by the cliometricians: *Time on the Cross*, the impressive research by Fogel and Engerman (1974) about the
profitability of slave exploitation in the Antebellum South, following Conrad and Meyer. In the book, both planters and slaves are depicted as rationally acting groups—although, at least for one of them, choice was meaningless since they were slaves. The slave owners are presented as ‘shrewd capitalistic businessmen’, capable of a ‘superior management of planters’, and the slaves are praised for the ‘superior quality of black labor’, since they ‘competed for [skilled] jobs’, ‘imbued like their masters with a Protestant ethic’, and strove ‘to develop and improve themselves in the only way that was open to them’ by being ‘diligent and efficient workers’ (Fogel and Engerman 1974: 73, 150, 201, 203, 210, 231–2, 263). Although the authors claim to try to redeem black history, giving the slaves a rightful central place in the economic development of the American South, the result is meagre if not doubtful.

Since this refers to a much discussed and well known topic in economic theory, and there is nothing novel in its importation by cliometrics, this chapter will not go into the details. Instead, the following section will deal with some specific contributions made by cliometrics.

The Industrial Revolution: Random Events as the Standard Mode of Variation in History

If the first topic was quite trivial, since cliometrics just adapted orthodox common sense about the rationality postulate without adding anything to it, the second concerns a specific contribution and actually a very important one from the standpoint of this book. It was to be expected: the drive to a ‘more scientific economic history’—or econometric history, as it was also dubbed—implied more than the willingness to apply the fancy tools of statistical inference. It required the imposition of some strict conditions, namely the genuinely probabilistic character of history itself.

It is well known how the early statisticians tried to solve the problem of extending their findings and methods to time series. Although some of the first important contributions were made by social scientists, such as Jevons and Edgeworth, these ultimately hesitated to extend their methods to the realm of society and economics. Later on, the same perplexity was stated by one of the founders of econometrics, Ragnar Frisch, who did not follow the probabilistic approach that he had been so instrumental in creating. Nevertheless, the econometricians generalized the concept of randomness, and
two of the directions of such a generalization were relevant for the cliometric revolution. The first was the consideration of historical events as random elements impinging upon the basic structure of the regular historical process. The second, as powerful as the previous one, was the statement that historical time series could be thought of as a sample from a large universe of possible realizations of the same process. It would therefore be possible to apply tests of hypotheses following the Neyman–Pearson strategy and to compute the significance of the parameters obtained from fitting the equations of the model to the data. That was the strategy followed by Meyer and Conrad in their papers presented to the 1957 conference: a ‘stochastic causal model’, precisely defined in terms of endogenous, exogenous, and random variables, was used to determine the minimal conditions for ‘causal ordering’ (Meyer and Conrad 1957: 532). But it must be added that this crucial point generated the stronger reactions at that conference. Kuznets indicated that a large part of the participants shared the same type of scepticism as Frisch and did not follow Conrad and Meyer or their disciples:

One of the questions raised in the discussion is whether the probability tests permitted by this interpretation of the $e$ term are valid for the kind of situation analyzed in economic history. . . . [The] general tenor [of the discussion] suggests a conclusion

different from that of the authors. The criterion of the discussion was, I believe, that there are almost no cases in which such a ‘reasonable approximation’ exists . . . [since the] tests would have to be made in terms of a universe of economic trends. (Kuznets 1957: 549–50)

More recently, Crafts discussed this problem in very great detail, and his solution is illuminating. In a paper published in 1977, he accepted that historical events could not be explained under universal covering laws, as necessary and sufficient conditions: ‘whenever and only if A, then B’ (Crafts 1977: 432). Moreover, the multiple regression would be non-operational, given the severe problems of interpretation, the uniqueness of observations, the multicollinearity of variables, and the insufficient number of degrees of freedom. The suggested alternative, as in the early econometric tradition,
was to consider history as an intrinsic process of chance: (p. 24)

All that needs to be maintained is that there are probability distributions of values of Y for given values of any X and that the probability distributions of Y are different for different values of X. . . . [And] the best we can do is to formulate explanatory generalizations with an error term. (Crafts 1977: 433–4)

In order to avoid retrospective inference and the consequential danger of the post hoc ergo propter hoc fallacy, Crafts suggested rejecting the concept of chance as ignorance of the true structure of events and alternatively considering chance as the expression of the irreducible randomness in history. Therefore, ‘the best we can do is to formulate explanatory generalizations with an error term’ (Crafts 1977: 433–4). But that was not a trivial assumption, since it was the decisive step in allowing for regression analysis and other tools of statistical inference. These rapidly became commonly used tools in econometric history and generated a flow of new studies and publications on a wide range of subjects.

The concept was for instance applied to the inquiry into the different motivations behind the distinct performances of Britain and France during the Industrial Revolution and the reason for France lagging behind. In particular, the discussion revolved around the question, why were the spinning jenny of Hargreaves and the water frame of Arkwright invented in Britain? Crafts's answer is that the ‘economic development in general and technological progress in particular in eighteenth-century Europe should be regarded as a stochastic process’ (Crafts 1977: 431). In other words, it was a ‘stroke of genius or luck’ (Crafts 1995b: 756), or ‘strokes of genius, luck or serendipity’ that provided decisive macro inventions *ab nihilo* (Crafts 1995a: 595)—and that decided the primacy of Britain. Consequently, there is no explanation for the time or location of the decisive inventions in cotton textiles that proved crucial for the acceleration of growth and the Industrial Revolution (p. 596). Then the puzzle becomes: had these inventions occurred in France, could that country have dominated the period of the Industrial Revolution?
The legitimacy and the logical implications of the question itself are doubtful and will be discussed shortly, but they may be ignored for the time being. Two other central aspects of the polemics are more interesting at this juncture: the historical determination of the radical change known as the Industrial Revolution, and the role and measurement of stochastic factors in history.

For a long time now, Crafts has been arguing that the case for the Industrial Revolution had been clearly exaggerated, and that previous computations by Deane and Cole should be revised. Many of his followers went further and (p.25) denied the very concept of the Industrial Revolution: it is 'a concept too many' (Coleman 1983: 435 f.), and 'English society before 1832 did not experience an industrial revolution let alone an Industrial Revolution', and '[its] causes have been so difficult to agree on because there was no “Industrial Revolution”, historians have been chasing a shadow' (Clark 1986: 39, 66).

Someone even added in a pamphleteering vein: 'Was there an industrial revolution? The absurdity of the question is not that it is taken seriously, but that the term is taken seriously . . . by scholars who should know better' (Cameron 1990: 563).

There is a quantitative answer to these statements (see Figure 1.1), although the decisive point lies elsewhere. The main facts can be assessed through historical investigation. Table 1.1 was provided by Crafts and compares estimations for the yearly growth in real output for Britain (in percentage terms). Before considering further evidence, it should be stressed that this table—even if one accepts Crafts's more pessimistic estimates uncritically—provides strong evidence for an impressive movement of acceleration in general growth, and in particular in the leading industry, cotton textiles. This is indicated by simple arithmetic. In the same table, we added the indication of the number of years necessary, for each growth rate, to double the level of production for each initial period, and the conclusion is straightforward about the acceleration of the trend of growth. This period is reduced to half or one third in the crucial years of the Industrial Revolution for the economy as a whole, and the textile industry could even double in scarcely more than ten years, when it represented more than 20 per cent of the total output.
Crafts's attack on Rostow and Deane and Cole's view of the Industrial Revolution in favour of 'a more gradualist interpretation' of 'steady growth, rather than a “take-off” or spectacular growth' (Crafts 1989: 65, 67), seems to be undermined by his own evidence (e.g. Table 1.2). Furthermore, he recognizes that 'within industries were to be found the few sectors where productivity growth was really fast; most notably in textiles, with its radical changes in technology', causing in any case 'enormous changes in the economic structure of Britain between 1760 and 1840', and a 'revolutionary change in the structure of employment', since 'by the second quarter of the nineteenth century the economy had achieved a rate of growth of the total

Fig 1.1. Time Necessary To Double the Level Of Production (Number Of Years), 1700–1860

### Table 1.1. Growth of Real Output 1700–1913 (% Per Year)

<table>
<thead>
<tr>
<th>Period</th>
<th>Industrial output</th>
<th>GDP</th>
<th>Deane, Cole</th>
</tr>
</thead>
<tbody>
<tr>
<td>1700–60</td>
<td>0.7</td>
<td>1.0</td>
<td>0.67</td>
</tr>
<tr>
<td>(99yr)</td>
<td>(70yr)</td>
<td>(104yr)</td>
<td>(99yr)</td>
</tr>
<tr>
<td>1760–80</td>
<td>1.5</td>
<td>0.5</td>
<td>2.45(^a)</td>
</tr>
<tr>
<td>(47yr)</td>
<td>(139yr)</td>
<td>(29yr)</td>
<td>(99yr)</td>
</tr>
<tr>
<td>1780–1801</td>
<td>2.1</td>
<td>5.7</td>
<td>3.4</td>
</tr>
<tr>
<td>(33yr)</td>
<td>(13yr)</td>
<td>(21yr)</td>
<td>(84yr)</td>
</tr>
<tr>
<td>1801–31</td>
<td>3.0</td>
<td>5.6</td>
<td>4.4</td>
</tr>
<tr>
<td>(23yr)</td>
<td>(13yr)</td>
<td>(16yr)</td>
<td>(26yr)</td>
</tr>
<tr>
<td>1831–60</td>
<td>3.3</td>
<td>3.0</td>
<td>1.98(^b)</td>
</tr>
<tr>
<td>(21yr)</td>
<td>(23yr)</td>
<td>(35yr)</td>
<td>(28yr)</td>
</tr>
<tr>
<td>(1831–73)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1873–99</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(33yr)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1899–1913</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(50yr)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
(a) 1760–1830.

(b) 1830–70.

*Source:* Crafts (1989: 67); Mokyr (1993: 9). Our computation of the delay, in years for doubling the current values of production is given in parentheses.
Table 1.2. Labour and Output in Britain, 1700–1840 (%)

<table>
<thead>
<tr>
<th></th>
<th>1700</th>
<th>1760</th>
<th>1840</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male labour</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Industry</td>
<td>18.5</td>
<td>23.8</td>
<td>47.3</td>
</tr>
<tr>
<td>Agriculture</td>
<td>61.2</td>
<td>52.8</td>
<td>28.6</td>
</tr>
<tr>
<td>Output</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Industry</td>
<td>20.0</td>
<td>20.0</td>
<td>31.5</td>
</tr>
<tr>
<td>Primary sector</td>
<td>37.4</td>
<td>37.5</td>
<td>24.9</td>
</tr>
</tbody>
</table>

Source: see Table 1.1.

(p.27) factor productivity which would previously have been inconceivable’ (pp. 69, 71, 68). This differential growth is the crucial feature of the Industrial Revolution: again according to Crafts, the annual rate of growth of real output of the cotton industry was 1.37 per cent for 1700–60 and 7.57 per cent for 1760–1800, whereas it was just 0.25 and 0.37 per cent for leather and 1.25 and 1.44 per cent for linen for the same periods (Crafts 1985: 23). This dual economy was fuelled by a modern sector of rapid growth, in spite of an ensemble of old sectors moving slowly: cotton, iron, engineering, mining, transport, and some consumer goods (paper, pottery) grew at 1.8 per cent a year for the period 1780–1860, whereas the traditional sectors were responsible for an aggregate growth rate of 0.7 per cent (Mokyr 1985: 4–5).

One may ask then what is the point of the slight differences in estimates and what can possibly be inferred from these differences. Trapped by his own polemics, Crafts is forced to spend pages and pages arguing against the misinterpretations and interpretations, and claiming repeatedly that ‘we reaffirm the importance of the industrial revolution as a historical discontinuity’ (Crafts and Harley 1992: 721). But the nature of this discontinuity is still to be uncovered, since there is more than a slight difference between explaining the process by historically interconnected causes and explaining it by small and meaningless random perturbations.

Since he adheres to the second explanation, Crafts maintains that the Industrial Revolution was simply a transformation in the existing industries. There is indeed an essential continuity in the economies, since modes of production are transmitted, market traditions and regulations endure, and routine dominates the technical processes. But what also happened,
and this is specific to this period, was the superimposition of new techniques, of new procedures, of a new form of best practice, leading to cumulative increases in productivity and to radical changes in the relative position of industries and firms. From this point of view, statistics reveal as much as they hide, since there were different rates of growth between industries but also inside each industry (Mokyr 1993: 9)—and the understanding of the Industrial Revolution is inseparable from the understanding of these different rhythms. As Crafts himself once again pointed out in 1977: “ ‘Industrial Revolution’ will be understood as a period of accelerated structural change in the economy, involving a rapid rise in industrial output, in the share of manufacturing in national product, and in factory-based activity (implying a different kind of economy) based on major technological innovation’ (Crafts 1977: 431). Figure 1.2 reproduces a graph presented by Crafts himself on the trend growth of industrial production: what better evidence could we have of the discontinuity introduced by the Industrial Revolution?

The question is therefore whether the process was the outcome of one or several random changes in the structure of the economy and, in particular, whether this was the reason for the differences between England and France. Surprisingly enough, one of the opinions invoked by Crafts as corroborating

(p.28)

Fig 1.2. A Radical Shift: Trend Growth In Industrial Production During the Industrial Revolution, 1700–1900
his own view—
that of Mokyr—
clearly leads to
another
interpretation:
‘the technological definition of the Industrial Revolution is a
clustering of macro inventions leading to an acceleration in micro
inventions’ (Mokyr 1993: 22). Furthermore, Mokyr argued that ‘the
key to the British technological success was that it had a
comparative advantage in micro inventions’ (p. 33). Of course, the
problem is discovering how and why this comparative advantage
was built in. Crafts himself provided relevant information for the
answer to this question, pointing out the institutional advantages of
Britain over France, namely in the patent system, in prior
improvements in transportation, (p.29) in the trade system, and
consequently in the ability to diffuse innovation; and, in the
opposite direction, the disadvantage of France as far as its ‘rent
seeking attitude’ and the ‘ability to resist change’ was concerned
(Crafts 1995b: 759; 1995a: 594, 596). North suggested that major
organizational innovations accounted for the breakthrough, as
much as technological innovations (North 1965: 87–8), and Landes
discussed the impact of the new ‘factory system’ from that same
point of view (Landes 1993: 140).
Now, three main approaches have been suggested to
interpreting these facts. The first one, much along the same
lines as Crafts, suggests that the Industrial Revolution concept
is a mere construction of a theoretical arte-fact: Clapham
suggested, as early as 1926 and based on data from the 1851
Census, that the change had been far less radical than the tale
tells (Landes 1993: 135). But, arguably, these data are not
completely reliable: for example, only in 1861 did the Census
include civil engineers along with clergy, medics, and lawyers
as ‘professionals’ (Crafts 1995b: 759). Furthermore, the
statistical aggregation may smooth over discontinuities and
change, and ‘drown sectors of innovation and change in a sea
of tradition’ (Landes 1993: 148). Not only is reliability
questionable for statistics, in particular those covering early
ages in industrialization, but also ‘it is often true that the
volume of data available is frequently below the minimum
required for standard statistical procedures’ (Fogel 1966:
652). Therefore, ‘the most that can be done is to make careful
“guesstimates” in the light of both the available resources and
general economic and statistical reasoning’ (Crafts 1989: 66).
And, as pointed out before, the evidence of a drastic change in
production conditions—as represented by the period necessary
for the level of production to double—challenges these ‘business as usual’ interpretations of the Industrial Revolution.

The second alternative, an elaborate form of the first, is that the Industrial Revolution is accountable by an explanation based on randomness. In that sense, Meyer and Conrad had suggested the formalization of any economic process as a hypothetico-deductive model in the Hempelian mould: a general covering law was represented by a deterministic system expressing all the systematic influences as explanatory variables, whereas the errors of observation, unsystematic influences, and omitted variables were to be represented by the random term (Meyer and Conrad 1957: 535). In this framework, Hargreaves's and Arkwright's inventions would be the expression of a purely chance draw, as Harley put it in 1990: 'The technological breakthrough in industry occurred in Britain in part because of the dynamic character of the economy but Britain probably also benefited from a lucky draw in the random process of invention' (quoted in Landes 1993: 147).

Crafts's somewhat more sophisticated explanation does not escape from this framework. Arguing in 1977 that Britain's primacy does not prove an ex ante larger probability for success, Crafts denied nevertheless that the result was purely fortuitous (Crafts 1977: 441). Yet, some years later, in 1995, he approvingly quoted Crouzet, saying that the mentioned inventions ‘could have been fortuitous' (Crafts 1995a: 595). Although the same paper recognized that Landes was right, that the previous growth comparisons which had been the basis for the 1977 argument were false, and that ‘it does seem now clear that both France's growth potential and the level of income per head were lower than the equivalent in Britain’ (Crafts 1995a: 592), the author did not conclude from this that the stochastic causal approach should be abandoned. Exogenous and random technological shocks and their eventual ramifications continued to account for crucial advantages, even if a larger potential for growth was obvious in British data (Crafts 1995b: 758–9; 1995a: 597).

Landes answered that this potential and the level of prior development were indeed the crucial ingredients for a reasonable explanation:
The possibility of revolutionary accidents has its theoretical interest . . . It is, however, a counsel of despair in the search for understanding and explanation. In other words, it is not the subsequent superiority of the British textile industry that tells us about Britain's chances for technological change (Crafts' bugbear of post hoc ergo propter hoc), but its previous development. (Landes 1995: 600)

This last alternative interpretation simply states that a complex explanation is required for understanding complex reality. In this framework, the formal models, based on the assertion of a covering law and established on the basis of a precise dichotomy between explained variables and explanatory ones added to the largely undefined random variables are nothing but a suspect and limited tool. Instead, our approach recognizes that historical explanation is generally overdetermined and looks for multi-level causation, unlike econometric explanation that is perfectly determined and based on simple correlation or multiple regression. Let then the provisional last word also be with Landes:

Here let me state a golden rule of historical analysis: big processes call for big causes. I take this as what economists call a prior. I am convinced that the very complexity of large systemic changes requires complex explanation: multiple causes of shifting relative importance, combinative dependency . . . temporal dependency. . . . I would also argue for a kind of reality principle. It is not hard to devise mathematical models for intrinsic inevitability—of small differences that are reinforced over time to produce an ever-widening gulf, of lines of development locked into ‘path dependencies’. But any resemblance between such lucubrations and the real world is purely coincidental and highly occasional—fortunately. The real world is made up of actors as well as of people acted upon. People and groups respond to change and challenge, evade constraints, and find other solutions. . . . Let me make a modest proposal. Economic history needs protection against bad numbers. The more artful our econometric techniques, the greater the recourse to quantification, the more protection we need. (Landes 1994: 653–4)
It was previously noted that cliometricians sometimes presented their work as addressing the old problems with new methods, while at other times they argued, quite reasonably, that they were performing a major shift in economic history, both in methods and in the definition of the relevant questions. This ‘significant break with the past’ has been related to the use of explicit and formal models, to the operational definition of variables as part of a stochastic causal model, and, finally, to the test of ‘the model (a logical statement of assumptions and conclusions) against the evidence (the world that did exist) and the counterfactual deduction (the world that did not exist)’ (Davis 1966: 657). This use of the model in order to test for a counterfactual hypothesis became a distinctive feature of cliometrics, since it was related to strong claims that the production of positive assertions depended on that test: ‘It is only through a comparison of what was with what might have been that we are able to make statements about the nature of events’ (p. 658).

The classical example was given by one of the papers Conrad and Meyer presented at the Williamstown conference. The authors suggested that a proper counterfactual test be held on the proposition that ‘slavery was not profitable in Antebellum South’, as a form of eventually refuting another one—’if the Civil War had not occurred, the South would have abolished Negro Slavery in an orderly fashion within one generation’—which could not be directly tested.

Addressing these major issues with counterfactual logic is like extinguishing a fire with petrol. Indeed, ‘[any] logic of causes via counterfactuals can only give us antecedents and consequences in the causal nexus by either an arbitrary decision or by a direct expression of our conceptual grounding’ (Climo and Howells 1974: 468).

(p.32) The establishment of causal nexuses via the counterfactual depends either on arbitrary decisions that cannot be logically disciplined or on metaphysical foundations. Consequently, one may argue that the counterfactual cannot provide any substantial inference, since the assumptions are obviously false. This is the traditional problem in logic: *ex falso sequitur quodlibet*; that is, from false assumptions you may conclude either false or true assertions—there is no way to discriminate between valid and invalid inferences. In other words, it is a dead-end. The definition of the counterfactual
must be made in terms of a law-like assertion, if it is either a necessary condition or a sufficient and necessary condition, supposing in any case adequacy to the world; but the counterfactual itself is defined as a false statement issued from this model and therefore unrelated to the real world. Trading realism for instrumentalism is a rare craftsmanship indeed.

The previous paragraphs incorporate both a general warning against the widespread use of counterfactuals in economic history, and a discussion of their drastic epistemic limits. But in spite of so many warnings from many different authors, counterfactuals are indeed widely used in economic, statistical and historical reasoning, since it is generally admitted that many claims on causality (more on this later on) may take the form of a counterfactual. It is argued that, since the assertion ‘A caused B’ can generally be restated under the equivalent form, ‘if Adid not happen, B would not have occurred’, the use of counterfactuals in causal analysis is widespread. This section contradicts that view and discusses the possibilities of an alternative logical development of counterfactuals, following Elster's classical work on the topic (Elster 1978).

The issue of causality is of course the central motivation for the inquiry into counterfactual logic. Consequently, one cannot establish convenient criteria for the validity of a conditional statement, unless a workable notion of causality is defined. It is assumed that the traditional notion of causality, that of Hume, based on spatial contiguity, temporal sequence and constant conjunction, i.e. defining cause as a sufficient condition for an event B, is too restrictive (Elster 1978: 178ff.). Alternatively, we take a broader view of causal implication as a form of weaker determination, abandoning the requirement of permanent conjunction and contiguity, since the complexity and interconnectedness of multiple causes is recognized. But unlike John Stuart Mill, who deduced the rejection of any conditional assertion given the interdependence of causes, Elster suggests that, even in these circumstances, causal links can be discussed in the framework of true conditionals from false antecedents to the true consequent, whereas of course false conditionals from true antecedents to false consequent are useless (Elster 1978: 12).
Rejecting the circular justification of causality through counterfactuals and vice versa, Elster proposes a criterion of legitimacy and another of assertability (and not of truth) in order to establish the validity of the historical counterfactual. The ‘dynamic criterion for legitimacy’ is simply that ‘we (p. 33) must require that a counterfactual antecedent must be capable of insertion in the real past’ (Elster 1978: 184). Simple, but not trivial, since it substantially reduces the range of possible assertions and grounds them on concrete history. In that case, the counterfactual would take the form: ‘If at time $t$ the configuration had been $A_1 \ldots A_{n-k} B_1 \ldots B_k$ [instead of the real process, $A_1, \ldots, A_n$], then at time $t$ the subset $x_{i1}, x_{i2}, \ldots, x_{ip}$ of the variables [the variables exhaustively describing the process] would have assumed the values $C_1 \ldots C_p$ [instead of the real event taking place]’ (p. 184). This condition of legitimacy presupposes that a theory is provided, enabling the researcher (1) to filter the choice of the antecedent, so that it can be insertable in the real past; sequentially, the second condition, the condition of assertability, requires the theory to be able (2) to establish the inference from the hypothetical antecedent to the hypothetical consequent. In response to Elster, McCloskey argued that sins of vagueness and absurdity could be avoided under the correct specification of a model suitable for testing (McCloskey and Nansen 1987: 702), but this defence is indeed a revelation, since this is just part of the problem. That is why a third condition must be imposed: there must be a minimum distance between the current state and the past branching point permitting the assertable counterfactual. If the actual state is $s_t [A_1, \ldots, A_n]$ and the hypothetical consequent is $s'_t [A_1 \ldots A_{n-k} B_1 \ldots B_k]$, then the distance between $s_t$ and a past state $s_{t-1}$ that would permit both trajectories is the distance to be considered, whereas $s_{t+1}$ would not permit those trajectories. In this framework, ‘the counterfactual as a whole is assertable if the consequent holds in the closest world(s) where the antecedent obtains’ (McCloskey and Nansen 1987: 191).15

These conditions do not necessarily prevent all possible wrong and useless counterfactuals, since their application is not straightforward. But they provide a necessary discipline in the field of historical research and define interpretable criteria for defining legitimacy and assertability in each context.
A major early example was the debate on the effect played by the railroads in the growth of the American economy. Fogel published a seminal work on that topic, assuming stringent counterfactual hypotheses: that no more canals or roads were built after 1890, but that 5,000 miles of canals as well as some roads were improved, that new storehouses would have been built, and that the spatial structure of agricultural production would have been changed to diminish transport costs. In this framework, he computed the social savings from railroads to be around 1.8 per cent of GNP\textsuperscript{16} (Fogel 1966: 650).

(p.34) Fogel was careful enough to state that it is ‘beyond dispute’ that railroads were efficient, although some alternatives were also identified (Fogel 1964: 10). Furthermore, ‘the only inference that can safely be drawn from such data is that railways were providing transportation services at a cheaper cost to the buyer than other conveyances’ (p. 13). Any economist used to the orthodox argument and finding evidence of a cheaper service and larger profit would stop here and rest their case, since that would supposedly be enough for the acquisition of a cumulative competitive advantage. But Fogel did not, and went for his counterfactual test; and one of his associates in this endeavour stated bluntly: ‘The fact that railroads rapidly replaced canals is not evidence of their overwhelming superiority . . . . Their effect of supplying industries was hardly sufficient to justify making the railroads a causal variable, in a theory of growth’ (Davis 1966: 661). This could have some truth in it: many examples of lock-in inferior trajectories are nowadays recognized, although even in these cases there is a causal nexus. But this is not the argument presented by Fogel or Davis: they simply assume that a functional equivalent technique of transportation would have appeared as manna from heaven and produced potentially the same cost reduction as railroads. As a matter of fact, Fogel just looks around for examples of canal transportation firms that adapted adequately to railway competition (Tunzelmann 1990: 296). At any rate, the authors do not state anything else on the matter, and simply argue that the direct effect of railroads was reduced.

The argument is legitimate, since a theory of technological innovation can be provided in order to support this counterfactual. But it is not assertable, following Elster’s criteria, since it is based on confusion between atomistic
withdrawal losses and the systemic effect of introduction gains (Elster 1978: 204 ff.): the railroads introduced further flexibility, generalized decisive steps in learning by doing, diffused new production techniques, and trained a layer of professional managers who would be crucial in the managerial revolution of the end of the century (Chandler 1977) (see Chapter 6). Moreover, they generalized a ‘cluster of interlocking, mutually supporting techniques’ (David 1969: 510–11); i.e. they stimulated increasing returns. These effects cannot be measured using Fogel and Engerman’s techniques, and consequently cannot be transported into the past looking for a suitable bifurcation point.

Although a large discussion has developed around this work, the main counter-arguments are still the original objections. Yes, railroads may have accounted just for 2–5 per cent of the growth of national income, accepting that the available method is accurate enough to measure it. But how much is 2 or 5 per cent? This was Usher’s—and Landes’s—objection to Fogel’s conclusions, as stated in a banquet at Harvard (Landes 1978: 7). It is obvious that the original argument does not stand if one puts logic back on its feet, and looks for a realistic framework for the explanation: growth without the railroads, or a fictitious 97% of actual growth, is strictly meaningless.

(p.35) The case could not be clearer than with Fogel’s argument:

An interesting case in point is the notion that railroads made a unique contribution to the creation of the psychological atmosphere favoring economic growth by building lines across sparsely settled territories. Although the evidence demonstrating that the eruption of a boom psychology followed in the wake of such enterprises is considerable, no evidence has been supplied which demonstrates that the absence of the railroad had deprived the nation of considerable mental disposition. And it is doubtful that such evidence can be supplied. For if the boom psychology was a response to the opportunity to profit from unexpected changes in the value of land and other assets, it was not a unique consequence of railroads. The same favorable mental disposition could have been created by the construction of a new canal or the introduction of any new mode of
transportation that unexpectedly and drastically reduced the cost of transportation in a given area. (Fogel 1964: 10–11)

What is really implied, one may argue, is the impossibility of a counter-factual conditional at least in this case: identification of relevant alternatives is not feasible (‘the same disposition could have been created . . .’). Furthermore, there are grounds for reasonable scepticism about the whole project (‘it is doubtful that such evidence can be supplied . . .’).

The example of the exercise by Hawke, who followed Fogel’s methodology and studied the global economic impact of railways in Britain, is quite telling. Assuming perfect demand inelasticity, i.e. a powerful and dubious ceteris paribus, Hawke concentrates on the passenger traffic; he discusses the cost of alternatives before the creation of railways, using an ex ante concept of social savings, and deduces a net effect that is larger than that computed by Fogel: ‘Dispensing with the railways in 1865 would have required compensation for between 7 and 11% of the national income’ (Hawke 1970: 401). Consequently, a single innovation could have influenced the whole economy in a non-negligible way. But this is just the opposite conclusion to that of Fogel, who tried to deny both the effect of single major innovations and the fact of discontinuities in the evolution of the economy.

The arguments of both authors are obviously dependent on the concrete use of the proposed methodology. The counterfactual is in each case vulnerable to the critique of assertability, based on the impossible insertion in the real past. Our argument is that the historical inquiry could have followed another direction, not abdicating from realism. Indeed, several scholars developed meaningful efforts in that alternative direction. The effect of technical breakthroughs has been addressed both by theories of innovation, which emphasized the importance of technological paths and canalization, and by the institutionalist approach of Alfred Chandler. Chandler showed that railroads provided a major organizational mutation and represented a best-practice frontier for modern firms. In this framework, what Fogel called, in a puzzling way, the ‘psychological’ impact of railroads can actually be assessed and traced back to the concrete history of firms, of managers, of trade unions, of economic regions and institutions, and so on, not as a bizarre (p.36) psychological
measurement, but as a concrete implication of the development of managerial practices. Indeed, the extensive development of the railroads implied a major change, and it was economically sound: ‘By 1840, when the new mode of transportation had only begun to be technologically perfected, its speed and regularity permitted a steam railway the potential to carry annually per mile more than fifty times the freight carried by a canal’ (Chandler 1977: 86). Furthermore, ‘by the 1840s the railroad construction was the most important single stimulus to industrial growth in Western Europe’ (Landes 1969: 153). The preparation of a generation of functional managers, supervising extensive geographical areas and applying rigorous tools of modern accounting, planning, and administration, controlled by intermediate levels of management, was a decisive feature of this process: they were the ‘pioneers of modern management’ (Chandler 1977: 87). They were the visible hands of this managerial revolution.

After two generations, the change was gigantic. ‘Indeed, by 1890, cumulative investment in railroads was greater than that in all non-agricultural industries combined and comprised more than 40% of the non-residential capital formation to date’ (Chandler and Hikino 1997: 38). A new transport and communication system was then complete, and that was the setting for the second industrial revolution. The introduction of railways opened a new era of mechanization, of increase in scale of sales and production, generating new modes of functioning of the capital and credit markets and developing new regional patterns in more unified national economies. A new world was taking breath.

We are then back to the problem of historical evolution: is it caused—or explainable—by small, random, independent, isolated and insignificant factors, or is it moved by path-dependent, systemic changes? The question remains. Given the irreversibility of time and the evidence of historical transformation, this puzzle may be translated into another: if random changes occur, but there is no invariant probability distribution over all the state space, i.e. if the asymptotic distribution evolves as part of the history of the system itself and the process is non-ergodic (David 1993: 208), then explanation must be represented by the formulation of a dynamic model considering not only the initial conditions, but
also the outcome of the complex process of the interaction of variables throughout the history of the system. That is the work of history, of Clio.
1.4 Conclusions

New economic history represented, and still represents, a major shift in the economic historians’ assessment of history and economics. Indeed, there was widespread acceptance that ‘the discipline originated largely as a revolt against classical theory’ (Fogel 1964: 389), and such abhorrence of the transformation of economics into a branch of mathematics and of social sciences \(\text{(p.37)}\) into positivistic sciences was loudly voiced by many. Schmoller had already argued for this independence of quantitative historical economics from economic ‘theory’, and his argument was part of the original Methodenstreit. The issue of the debate is well known: the ‘theoreticians’ won the day and the neoclassical revolution was imposed as the epitome for economics. Later, it was revived under the impetus of the second neoclassical revolution, when econometrics emerged in the 1930s and deepened the differentiation with economic history. This was why Ashley, when occupying the first chair of economic history at Harvard, asked for a truce based on mutual ignorance and distance between economics and economic history. He was simply ignored by the following generations, and even when Schumpeter joined him and argued, by the turn of the half-century, that history should be part of the education of any economist, their voices did not reach far. But truce there was, not because they asked for it, but simply because no attention was paid any more in the new orthodox economics to this marginal topic: since axiomatization dominated the efforts of the young generations of neoclassical economists, imposing the rule of sophisticated econometric confirmation and theorization through modelling, they were not concerned with history, which was too difficult and too different.

But such a situation did not last long. As indicated in the first sections of this chapter, the reincorporation of history into economics became the goal of a new tribe: with the emergence of econometric history, or new economic history, neoclassical economics was back in history and the cliometricians were propelled to the forefront of their profession. Indeed, ‘New economic history represents a reunification of economic history with economic theory and then brings to an end the century-old split between these two branches of economics. Economic history emerged as a distinct discipline during the course of the mid- and late
nineteenth-century revolt against the deductive theories of classical economics’ (Fogel 1965: 94).

Fogel’s argument runs as follows: economics was devoted to static models, and therefore history could not be addressed, since historical evolution is dynamic by nature. Consequently, the divorce was understandable as long as dynamic methods were not developed and rigorous quantitative testing was not accessible. Afterwards came the moment for reconciliation, and that was the cliometricians’ major contribution to economics. Yet the old historians reacted as vehemently as they had done on the first occasion: they feared the excess of simplification involved in this excursion. This is why Chandler, more recently, repeated the same plea as Ashley: ‘I see no need to strengthen the union between the two branches and to form a new or special discipline. It seems to me we should encourage an explicit division of labor. Let us each practice our own trade, each stick to our own last, with an awareness of the possibilities and limitations of our own particular fields’ (Chandler 1970: 144).

The warning did not work. History, or at least economic history, was thus conquered by neoclassical economics—eventually not all of it, but at least a representative portion of it. As this chapter has argued, several intended and unintended consequences of this fact were the submission of the research on institutions to mere quantitative inquiries and testing, the vindication of instrumentalism against realism, and the primacy of hypothetico-deductive methods with little logical or methodological foundation. Some clarity of measurement was obtained, but at the price of rather obscure and metaphysical hypotheses about what was being measured. New patterns of rigour and exactitude were established, but the techniques designed to go further in the investigation lacked epistemic coherence. The final result of the Blitzkrieg was devastation.

A very different earlier attempt to establish rigour and clarity of measurement was that of Wesley Mitchell in his work on business cycles. As the inspirer and first director of the National Bureau of Economic Research (NBER) from 1920 to 1945, he was able to launch a programme of research into the major characteristics of successive business cycles. He decisively rejected general equilibrium theory (Louçã 1997:...
155ff.), while contributing a great deal to economic theory, as well as to quantitative methods. However, as we have seen at the beginning of this chapter, it was at an NBER Conference in 1957, nine years after Mitchell's death, that cliometrics took off. It is all the more encouraging therefore to see in a recent NBER publication (Lamoreaux et al. 1999) a rather optimistic (perhaps over-optimistic) view of trends in contemporary economic theory which might facilitate a new rapprochement between economics and history. The editors start with a realistic assessment of the depth of the schism:

To the present day, the Business History Conference is dominated by trained historians, whereas the Economic History Association is controlled by trained economists. Despite large areas of common interest, the professional reference groups, not to mention the norms about what constitutes interesting questions, pertinent evidence, and persuasive argument, sometimes seem alarmingly different. Moreover, in the absence of a compelling new interdisciplinary effort, this divergence seems likely to endure. (Lamoreaux et al. 1999: 5)

The NBER has been making such an interdisciplinary effort in its own conferences and publications, and claims that ‘. . . recent developments in economic theory provide a historic opportunity for greater communication and we think that the essays that resulted from these conferences show the new inter‐disciplinary approach to be uncommonly promising’ (Lamoreaux et al. 1999: 5).

(p.39) Lamoreaux et al. contend that the new developments in evolutionary economics mean that the profession is no longer dominated by the old neoclassical thinking of the 1960s and 1970s, and that ‘this new thinking has made economic theory much more useful for the writing of business history and vice‐versa’ (1999: 6). Referring specifically to the ideas of Nelson and Winter and other evolutionary theorists, they describe this as ‘the new economic theory’ (emphasis added). We think that this probably exaggerates the extent to which evolutionary economics prevails in the United States or anywhere else, and indeed, they concede that it is not what undergraduates are taught. Nevertheless, we applaud their continuing efforts to resolve the contemporary Methodenstreit.
Lamoreaux et al. respond to a necessity: to recombine history and economics. This is a decisive agenda: economics cannot evolve without history. In the historical framework, economic theories, models, and hypotheses must in fact be more precise and effective in understanding and explaining reality. Causality in economics must be assessed as a complex process of determination, not as determinism. Artificial reasoning must be replaced by concrete and detailed research and debate on analysis and on policy making, recuperating economics as a moral science or as political economy, just as it was to Adam Smith and the early classical authors. This argues for Clio, for remarrying economics and history as an alternative strategy to that of cliometrics—indeed, that is what this book is about.

The alternative strategy followed in the remaining chapters of Part I is based on several converging contributions, taking as a common point of departure the verification of distinctive rhythms that constitute the framework for the explanation of crucial historical processes. Hobsbawm identified this ‘secular pattern of the world economy’ as the recurrence of long periods of expansion and long periods of depression (Hobsbawm 1997: 37, 142, 229, 312), but the idea goes back to the origins of statistical inquiry in economics. Kondratiev, then Schumpeter and some of the early econometricians, were the first to support this conjecture and to take it as the starting point for new theories of capitalist development.

The empirical content and the theoretical coherence of this thesis are discussed in the following chapters. For the moment, let us just conclude, with Temin, that ‘It is unlikely that existing economic models can incorporate a deviation from equilibrium that lasts for half a century. If countries are pushed off their growth path for so long, equilibrium theory is hardly relevant’ (Temin 1997: 138).

The crucial question, therefore, is how to replace this failing equilibrium theory—and what is required is precisely an alternative to the cliometric strategy of reunification of history and economics. As we will see in the next few chapters, some mathematical economists, and not least the founders of econometrics, shared this new historical heuristics centred on the study of structural changes in the process of capitalist development, i.e. on large impacts of societal innovations and changes. In this framework, (p.40) the Industrial Revolution cannot be assessed merely from the point of view of the
increases in the aggregate volume of investment: it is the creation of the ‘new system of machines’ (Marx) or the ‘factory system’ (Landes) that is interesting for our research. And that is why the conjectures on the random effects, historical accidents, and perturbations are marginal—since they could be relevant only in the context of equilibrium theory, for it takes a stable structure to value the explanatory role of shocks.

Accidents are, of course, quite common in history as well as in daily life. In Gettysburg, Colonel Chamberlain was charged to lead the 20th Maine by mistake—it should have been the 2nd—and with his men stubbornly defended Little Round Top, preventing the advance of the Confederates and eventually changing the fate of the battle. In 1930 Hitler miraculously survived a severe car crash, which could have changed the destiny of some millions of human beings in years to come. But the difficult question science can ask and intend to answer is not about the indeterminacy of these events and unexpected mutations in space and time, but rather about their ‘long’ reasons, the changes they are part of, or that they spark—in other words, the impact and the meaning they acquire in a specific structure.

One example may serve to illustrate this point. In their first encounter in Cajamarca, on 16 November 1532, Emperor Atahuallpa and Pizarro had disparate forces: 80,000 soldiers formed the Inca army, but the Spanish conquerors were just 106 men plus 68 mounted soldiers. Yet they slaughtered and defeated that far larger army and conquered Peru. The behaviour of each soldier certainly counted, but fundamental reasons preparing this outcome include the previous crisis of the Inca Empire, the unknown military impact of horsemen, and the effective and symbolic impact of the new weapons. Retrospectively, one must add the importance of the competitive advantage of Europe in terms of the domestication of animals and plants and, consequently, the increases in population and the control of germs, the creation of industries including the military industry, in determining the outcome of Cajamarca (Diamond 1998: 67–8). To complete the picture, the capacity of the conquerors to manipulate the symbolic languages, associated with the practice of modern institutions
of power, also counted and was not at all a minor effect (Todorov 1990).

This is why we do not share McCloskey's too general conclusion about little events that have big consequences (McCloskey 1991: 27). They may do so under certain circumstances—but this is frequently not the case, and it is not what history can explain. Furthermore, the use of the metaphor of deterministic chaos is not adequate, since initial conditions refer to a certain structure whose interactions produce complex trajectories, but this structure is still a defined structure once for all, which does not evolve. From this point of view, McCloskey's conclusion is also excessive: according to her view, only narrative methods are available in this chaotic world, and these are not interpretable in a causal sense (McCloskey 1991, and for the debate, Dyke (p.41) 1990; Roth and Ryckman 1995; Reisch 1991, 1995). From the opposite point of view, Landes emphasizes the intrinsic complexity of economic history: 'Here let me state a golden rule of historical analysis: big processes call for big causes. I take this as what economists call a prior. I am convinced that the very complexity of large systematic changes requires complex explanation: multiple causes of shifting relative importance, combinative dependency . . . temporal dependency' (Landes 1994: 653).

This is certainly not the case in history, since no structure can be postulated to represent the fixed form of a social process; but a structure does indeed exist, although it is evolving. Complexity, in this case, refers not only to the feedback processes, but also and most importantly to the change of the structure itself during the process. This is evolution, and this is why history cannot use either static equilibrium theories or dynamic models based on reversibility, but must resort to processes that have the characteristics of irreversibility, non-ergodicity, and path-dependence (David 1997). The Industrial Revolution, the favourite example for this chapter, proves this point quite well. Small events could have precipitated important alterations in the industrial landscape, but we can understand the landscape itself only if we consider the systemic changes in the logic of the co-ordination processes of the large and interrelated techno-economic or socio-institutional systems. And that calls for reasoned history:
None of these advances [the new mechanical innovations], however, was sufficient in itself to trigger a process of cumulative, self-sustaining change. *For it took a marriage to make the Industrial Revolution*. On the one hand, it required machines which not only replaced hand labour but compelled the concentration of production in factories . . . On the other hand, it required a big industry producing a commodity of wide and elastic demand, such that (1) the mechanization of any one of its processes of manufacture would create serious strains on the others, and (2) the impact of improvements in this country would be felt throughout the economy. (Landes 1969: 81)

Further on in this book, we take on the task of providing an alternative account of the Industrial Revolution (Chapter 5) and of the development and impact of railways (Chapter 6). But we look first to some of the economists who, through the century, contradicted orthodox economic theory, endeavoured to re-establish the role of historical analysis in theory and practice, and tried to unveil the enigma of evolution. We turn first to Schumpeter, since he is often thought of as the founder of evolutionary economics. In fact, this was not as straightforward as it sounds as we hope to explain in Chapter 2. We turn to those efforts to understand and explain economic evolution and its mysteries, which are not simple, as Charles Péguy emphasized so eloquently:

‘Rien n’est plus mystérieux, dit-elle, comme ces points de conversions profonds, comme ces bouleversements, comme ces renouvellements, comme ces recommencements profonds. C’est le secret même de l’évènement.’ (Charles Péguy, *Clio*, 1932: 269)

Notes:

(1) Douglass North in 1963 coined, or first used, the term ‘new economic history’, which became the epitome for the new movement (Fogel 1964: 377). Gerschenkron, on the other hand, never practised cliometrics, while sharing its purpose. Yet he kept a critical attitude, and the ‘Harvard wing’ of the movement was very sceptical of the excessive reliance on the neoclassical price theory (Sutch 1982: 28).
There was also a political confrontation, mostly ignited by Meyer and Conrad's paper and by Fogel and Engerman's book on slavery: some witness that at the session on ‘Slavery as an Obstacle to Economic Growth’, organized at the September 1967 annual conference of the Economic History Association, held in Philadelphia, there was an uprising against the authors and their thesis. Fogel and Engerman took pains to argue that the authors of the original work on the topic were either leftists (Conrad) or at least institutionally respectable members of the profession (Meyer was a director of NBER), and that no accusation of a right-wing political bias in their paper was legitimate (Fogel and Engerman 1974: 16). Nick von Tunzelmann, who was present at the 1967 meeting, indicated in private correspondence that the political confrontation was nevertheless contained in the academic standards. Yet, it is true that, very quickly, ‘Broad, vague, potentially subversive themes like the history of capitalism, the social consequences and causes of poverty, were eschewed in favour of politically safe exercises deploying positivistic techniques and orthodox neo-classical models’ (Coats 1980: 202).

According to Weber, an ‘ideal-type cannot be found empirically in reality’ (Weber 1949: 90, also 42–7, 91–102).

The proposal may be found in the Frisch Archive, Oslo University.
(5) Evsey Domar was a commentator for this paper given by North in 1965, and he had been one of the economists at the Williamstown conference. Although in 1957, as Kuznets told the story, he was quite sceptical of the new approach, in 1965 he readily attacked economic historians for ‘their predilection for descriptions and tautologies and their neglect of analytical models’ (Domar 1965: 116). Yet, faced with North's outstanding criticism of the current state of cliometrics, Domar decided to refrain from adding to North's ‘self-mutilation’ (p. 116). It must nevertheless be emphasized that at that time what North was criticizing was the poor quality of the research developed under the flag of cliometrics, not the approach itself. In the same paper, he still considered economic theory to be the most suitable guardian of the temple: ‘In summary, it is my conviction that we need to sweep out of the door a good deal of the old economic history, to improve the quality of the new economic history, and it is incumbent upon economists to cast a skeptical eye upon the research produced by their economic history colleagues to see that it lives up to standards which they would expect in other areas of economics’ (North 1965: 91).

(6) North had some previous roots closer to the old historical school, since he had been a self-proclaimed Marxist in his youth, although at that time he was already engaged in a ‘long term love affair with price theory’ (Hughes 1982: 4–5). In the early 1970s his endorsement of some of the critiques of the ‘Harvard wing’ and the break with the ‘neoclassical wing’ of the new economic history movement recapitulated crucial arguments of the historical schools. When Fogel and Engerman published Time on the Cross, the rupture was already quite evident.

(7) Why not also ask why the new process of rolling and puddling, used for the transformation of pig iron into wrought iron (the ‘most important single invention during the industrial revolution’: Mokyr 1985: 10), was developed by Henry Cort in 1784 in Britain and not by someone else in France? Why did the good fortune of the anno mirabilis of 1769, when Watt and Arkwright's patents were registered, not extend to France as well? But the succession of this type of questions provides an answer in itself.
Mokyr changed his vision of cliometrics, as far as one can tell from the two versions of his 1985 paper, ‘The Industrial Revolution and the New Economic History’. In 1985 he was quite sympathetic to the new approach, despite pointing out its limits: ‘The New Economic History has shown itself best qualified to answer questions that it itself poses, often well-defined questions that yield clear, refutable hypotheses. Indeed, the very definiteness of the new methods has confined them to a narrow range of problems’ (Mokyr 1985: 2). In his new version of 1993, Mokyr strongly attacked some cliometricians’ denial of the Industrial Revolution in Britain.

Apparently, Crafts implies that these macro inventions were random events. Seen from the opposite viewpoint, modern innovation theory argues for a shift in focus, concentrating not on inventions, in relation to which a precise scientific hypothesis can hardly be formulated, but on innovation. In this framework, innovation is the relevant unit for research, being seen as an economic selection process occurring under pervasive uncertainty, bounded opportunities, the influence of technological bottlenecks and trajectories, and experimental and local search behaviour, given the heterogeneity of agents (Freeman 1994). Even if one radically claims that inventions are pure random processes, one must recognize that the selection and canalization of innovation are deeply rooted in the cumulative capabilities of the economy, and are not purely stochastic, but socially defined processes.

This is a clear example of how there is some delay in statistical categories recording the changes in reality and how these same categories may hide its main features for a time. In the same sense, there are multiple examples of new industries that are only accounted for in statistics long after they first emerged.

This can be recognized by a cliometrician: ‘The growth of cities, the increase in population, the rise in national income per head, and the shift from farm to factory happened all at once. Britain could have industrialized without expanding her population or urbanized without enriching herself. Yet in fact, she did all these things together, compounding the effects of one with the effects of the other’ (McCloskey 1985: 54).
In his rather critical 1965 assessment of the current state of cliometrics, as previously cited, North wrote that he used to give his students a simple task in order to highlight the limits of ‘old economic history’ and some of the shortcomings of certain ‘new’ ones: they should ‘make explicit models of [some] articles’ (North 1965: 90). This game, of course, is not innocent: it supposes that the logical coherence and explanatory power of a formal mathematical model is the model for explanation in history and, not surprisingly, uncovers its failure.

‘Indeed, the new economic historians have not been primarily engaged in launching new counterfactual propositions, but in making explicit and testing the ones they find in traditional theory’ (Fogel 1966: 653).

Or, more recently, in psychology: Elster argues that counterfactual beliefs are generally present in the formation of emotions (Elster 1999: 49, 265).

Or, to put it in a different form, ‘the possible-worlds account of subjunctive conditionals does, of course, allow some conditionals with false antecedents to be true and some to be false. It will simply depend on whether the closest worlds where the antecedent is true are all worlds where the consequent is true—or, equivalently, whether the corresponding material conditional is true, not just at the actual world, but at every world at least as close as the closest antecedent-worlds’ (Jackson 1987: 64–5).

Fogel chose just to compute the effect of the railroads as providers of transportation for four specific goods: wheat, pork, beef, and corn. The effects of the railroads as users of inputs, as transportation for humans or for other goods was ignored. As a generalization, Fogel suggested nevertheless that, if all goods were considered, the total savings would amount to no more than 5% of GNP.

By the very same time, Leontief, who could not be accused of anti-neoclassical bias, pleaded for interdisciplinary co-operation and for a pluralistic approach to history (Leontief 1948: 617). He later maintained his criticism of traditional econometrics for requiring dynamic stability as a condition for the legitimacy of a model, whereas the strategy of historical
inquiry should be based on the research on the ‘developmental process’ under a less aggregated form (Leontief 1963: 1–2).