Chapter 1

TRUTH ON THE CROSS

Science and Ideology

In Greek mythology, Clio is the muse of both history and epic poetry. The verb *kleio* means to celebrate or extol, while *historia* means inquiry or investigation. In Herodotus, the combination of fabulous elements and concrete facts still reflects this ambiguity. Thucydides broke more decidedly with the traditional view by delivering a style of investigation that we today would call "positivistic," even though he did not really consider himself as a historian but as a sort of political scientist.

Time in antiquity passed at a somewhat slow pace, so that half a millennium later the Syrian-born Lucian could refer to Thucydides as if he had been a contemporary. His pamphlet *How to Write History* is probably the first work on historical method to come out of the Greek cultural world. Lucian was reacting to the rather mediocre literature that had arisen in celebration of the victories of the Romans against the Parthians (A.D. 161–66). As the army of Lucius Verus gradually advanced in the Middle East, it found new adulators ready to manipulate the truth as it suited them. They produced accounts that were so improbable as to seem grotesque, and they even distorted the Greek language, cramming it with pointless Latinisms. Hence, it was necessary to reestablish the rules of Thucydides:

> Here is then what the historian should be like: fearless, incorruptible, free, genuinely truth-loving, one who calls a fig a fig and a boat a boat, as that comic playwright used to say; someone who does not pass judgment motivated by hatred or withhold it out of compassion, deference, or shame. An impartial judge, well disposed to all, but never to the point of conceding to anyone more than his due, who when he writes has no fatherland, no city and no sovereign; one who does not calculate what will please this one or that, but reports what has happened. (*How to Write History* 41, my translation)

Lucian uses forceful adjectives and alludes to renunciation, the voluntary renunciation that is required of the historian as the price of objectivity. When he operates, the historian has to be like a foreigner (*xenos*) without a city (*apolis*) and without a sovereign (*abasileutos*). For modern scholars to adhere to these precepts means coming to terms with membership of a community and avoiding the often unconscious influence that it exerts on their way of thinking. It means shunning power (not only political power) and giving up the advantages that it offers to whoever submits to it. But Lucian also warns against following fashions and conforming to trends; otherwise historiography becomes an exercise in serial imitation.

The Roots of Economic History

Before considering how Clio comes into this book, we need to introduce the main character: economic history. As in all stories worthy of the name, one cannot guess the direction in which someone is heading without knowing where they started from. The tale of Little Red Riding Hood opens in the mother's kitchen and finishes in the wolf's stomach. The perils of the wood would be difficult to imagine without the prior experience of the reassuring peace of the village.

In the first place, economic history has not always existed. During the Renaissance, nobody would have found it of very great interest to deal with the economies of the past. When in the 1510s Machiavelli wrote his *Discourses on Livy*, his concerns were the structure of republics, warfare, and leadership. Machiavelli would enter his study dressed in "regal and courtly" attire, as he told Vettori, to converse with the ancients, the great statesmen now departed, and they could respond to him precisely because they knew how to write. He would ask them about "the reason for their actions" (1513, p. 142) and not about what they produced or how their fellow citizens made a living.

Two centuries later, Edward Gibbon (1776–89) did not find it beneath him to deal with manners and customs as long as it could be useful for understanding the political collapse of an empire. Nowadays it would be considered as coming within the domain of social and cultural history. Yet the economic past started to become a matter for investigation precisely at that time. Gibbon's contemporaries sensed that the economy was changing irremediably and that new values were coming to the fore. Even when they are not historical works, historical references abound in the writings of Smith, Malthus, or Marx. One of the first cases in which the elevated term "history" is applied to a subject that is anything but elevated is the *History of the Cotton Manufacture* written by Edward Baines (1835), a forerunner of David Landes's triumphalistic account *Unbound Prometheus* (1969)!

However, only when economics became a deductive science, and one prone to universalize particular, culture-specific patterns of behavior, did economic history acquire its own identity. This transition was completed in the final quarter of the nineteenth century and was not entirely unexpected.

As early as the 1840s, the German economist Friedrich List, who was heir to the cameralist tradition, had targeted the theories of the British classical economists. Because he also questioned their policy views, his attack had obvious practical implications. List criticized the "prevailing theory" for the way it hastily generalized conclusions drawn from observing the first industrial nation and mistaking them for laws of nature. "Not considering historical facts, except so far as they respond to its particular tendencies," he maintained, "it knows not or disfigures the lessons of history which are opposed to its system" (1841, p. 65). Hence, Smith overlooked the importance of the nation as an economic actor, while Ricardo interpreted rent as the price of the natural fertility of land and based his whole political economy on this principle:

> An excursion into Canada would have afforded him proofs, in every valley and on every hill, that his theory was built upon sand. But having only England in view, he falls into the error of supposing that the English fields and meadows, the apparent natural fertility of which produces such large returns in the shape of rent, have been always the same. (List 1841, p. 335)

Richard Jones (1831), the Englishman who opposed Ricardo, had been motivated to investigate comparative economic systems by considerations of a similar tone.

But these were relatively minor sins. Despite the shortsightedness of their insular perspective and their inclination to overgeneralize, the classics in any case started from an analysis of social aspects: this involved in Smith's case the separation of the public from the domestic sphere and in Ricardo's a theory of the conflict between classes and groups. In the late nineteenth century, on the other hand, the unit of analysis adopted by marginalism was disembedded individuals who lived by maximizing their own gratification and monetary income. In the most extreme version of this approach, known today as neoclassical economics, one presumes the agents are aware of the rules of differential calculus and how to apply them to all the possible combinations of goods and production choices that exist. Marginalism spread to Britain, Austria, Switzerland, and the United States, while France and Italy were only mildly affected. Germany remained practically immune until World War II.

The great dispute on method, the *Methodenstreit*, broke out in the 1880s. On one side was Gustav Schmoller (1883), the leader of the German school, in whose view one could not formulate any economic theory that was not based on the historical analysis of society. On the other side was the Austrian Carl Menger (1884), who maintained that it was possible to know the principles of economic behavior of individuals a priori. Similarly in Britain, William Cunningham (1892) argued against Alfred Marshall, but the outcome of the controversy was less fortunate, one reason being that Marshall had embraced marginalism only very tentatively (Hodgson 2001, p. 107). Arnold Toynbee (1884) also devoted himself to the study of the "industrial revolution" (he actually popularized this expression) motivated by the wish to reject theoretical commonplaces such as the universal benefits of free trade.

Once it had entered through the back door into the pantheon of history, which Edward A. Freeman, a contemporary of Cunningham's and Regius Professor at Oxford, used to define as "past politics" (Bentley 2005, p. 192), the study of the economic past made great advances after the 1920s. In this difficult conjuncture, the Great Crash, the instability of the interwar period, and increasingly complicated industrial relations made a deep impression on public opinion and reminded contemporaries of the need to look back into the past.

But as the "civic" significance of economic history made headway, it became more and more divorced from economics. Berlin ceased to be the intellectual heart of the Western world, and the model of historical-institutional economics that the Germans had exported to some extent everywhere soon lost ground. In the postwar period, John Maynard Keynes, the second antagonist of neoclassical theory was no longer. He had conceived economic knowledge as an art that was supposed to guide "practical men." His intuition, with its far-reaching import, that disequilibrium was the rule in capitalist economies and equilibrium the exception, was ably neutralized in the classrooms of MIT, following a famous article by Hicks (1937).

Having abandoned the ambition of formulating grand theories based on empirical evidence, economic historians dedicated themselves entirely to the interpretation of the latter. "History for its own sake" might be the most suitable motto to describe this new phase. The change of direction certainly brought with it some positive aspects, because economic history was worthy of a professional body of devotees, but it also left open a latent conflict with the discipline it had separated itself from, namely economics.

The Crisis of Economic History

Today economic history is going through a deep identity crisis brought about by the development of a movement founded in the United States at the end of the 1950s and known as "new economic history" or "cliometrics."

History is normally expected to improve our understanding of the past. It is probably agreed that what distinguishes good historical research is its capacity to throw light on the workings of societies that differ to varying degrees from our own. On the other hand, the (unconfessed) aim of cliometrics is not to increase our knowledge of the past. It is to create narratives of the past compatible with neoliberal economics, and often it is a highly ideological exercise to endorse specific worldviews, theories, and policy recommendations.

Until relatively recently, European economic historians tended to ignore this phenomenon, and starting a dispute with cliometricians was considered a waste of time, because, it was said, "they do not form part of the historical profession." As long as these scholars were operating in the United States and dealing mainly with American history, they were no great menace. But nowadays this line is no longer tenable. Two decades ago one might have smiled at McCloskey's claim that the new economic history had "won the West," imposing a "Pax Cliometrica" (1987, p. 77), but in the 2010s the risk appears to be more real. In twenty years there has been a burgeoning of armies of American-trained PhDs on European soil, and even if they are still a minority, it is a very aggressive minority. Moreover the cliometricians use increasingly sophisticated tools of persuasion, and their works are sometimes taken to be reliable by historians who are specialized in other fields and who are not familiar enough with economics to be able to form an independent judgment. Cliometricians occupy a large share of the publishing market in the English-speaking world and enjoy great visibility. This "literary genre" now covers the history of all five continents, and its timeline stretches from antiquity to the present day.

But in the meantime dissatisfaction has been building up not only among historians but also among dissident economists. Writing from a neo-Schumpeterian perspective, Freeman and Louçã, for example, have called for "remarrying economics and history as an alternative strategy to that of cliometrics" (2001, p. 39). Their appeal for a "reasoned history" recalls that of Fritz Redlich, who was one of the first critics of the new economic history: "In my opinion," he wrote, "the future belongs to both analytical qualitative and quantitative economic and social history" (1968, p. 96). At the time, "analytical" history meant problem-oriented history, an approach that was as much against narrative history as against cliometrics.

The problem is to define what the "alternative strategy" is today. There still seems to be general confusion about the nature of the "new institutionalism," which Freeman and Louçã themselves see as a break with neoclassical economics. It is in fact an attempt at product differentiation with deference to the mainstream and is dictated solely by the requirements of academic politics. Its invention became indispensable the moment Douglass North realized that the application of neoclassical models to history was something "that quickly runs into diminishing returns and leaves the economist with the conviction that we are marginal if not dispensable to the profession" (1978, p. 78). He has recently acknowledged that in the 1970s "economists came to see economic history as a luxury rather than as adding a new dimension to economics. . . . The result was that the demand for economic historians decreased. . . . The new institutional economics inspired by Ronald Coase's work was a consequence" (2008, p. 211).

What was needed was a formula that differed from neoclassical theory just enough not to become an identical copy or to contradict its basic principles; it also had to sound familiar to the dominant orthodoxy. The new institutionalists collected the accusations of unrealism that had tormented neoclassical theory since its beginnings and exploited them to their own advantage. They patiently subjected it to a patching-up operation and then presented themselves to mainstream economists as the ones who could save them from the attacks of historians, sociologists, anthropologists, and the like; and this way their chairs were kept safe.

Chapter 2 gives a detailed analysis of the new institutional paradigm and its strange alliance with rational choice theory. The most recent developments of American-style economic history are discussed in chapter 3. The second part of the book presents an alternative way of practicing economic history that developed mainly in Europe in the postwar period. Chapter 4 is dedicated to microeconomic history, while chapter 5 covers macroeconomic issues. The potentialities of these approaches are highlighted in the light of recent theoretical results in fields such as economic sociology and anthropology. Chapter 6 is a manifesto for the reconstruction of economic history and calls for a new pact between history and the social sciences in order to counter the way economists have abused the past.

The main point being made here is that we need a different paradigm of historical research that is not subject to economic theory but contributes toward renewing it. If theory is to be based on facts, it makes sense that history should correct theory and not vice versa. It is not a question of inventing this paradigm out of nothing but of learning from the lessons of certain innovative economic historians of the twentieth century. They suggest a third way between a narrative type of history and one reduced to applied social science.

It is important to avoid a possible misunderstanding from the outset. Given the target of my criticism, some may be tempted to think that this book wishes to offer up a "progressive" reading to set against a "neoliberal" reading. The stereotype of the Rive Gauche intellectual looking down on economic activity and conceiving the market as grubby and immoral is so deeply rooted that some have even felt the need to write books to defend "bourgeois virtues" (McCloskey 2006). I am convinced that ideology of any type taints the work of the historian, and in this book a special effort has been made to avoid it. The unnatural effort at alienation that Lucian demanded would certainly be hard for anyone, but constantly striving toward such an objective is what distinguishes honest historical research (Cipolla 1991, p. 66). Thus, if the neoliberal drifts are the subject of this book, this is only because they reflect recent developments in the discipline; the same thing could have been said about certain forms of Marxism, if that did not now belong to another phase.[1]

Even in the past history was largely conditioned by ideology, and it would be pointless to deny it. In the twentieth century, Marxism and liberalism produced influential paradigms and informed the work of hundreds of scholars, but this influence was generally kept within the limits of decency and only rarely did it go so far as to pervert the work of the historian. Two authors such as Eric Hobsbawm and Walt Rostow certainly started from differing ideological positions, but advantages could be gained from reading both once their premises had been allowed for. Nowadays, however, a well-organized group of scholars seems to believe it holds the monopoly of knowledge; what is more, they demand the respect for their theses, presented as objective truths, that is due to the hard sciences.

On the Shoulders of Giants?

When historians are convinced that earlier interpretations of the past are correct and not some fashionable trend of the moment, too often they tend to dilute their arguments by adopting a halfway line between "old ideas" and fashionable trends. In rehabilitating a former generation of historians, the present book takes a necessarily inconvenient stand and does not seek facile compromises.

At this point, it would be useful to clarify my view of the evolution of historiography. I do not think that in the historical sciences there is necessarily any progress, as there is not in economics where rival theories have always coexisted (see Boehm et al. 2002). What the historians of a generation write may or may not be better or more informed than what their immediate predecessors wrote. In other words, history is not completely

[1] In 2004 a conference organized by the British Academy was significantly called "Marxist Historiography: Alive, Dead, or Moribund?"

cumulative knowledge. Each generation constructs and reconstructs the past guided by the sensibility of the moment.

A work such as the medievalist Otto Brunner's *Land und Herrschaft* was considered orthodox in the Germany of 1939, when it was published, not because it represented the official position of the Nazi regime, but because it responded to the common feeling at the time. Brunner argued that lordship was not the product of constitutional history drawn up at the table; institutions were not of a legal nature and did not originate in a contract but evolved from patriarchal rule over the household (see also Brunner 1958) and from the relationship that bound a charismatic leader to his band of warriors. Orthodoxy in the world after 1989 is represented by the neo-Hobbesian account of North, Wallis, and Weingast (2009), which maintains an entirely antithetical truth. Brunner clearly had in mind the role of the *Volk*, which he believed could have a direct and nonmediated relationship with forms of authority; likewise, it is clear that North and his coauthors wish to elevate economics over politics, the individual over society, and impersonal interactions over social bonds.

Even when it is a highly academic activity, the writing of history is still connected to the process whereby society maintains, transforms, and passes on public memory. The anthropologist Mary Douglas puts the question this way:

> Every ten years or so classroom text books go out of date. . . . The revisionary effort is not aimed at producing the perfect optic flat. The mirror, if that is what history is, distorts as much after revision as it did before. The aim of revision is to get the distortions to match the mood of the present times. . . . When we look closely at the construction of past time, we find the process has very little to do with the past at all and everything to do with the present. (1986, p. 69)

Should this awareness result in embracing relativism and asserting that everything we write is irremediably linked to the moment and place in which we live and to our particular point of view? No, of course it should not. Today, a century and a half after first investigating the archives, we know a great deal more about the past than at the time of Leopold von Ranke, but the point is that there is no reason to prefer one historical explanation only because it is recent or more in line with the consensus of the moment. I have always found this attitude extremely naive, which among other things fosters conformism. Not only can we measure ourselves with the scholars of the past, but we have a duty to do so. The historians of the postwar period, in particular, were working on the same sources that we are using and were often even more scrupulous and systematic. It is paradoxical that, while the rational choice narratives are based on the argument that human nature is always the same, our academic predecessors are seen as being in some way psychologically inferior or less rational.

But there is another reason why the historians of the past should be taken seriously. A good knowledge of historiography—and the same could probably be said of the history of economics or the history of sociology—greatly diminishes the current practitioners' claims to being innovative. When economists say that "history matters," they invariably think of "path dependence," the concept introduced by Paul David (1985) and often cited as if it was a Copernican revolution. This idea not only is not new but had already been shown to be unhistorical at least seventy years ago. In his *The Historian's Craft*, published posthumously in 1949, Marc Bloch warned against the "idol of origins," which leads to "confusing ancestry with explanation" (p. 27). Just as the seed from which it develops contains the destiny of a plant only to a minimum degree, so the history of social facts results from forces that are not found in the "initial conditions" to any great degree and whose effects are not propagated automatically.

Understanding the Context

The new economic history is an unmistakably American phenomenon that cannot be understood without taking account of a combination of cultural factors, the intellectual climate, and institutional circumstances in the United States between the 1950s and 1960s.[2] With regard to cultural factors, it is worth hearing what Joseph Schumpeter, a direct witness, had to say. With the excuse of opening up a parenthesis on Ricardo, in a footnote in his *History of Economic Analysis* he comes out with a harsh verdict on his students at Harvard:

> I do not think that Ricardo ever did much historical reading. But this is not what I mean. The trouble with him is akin to the trouble I have, in this respect, with my American students, who have plenty of historical material pushed down their throats. But it is to no purpose. They lack the historical *sense* that no amount of factual study can give. This is why it is so much easier to make theorists of them than economists. (1954, p. 472n)

[2] Its reception in Europe was decidedly cool (Van der Wee and Klep 1977). In Britain, thanks to Charles Feinstein, cliometrics managed to penetrate Oxford, even though Roderick Floud (2001) had previously practiced it as a graduate student under the influence of Lance Davis. Today, along with the London School of Economics and perhaps Warwick, Oxford is still the only British university where such an approach has a significant presence. By contrast, about 50 percent of the articles currently published in the *Economic History Review* are in the cliometric style.

There is undoubtedly some truth in this argument, but Schumpeter may have jumped to his conclusions too quickly. It is by no means easy to have a "historical sense" if one has grown up in a young town with perpendicular streets named "Main Street," "North Street," or "South Street." In the Old World, borders between states and regions reflect linguistic and cultural identities that have resulted from a millennial relationship between man and his natural barriers, while in the New World they have been drawn up with a stroke of the pen, though this can hardly be considered a peculiarity of the United States. Besides, how can it be explained that some of the most brilliant historians of the twentieth century, and of the present century, are American? How can their unique capacity for combining their close study of the present with a depth of vision be explained?

The problem is probably related to the mindset of the students who enrolled in an economics program. The standard of American secondary education is still quite low in comparison with that of a European lyceum or a British grammar school. At high school, students receive very practical training but are denied the study of philosophy, which underlies critical knowledge, and they are used to thinking of history as coinciding with the constitutional and political events of their own country. Hence, their first serious contact with the humanities takes place at university; but this applies only to those who decide to specialize in these disciplines. Yet, even philosophy is a dangerous exercise if it is divorced from history, and it can easily lead to baseless argument. One wonders if the author of the following passage has any idea of what the "economy" she is speaking about really is: "We have, in sum, a Parmenidian economic world. The flux of everyday life is illusory. Homo oeconomicus has never evolved, at least since the emergence of capitalism and possibly long before that. . . . One wonders why it took economic historians so long to catch on" (Schabas 1995, p. 198).

In any case, though Paul Samuelson did stand out from among Schumpeter's students, Alfred Conrad and John Meyer, the founding fathers of what would be called cliometrics, were also among them. What was meant by the term "cliometrics"?[3] Even though it would like to suggest the application of certain statistical methods to history, the distinctive contribution of this approach "has not been so much the use of 'econometrics' but the use of *economics*—the application of standard economic reasoning in the posing and answering of historical questions" (G. Wright 1971, p. 416). Others went as far as to define economic history as "a form of applied neo-classical economics" (Temin 1973, p. 8).

[3] The term was coined by Stanley Reiter, a mathematical economist at Purdue, in 1960.

The intellectual elements out of which the new paradigm was able to develop, starting at Purdue University—a center of excellence for aeronautical engineering, with an abundance of "computers but no library books" (Hughes 1991, p. 233)—were linked to a milieu open to new ideas. This type of environment gives American academic life its characteristic and unequalled dynamism, but it also has less positive aspects, which according to the late Bob Coats include "the concomitant tendency to exaggerate claims to originality; a disrespect for past achievements . . .; the overinvestment of resources in academic trivia and gimmickry; the academic pressure to 'publish or perish'; and—especially in this case—the veritable passion for quantification and measurable standards of performance" (1990, p. 14).

Compared to Britain, economic history had never enjoyed real disciplinary autonomy but had grown up under the wing of history as well as of economics departments (Cochran 1969, p. 1563). In the postwar period, the latter underwent important changes, when axiomatization and the development of econometrics gave the decisive boost to transforming economics into a mathematical science (Weintraub 2002). Consequently, on the one hand "'making like economists' meant looking down on the practitioners of 'softer' subjects . . . in an era when a simplistic concept of the scientific ideal prevailed" (Coats 1990, p. 15), while on the other it was a survival strategy for those having to live with a much more powerful neighbor (Coats 1980, p. 190).

The new economic historian had to go to the great supermarket of economic theory and select a model to apply to the concrete circumstances of the past as need be. Anyway, it is significant that the chosen theory has always been the one practiced in mainstream departments; there has never been a post-Keynesian cliometrics, or an Austrian cliometrics! Considering the axiomatic nature of neoclassical economics, this implied believing in the existence of universal laws of human behavior that were recognizable a priori. However, it would be wrong to attribute the first cliometricians with undue methodological awareness. In many cases, there were superficial reasons for choosing to become applied economists. A more perceptive author, Lance Davis, condemned the poor critical sense of his colleagues and defined the theory as the "siren lure" (1968, p. 78), but the warning seems to have fallen on deaf ears.

The faith in theory is part of a more general mental attitude. Recently I happened to attend the seminar of a PhD candidate from an Ivy League school. In his paper, the size of the population of early modern towns was used as a proxy for wealth to test "economic growth." One of those present pointed out that Naples was the most populated city of the preindustrial West, yet it certainly did not have the aspect of a rich or dynamic place. At this point, the PhD candidate produced an astute argument (from the point of view of an economist), saying that what was important was not the population stock but the rate of its growth. From the historian's perspective, this is a rather clumsy attempt at putting matters right; by the same token, one could object that, being notoriously passionate, Neapolitans reproduced just as quickly.

What distinguishes the gaze of the economist from that of the historian? It is mainly the fact that it is based on deduction rather than on induction. In the foregoing example, the explanation given was a logical one, but it was not plausible, and it fell short as soon as the reasoning was transferred from the vacuum of the imaginary model to a concrete situation. In 1973 Carlo Cipolla, who had a unique penchant for wit, wrote a celebrated parody of this way of reasoning. He invented a longing that Peter the Hermit had for pepper; this longing was to cause the First Crusade and set off an unlikely sequence of changes in population, trade, and the production of goods and services in the twelfth century. The "aphrodisiacal constant of pepper" was inserted into the multiplier model to form the equation of the "economic development of the Middle Ages." In this way Cipolla was able to show that totally implausible, but perfectly logical, narratives could be fabricated.

The second distinctive trait of the new economic history that can still be recognized, even though it has become routine and is no longer at the cutting edge of the profession, is the particular use of statistics that qualified it as "econometric history." It developed out of the need for model validation. This method also differs in one fundamental aspect from sophisticated techniques of descriptive statistics and time series analysis. While the latter leave the responsibility of interpreting quantitative data entirely up to the historian, a multivariate regression gives rise to a set of correlations between a dependent and several independent variables, each potentially able to explain the phenomenon with a certain probability and magnitude. Multivariate analysis thus becomes an *explanatory tool*. However, it is extremely dangerous to attribute the significance of a causal link to this type of connection. Two or more variables can be correlated for the most diverse reasons, but, more importantly, there is no guarantee that the ones that really matter have been included in the model.

An interesting example is contained in an influential theoretical work of Acemoglu and Robinson. Four regressions, carried out by combining different indices, suggest a negative association between democracy and inequality. Apparently, "countries that are more unequal . . . tend to be less democratic." However, Acemoglu and Robinson recognize in all honesty that it is impossible to establish causal relations between the two phenomena. "Moreover," they continue, "these correlations are not always robust to the inclusion of other variables in a regression model" and, after citing the results of studies that are incompatible with each other, conclude: “The existing empirical literature is, therefore, rather contradictory” (2006, pp. 59–62).

The fact that even economists are beginning to consider econometrics as inconclusive, or at the very least to realize its limits, should induce cliometricians to start seriously asking themselves questions.

Reality and Fiction

The hope of transforming economic history into a deductive discipline initially found expression in the enthusiasm for “counterfactual history.” Robert Fogel’s book *Railroads and American Economic Growth* (1964) led the way for this enterprise. Historians had traditionally considered the railroads an important factor in the economic development of the United States in the nineteenth century. Fogel thought of these conjectures as being little more than idle chatter and wished to subject them to rigorous testing. He built a model that would allow him to have an idea of what the American economy would have been like in 1890 without the railroads. The economy was represented as being in general equilibrium, something which even on paper is problematic. Albert Fishlow (1965), who embarked on a similar but less audacious exercise, chose a different year (1859) and came to opposite conclusions.

Fogel did not stop at “removing” the railroads but thought up the details of an alternative system of transport. He designed a network of canals and roads that were suited to transporting the same amount of goods and following identical trajectories. In particular, he started from the assumption that the railroads did not create traffic and that the volume of goods was not related to the cost of the service. Under these hypotheses, he calculated that the macroeconomic impact of the railroads had been modest.

The limits of this imaginary construction lie precisely in the fact that it is imaginary. It cannot be excluded that a different and more costly system of transport would have followed different trajectories and conveyed fewer quantities of goods; moreover, the prices of raw materials, starting with coal, are certainly not an independent variable. But the problem lies at the root: was it the towns and trade that attracted the railroad, or was it the railroad that brought about the rise of the towns and stimulated trade and industry? Right from the start, Fogel implicitly adopted the former standpoint and thus predetermined the outcome of his investigation. Paradoxically, he depicted an economy without railroads benefiting from the effect of the railroads!

The cliometricians argued that resorting to counterfactuals in history writing was inevitable and that they had only made a quite widespread way of reasoning explicit. According to Lance Davis, “the unique contribution of the historian lies in his ability to understand sequences of events (*i.e.*, to interpret causal relationships). Any step in this direction, however, leads necessarily to the use (implicit or explicit) of a counterfactual argument” (1968, pp. 75–76).

Here there is a fundamental misunderstanding. History is not a futile search for immediate causes. The historian is not interested in knowing *if*, but *why*. If Franz Ferdinand of Austria had not been assassinated at Sarajevo, World War I would not have broken out on July 28, 1914, and perhaps not even that summer. But does it make sense to wonder how things would have turned out? No, it does not, unless one believes that events are the driving force behind history. Would it not be of far greater interest to explore the relationship between the war, late nineteenth-century nationalism, and the second industrial revolution?

Wondering what the world would be like if the Axis had won World War II and other puzzles of this type that enthuse military historians so much is very tempting (Ferguson 1997; Tetlock et al. 2006). Could the Axis have won the war? If Hitler had been satisfied with the successes achieved up until 1941, and the Allies had accepted the existence of a continental empire, the scenario does not seem to be implausible. It would undoubtedly have involved dramatic changes for the lives of millions of people, but from the point of view of the history of geopolitical structures, would it not have been a *déjà vu*? After all, in the previous century, a similar empire had materialized with Napoleon. It lasted ten years and, even then, the invasion of Russia was fatal. In chapter 5, we return to the relationship between event and structure in the historical explanation.

Yet the counterfactual reasoning that Davis speaks about is to do with the causes of certain phenomena and not with their repercussions. It is in fact a form of post hoc fallacy, that is to say: “A precedes B,” therefore “A causes B.” Furthermore, the interactions between A and B cannot be isolated from elements C, D, E. . . . If A does not happen, then D is also likely to change, and so on, in a way that is certainly not automatic. This is what underlies the harsh (and somewhat crude) judgment of E. P. Thompson, in a book otherwise dedicated to criticizing Althusserian Marxism, when he mentions “the counterfactual fictions; the econometric and cleometric groovers—all of these theories [that] hobble along programmed routes from one static category to the next. And all of them are *Geschichtenscheissenschlopff*, unhistorical shit” (1978, p. 108).

The Defining Moment

The year 1974 was a turning point in these matters. What for fifteen years had been a movement of Young Turks became institutionalized. Counterfactual history, all in all, had been a passing fashion. *Time on the Cross*, Fogel's next book, which was coauthored with Stanley Engerman, was a compendium of all the essential ingredients of the new approach: the quantitative techniques just described were there; the ample recourse to deductive reasoning was there; there was sensationalism, with strong claims aimed at shocking the audience; and a profound lack of respect for historical scholarship was also displayed. The publishing operation was carefully studied, and the book was entrusted to a major trade publisher and organized in two volumes. The first volume was aimed at a general readership; it was without footnotes and contained the narrative. In the second volume were the calculations and a summary indication of the primary sources.

The thesis put forward by Fogel and Engerman was very simple. The historians who had up until then dealt with slavery in the antebellum American South had completely misunderstood the phenomenon, because they had not adopted the conceptual framework of neoclassical economics and did not have the econometric tools. Fogel and Engerman argued that slavery was a highly efficient system based on a mutual agreement between benevolent, profit-maximizing masters and responsible, income-maximizing slaves. The former did not act according to "patriarchal commitments" but followed their capitalist instincts. The latter had absorbed the ethic of hard work and Victorian values from their masters; they were more productive than midwestern agricultural laborers and enjoyed a higher standard of living than "Yankee" industrial workers.

The claim about the efficiency of slave agriculture in the Old South had already been made and had been much debated since the time of Conrad and Meyer (1958). The radical innovation of *Time on the Cross* was the system of motives attributed to the actors, who reacted to incentives exactly in keeping with a microeconomics textbook.

The values of the Protestant slave owners were certainly different from those of the slave owners in the ancient world, and their family lives had little in common with the customs of the Portuguese in Brazil (see Freyre 1933). Like the New England settlers, they had a sense of their economic interests, and behind the importation of slaves there was a calculation of this type. But this does not mean that the slaves were *homines oeconomici*, or that they had given up their traditional culture to adopt WASP values (M. Smith 1998). Historical research is not required to discover this: the cultural peculiarities of the various ethnic groups that make up American society are still evident (see chapter 6), just as more than two centuries of coexistence have not eliminated differences between the Québécois and the English-speaking Canadians.

Fogel and Engerman set themselves up as fighting a battle in the name of science, but in *Time on the Cross* the ideological element lurking behind the math played a role that was without precedent. Slavery was like a heavy stain on the history of a country that had made a banner out of civil and political rights. Showing that it had been profitable for the South, and that the slaves were well treated and by no means hostile, responded to a need to blot out the original sin of the nation and assuage its sense of guilt in a period marked by great social struggles.

Even the most superficial reader is struck by the work's systematic decontextualization of the data from any qualitative reference. But there are more serious problems. Herbert Gutman's book-length review *Slavery and the Numbers Game* (1975) divulged the biased samples, factual errors, and numerous exaggerations in the quantitative analysis. Equally ferocious criticism came from cliometricians (Walton 1975; David et al. 1976). A year after its publication, *Time on the Cross* did not seem to have an academic future despite the strong impact it had had on the media. Thomas Haskell wrote an obituary in the *New York Review of Books*, which ended with the charge that "now [Fogel and Engerman] must prove . . . that their book merits further scholarly attention" (1975, p. 39). But the tone of the controversy gradually died down, and Fogel's fidelity to the economists was rewarded with the Nobel Prize, which he shared in 1993 with North. It comes as no surprise that in a recent publication dedicated to the self-celebration of cliometric achievements there is hardly any mention of these early controversies (Lyons et al. 2008).

The way for the new economic history had been pointed. If nothing else, *Time on the Cross* had shown something: namely, that it is very simple to condition the result of a regression through the choice of independent variables with the aim of confirming or refuting a theoretical hypothesis. This is what led the cliometricians away from primary sources and toward a selective and opportunistic use of data. As Bernard Alford has noted, "some practitioners of the new approach—unencumbered by the need for archival research—frequently overreached themselves as they sought to graft selective historical evidence onto the latest fashion in economic theory and sell it as economic history" (2004, pp. 640–41).

Among outside observers, the idea had long prevailed that this unfortunate trend could not last. In his essay on the method of economic history about twenty years ago, Cipolla wrote: "If the 'new economic historians' want to come to grips with historical reality in all its complexity they will have to abandon their *esprit géométrique* for the subtler if less elegant *esprit de finesse*. This may actually happen sooner than expected" (1991, p. 70).

The *esprit de finesse* is essential for understanding the past, but unfortunately it does not enable us to foresee the future. If Cipolla's prediction had proved to be right, the rest of this book would never have been written.