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STEFANO'S FACE

Comment on "Spleen: the Failures of the Cliometric School" by Stefano Fenoaltea

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ABSTRACT

I agree with Stefano. On all the three facets of cliometricians' activity – economics, history, and economic history – we can spot the sign of a failure. Cliometricians recognized that economic history needs help from economics in order to overcome its serious methodological flaws; but they failed to realize that economics, in order to be useful, needed some help too, from history. Context is all for history, but also for economics. Stefano argues that cliometricians, mostly American, failed because their narrow, provincial cultural background kept them from catching up with French post-modernism. I would argue that they failed because they disregarded American pragmatism, which might well have saved their souls; they didn't lack a cultural background on which to build a sounder methodology, they simply chose to ignore it. I conclude that three specific failures don't add up to an overall failure. Too many cliometricians may remain wedded to whiggish and naively positivist views, but ours is an open and lively discipline, and it cannot be penned into one orthodox enclosure.

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1. Stefano's Face [1]

"Spleen" is the key word in the paper. It is the first word the reader comes across and it does not appear thereafter, apart from the quotation

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[1] In the following, I refer to the "Spleen" paper only by indicating page numbers.

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from *Les Fleurs du mal*. Spleen is a deep existential word to which Stefano adds an epistemological connotation. The paper is atypical. It deals with methodology as a fact of life, as an emotional past, which is not here anymore. We cannot smell the paper of the old books, we cannot see the light of the libraries, we cannot change it. He defines cliometricians' failures as embarrassing. And adds that "with luck Valhalla does not exist at all, and our embarrassment will end with our death" (21).

In the *Epilogo* of one of his latest books, *El Hacedor*, Jorge Luis Borges writes:

> Un hombre se propone la tarea de dibujar el mundo. A lo largo de los años puebla un espacio con imágenes de provincias, de reinos, de montañas, de bahías, de naves, de islas, de peces, de habitaciones, de instrumentos, de astros, de caballos y de personas. Poco antes de morir, descubre que ese paciente laberinto de líneas traza la imágen de su cara.[2]

Stefano, after fifty years of great scientific production during which he actually assigned himself "la tarea de dibujar el mundo" – the task of portraying the world, at least the Italian economic one, 1861-1913 – indeed has drawn, year after year, provinces, mountains, ships, fishes, houses and people. After such a long time he is taken by anguish: connecting such lines does not render his face. Or does it?

It is difficult to answer. In principle, it seems to be an unreasonable outcome. We cannot not find our face drawn by the lines of our life-long work. As Stefano himself reminds us, "our theories, our facts … are constructs that define and project an image of ourselves; they are shaped by fears and aspirations so deep we do not admit them to our conscious minds, by prejudices so strong we do not recognize them" (9). It is difficult also because Stefano's self-alleged failures and the failures for which he blames his fellow cliometricians are intertwined in the paper. I suspect they are, under many respects, inextricable.

This is the background of the paper. It deals with three alleged failures. They are related to the threefold identity of cliometricians, who are economists, historians, and economic historians. The failure as economists, Stefano argues, occurred because cliometricians, despite their potentially wider cultural and epistemological horizon than economists', failed to help their colleagues out of a backward, "primitive" and naïve view of science. Clio-

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[2] Borges (1960). The title of the book in English is *Dreamtigers* and the quotation from the *Epilogue* reads as follows: "A man set himself the task of portraying the world. Through the years he peoples a space with images of provinces, kingdoms, mountains, bays, ships, islands, fishes, rooms, instruments, stars, horses, and people. Shortly before his death, he discovers that the patient labyrinth of lines traces the image of his face".

metricians' wider cultural and epistemological horizon was only potential, never actual: this led to the second failure, as historians, which is the other side of the coin. Cliometricians have failed to understand what measurement really is; and this is again a failure of cliometricians as economists. Economists are not required to deal with historical sources, to interpret and contextualize them, nor are they accustomed to struggle to extract statistical information from historical sources which were not produced to that very purpose. Cliometricians should. But their identity as economists did prevail, hence they have appeared not to be up to their mission as historians. Cliometricians, according to Stefano, have failed also as economic historians. They have failed in understanding what GDP really is. In fact, it is not a measure of something, he claims; it is a social construct, one of many possible constructs.

The introductory section ends with an interesting sentence: "It took four years of graduate work to train me as an economist; I have spent the succeeding fifty trying to train myself as a historian" (6). We face a palpable paradox here, which is the core of the claimed cliometricians' failure as economists. According to this biographical metric (i.e., 4/50), economics, which is the "mistress" of the cliometric house, its core methodological machine (quantification being "a mere servant"), turns out to be a mere technical tool to make sense of historical data. Not differently from an engineer who learns how to build a bridge and then applies the acquired technical knowledge in the real world, economics in this view is taken to be a corpus of out-of-context, abstract techniques.

This leads to the bizarre idea that, once you have studied the most recent literature, you have mastered "economics" at its height: a graduate student today, Paul Samuelson proclaimed, is a better economist than Keynes, Stefano recalls (7).

Cliometrics was born in such a cultural context: a context in which it was not important to understand what a historical context is. It simply disregarded the relationship between theory and reality, between language and concepts. But Stefano had potential trump cards, he reminds us: his European half (see his footnote 5) and the struggle in his "formative years, with Latin and Greek". Unfortunately, we now know that such a view of the economist as engineer would become hegemonic in the ensuing decades. It is still often, I suppose, how economists usually think of themselves.

Stefano regrets that cliometricians didn't spur their fellow economists to use their critical spirit, urge them not to lose touch with the broader world's culture and, ultimately, with reality.

> Only we cliometricians are at once trained economists respected by economists, and professionally tied to history, to the humanities, to the broader culture

of the West. It was our duty to the profession, for no other economists could do it, to cultivate those external ties, to bring contemporary culture into our Departments of Economics, to drag our economist colleagues out of the nineteenth century and into the twentieth. We have done nothing of the sort. We have restricted our *fréquentations* to other economists […]; we have become and remained economists pure and simple (in both senses of the word). We have become no more than economists who deal with yesterday's numbers rather than today's. (11).

This is a failure, indeed. And it was avoidable. In the opening chapter of John Hicks's *A Theory of Economic History*, published in 1969, two years after Stefano's PhD thesis, we find some very interesting thoughts, to which we should still pay attention today. Hicks argued that "in spite of the vogue of 'Quantitative Economic History', economic historians are under less temptation than economists to see their subject as purely quantitative" (Hicks 1969: 2). He motivated this view on two grounds. One reason is "that as we go back in time the figures become so patchy" (Hicks 1969: 1). I will return to this later. But a "deeper reason" underlay Hicks' position: he pointed out that as we go back into the past, "we are bound to find […] that the economic aspects of life are less differentiated from other aspects than they are today" (*ibid.*). According to Hicks, economic history is the history of specialization, not only among economic sectors but "also a specialization of economic activities (what are becoming economic activities) from activities of other sorts".[3] This is a gradual and a never completely accomplished process which, in fact, made economics possible, as a field of study: "it has gone far enough for us to imitate it in our studies", Hicks observes (*ibid.*: 2). Economics, as an academic specialization, "corresponds to some-

[3] *Ibid*. In this vein, it is worth reading Robert Shiller on the historical evolution of the American housing market: "Before 1960 general public attention to the housing market often tended to take the form of outrage at the exorbitant rents that landlords were able to extract from their tenants, rather than concern about the course of prices of single-family homes. People were living in a less avowedly capitalist economy, and they were not primed to believe that their well-being depended in large measure on their property. Prior to the last decades of the twentieth century, public attention focused instead on rent control and on a housing cooperative movement, whereby groups of people would buy an interest in an apartment building that they controlled as a group. From these conspicuous examples of government and collective intervention in markets, people might plausibly have imagined that something would be done by authorities to prevent home prices from getting out of control" (Shiller 2015: 34). "Life was simpler once; one saved and then bought a home when the time was right. One expected to buy a home as part of normal living and didn't think to worry about what would happen to the price of homes. The increasingly large role of speculative markets for homes, as well as of other markets, has fundamentally changed our lives. The price activity that was once very local and confined to such events as the building of highways, canals, and railroads has become national and even international, and it is now connected to popular stories of new economic eras. The changing behavior of home prices is a sign of changing public impressions of the value of property, a heightening of attention to speculative price movements" (*ibid.*: 35).

thing which is in fact happening in the 'real world'" (*ibid.*). But we must be careful because what we study as economists "is not all that is happening in the world; we suffer, and we know that we suffer, by getting so far apart" (*ibid.*). And economic history can bring relief: one of its major functions, as Hicks sees it, "is to be a forum where economists and political scientists, lawyers, sociologists, and historians – historians of events and of ideas and of technologies – can meet and talk to one another" (*ibid.*).

This is what John Hicks thought of economic history in the years when Stefano was starting his cliometric enterprise. And this seems to be very close to what now is causing him *spleen*. I interpret Stefano's epistemological confessions as a regret (a sentiment which in fact tends to be associated with *spleen*): his European half was not sufficiently uninhibited at that time. It should have been allowed more power and more freedom. One may wonder whether such ideas as those of Hicks' were generally known to students, then.[4]

I fundamentally agree with Stefano's claim. I agree that cliometricians failed as economists, as historians and as economic historians. I will deal with the three failures one at a time, in the following three sections. But I am not so sure that cliometrics has failed, after all, as I will argue in the conclusions.

2. A Whig View of Economic Thought

Cliometricians' battle against traditional economic history was engaged for good reasons: providing economic history with analytical tools, adding discipline to the way scholars study the past. But they fought it the wrong way: they thought that a field of study, in order to be useful, needs to be scientific and to be scientific it needs to attain methodological autonomy. To this aim, the best strategy was deemed to embrace mainstream economics

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[4] After all, they were very also close to those of an economist and statistician as Simon Kuznets, one of the fathers of economic measurement: "If we are to understand modern economic growth – he wrote in his *Modern Economic Growth*, in 1966 –, we must measure its magnitude in terms of the modern system of ends, means, and values. Further, if we want to contrast modern economic growth with earlier periods and patterns of growth, we must evaluate and appraise the earlier periods also in modern terms in full knowledge that part of the difference would be due to the fact that the societies of the earlier times did not share many of the notions of means, ends, and values that constitute impulses to growth in modern times. This is not to argue against using different systems of notions for different economic epochs, if it is desirable. The argument here is only that if our interest is in modern economic growth, the basic notions of modern economic society as to ends, means, and values must be used even for comparison with earlier times" (Kuznets 1966: 23).

and to abandon interdisciplinarity. Hicks turned out to be somewhat optimistic: the cliometric turn induced at least its followers to abandon the interdisciplinary forum he had praised; most importantly, the new economic historians never acquired its humble intellectual openness.

Stefano regrets that he did not do as much as he could have to counter economists' isolation and self-referentiality, which led to provincialism, that is to American dominance in economics and, fundamentally, to a Whig interpretation of history and of the history of economics: "an interpretation ('palpably') designed to portray us, we modern Westerners, as the pinnacle of human accomplishment" and, we may add, designed to consider the "American" economics papers written in the last three years as the highest achievement in our "science" (7).

The provincialism of "American" economics – I would add – was leading our science to a more radical and damaging form of provincialism and isolation, the provincialism "of time" as T.S. Eliot (1944) had defined it:

> In our age, when men seem more than ever prone to confuse wisdom with knowledge, and knowledge with information, and to try to solve problems of life in terms of engineering, there is coming into existence a new kind of provincialism which perhaps deserves a new name. It is a provincialism, not of space, but of time; one for which history is merely the chronicle of human devices which have served their turn and been scrapped, one for which the world is the property solely of the living, a property in which the dead hold no shares. The menace of this kind of provincialism is, that we can all, all the peoples on the globe, be provincials together; and those who are not content to be provincials, can only become hermits (Eliot 1944: 30).

Provincialism of time is the main illness from which contemporary economics suffers; and cliometrics is a branch of economics which has given up economic historians' intellectual dowry. As a matter of fact, cliometricians' premise was right: economic history needed help from economics in order to overcome its serious methodological flaws. However, they disregarded the very fact that economics, in order to be useful, needed some help too: from history.

As Dani Rodrik (2015) more recently pointed out, "in economics, context is all. What is true of one setting need not be true of another".[5] This is

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[5] Rodrik (2015: 67). "Some markets are competitive; others, not. Some require second best analysis; others may not. Some political systems face time-inconsistent problems in monetary policy; others don't. And so on. It is not surprising to find – as with, say, privatization of state assets or import liberalization – that the responses of different societies to quite similar policy interventions often vary greatly. Savvy economists end up applying different models to make sense of divergent outcomes. This reliance on multiple models does not reflect the

a line of reasoning which we could have expected to stem from economic historians, from cliometricians.

Rodrik does not explicitly mention economic history, but the way he urges scholars to understand the role of context in economics is something which economic historians should have in their DNA. The main point raised by Rodrik is a point which all economic historians should be familiar with. It is well developed by Gilboa, Postlewaite, Samuelson and Schmeidler (2014) who draw a distinction between "rule-based" and "case-based" knowledge. The former approach is what traditionally tends to be regarded as scientific: it points to the identification of a law which is possibly corroborated by the data. In this case, models and data are accurately separated from each other and "observing counter-examples to the rule suggests that the rule has to be revised, or that its domain should be restricted" (Gilboa *et al.* 2014: F518). This is the typical approach taken by cliometricians, as one of its founding fathers, Robert Fogel, describes it:

> The cliometrician's model for proving his case or disproving an opponent's case is the empirical-scientific model. The strategy is to make explicit the implicit empirical assumptions on which many historical arguments rest and then to search for evidence, usually quantitative, capable of confirming or disconfirming the assumptions (Fogel 1983: 51).

On this background, Fogel (1983), in his interesting article comparing what he defines "scientific" and traditional history, identifies the two methodological approaches as follows:

> "Scientific" historians tend to focus on collectivities of people and recurring events, while traditional historians tend to focus on particular individuals and particular events. I do not mean to suggest either that "scientific" historians do not study particular events or that traditional historians do not study social and political movements. But when "scientific" historians study the stock market crash of 1929, the decision of the British Parliament to end slavery in its colonies, or the downfall of Louis XVI, they proceed on the assumption that these particular events were the outcome of processes that were governed by functional relationships containing both systematic and stochastic terms (Fogel 1983: 42).

So, "particular" events are interpreted by Fogel as non-systematic variation, i.e. variation in the stochastic term.

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inadequacy of our models; it reflects the contingency of social life. Knowledge accumulates in economics not vertically, with better models replacing worse ones, but horizontally, with newer models explaining aspects of social outcomes that were unaddressed earlier. Fresh models don't really replace older ones. They bring in a new dimension that may be more relevant in some settings" (*ibid.*).

Another way of making the same point is to say that traditional historians often concentrate on problems in which the influence of the stochastic terms are predominant. What is background for one group is the central concern of the other. Some scholars might be inclined to argue whether the systematic or the stochastic elements ought to be the principal focus of historical research.[6] The answer will surely vary from case to case and will in part depend on which aspect has been most fully explored by previous scholars (Fogel 1983: 43-44).

The rule-based methodology can be contrasted with the case-based one. The case-based methodology is based on reasoning by analogy. It draws on other cases that present similarities: against this background the distinction between a deterministic, collective or average, behavior and a stochastic component attributed to individual agents or to particular events does not appear very sound. "When the relevant data cannot be forced into succinct rules without sacrificing too much relevance, the case-based approach becomes particularly useful" (Rodrik 2015: 72). According to Gilboa *et al.* (2014), the knowledge generated by economists is to a great extent case-based: "instead of offering general rules or theories that should be contrasted with data, economists often analyse models that are 'theoretical cases', which help understand economic problems by drawing analogies between the model and the problem".[7]

The point here is that "in this perspective, economic science advances by expanding its collection of useful cases" (Rodrik 2015: 72). And it is easy to see that history can help economics in its task by offering cases to be framed with economic logic; and in turn advances in economics, that is expanding its collection of cases (and models), help interpret historical phenomena, and allow a debate in which "sources of disagreement" may be rationally and constructively identified. This is true also when it is not possible to conduct a rigorous empirical verification, which is the typical condition in which economic historians find themselves.

Two implications of the line of reasoning summarized in Rodrik (2015) are worth spelling out. One is simple and immediate: the history of eco-

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[6] But specificity may regard the structure or even the behavioral pattern, as in Shiller (2015), see footnote 4.

[7] Gilboa *et al.* (2014: F515). The authors put forward an interesting example referring to the well known Akerlof's "market for lemons" paper: "As stated, the example can be viewed as the claim, 'I have observed a case in which idealised agents, maximizing expected utility, with the following utility functions and the following information structure, behaved in such and such a way'. The relevance of this observation for prediction will depend on the perceived similarity between the idealised agents and the real agents one is concerned with, the similarity between the situation of the former and that of the latter, and so forth. An economist who is interested in real agents would therefore have to judge to what extent the situation he studies resembles the idealised situation in the 'case' reported by Akerlof" (Gilboa *et al.* 2014: F518).

nomic thought should be taught systematically. Unfortunately, as Stefano recalls in his paper, that "is not part of the core curriculum, it is mere antiquarianism as useless to an economist as a study of Ptolemaic epicycles to an astronomer" (7). The other implication is more complex. It refers to postmodernism which Stefano seems to interpret as the more advanced counterpart to American positivism, in which cliometricians framed themselves. I agree with Stefano when he writes that cliometricians' view of their "'science' is that of nineteenth-century positivists, blithely confident that we can observe reality, establish "'the facts'" (8). I also agree that "Western culture, led by French literary criticism and philosophy, has meanwhile moved beyond that, to postmodernism" (*ibid*.). On this basis, Stefano seems to contrast American culture, as the conservative bearer of nineteenth-century positivism, with the European, or more strictly French, culture which offers a more progressive and realistic view of science. My view here is that the provincialism of American economists and cliometricians (even time provincialism, to recall T.S. Eliot's warning) is not strictly attributable to the broad American culture which hypothetically conditioned American cliometricians. Quite to the contrary, to a great extent, they bear responsibility for their intellectual choices.

America's cultural and philosophical traditions offered solid and deep epistemological views to inform the cliometric adventure and allow it to escape from the provincial positivistic cage its practitioners chose to close it in. Pragmatists such as Charles Peirce or John Dewey would have lent them the right perspective to think economic history in a new way, rejecting both naïve positivism and postmodern skepticism. It would be interesting to understand why they chose to take such a conservative road. I will not attempt to do so here. I just want to emphasize that an economist like Rodrik, who clearly rejects both positivism and the Whig conception of the history of economic thought, is not a postmodernist. In fact, he calls himself a pragmatist (Rodrik 2015: 81). But it is also interesting to recall that many of the epistemological positions of an economist like Luigi Einaudi, to whom Stefano rightly pays tribute in the paper, may be considered of a pragmatist orientation. Einaudi was a good friend of Giovanni Vailati, a mathematician, physicist and philosopher whose thought was very close to that of the American pragmatists.[8]

Pragmatism is also important to our understanding of our relation with historical sources. It teaches us that knowledge is possible; we are not compelled to stay in the skepticism versus positivism straitjacket, as Ste-

[8] In Baffigi (2010), I studied some aspects of the influence of Vailati's pragmatism upon Einaudi. For a short discussion of Vailati's economic thought, see Baffigi (2019).

fano certainly agrees. This is important also when we turn to the issues of data and measurement, as we shall see in the next Section.

3. A Positivist View of Empirical Evidence

An important consequence of the rule-based approach – the one typically adopted by cliometricians – is that it tends rigidly to separate facts from interpretations, in the typical nineteenth century positivist tradition which considers a fact as "a datum of experience as distinct from conclusions" (Carr 1961: 6). The nineteenth century was "a great age for facts", the historian Edward Carr points out (Carr 1961: 5). He recalls Leopold von Ranke, the famous positivist historian who is still well known for remarking that the task of the historian is "simply to show how it really was (*wie es eigentlich gewesen*)". Ranke's approach influenced generations of historians and, not surprisingly, his authority stretched even to the cliometric school. Interestingly, Robert Fogel puts Ranke in his Pantheon. He claims that

> cliometricians conform to Ranke's admonition that historians should devote themselves to the task of determining what actually happened. Just as the nineteenth- and early-twentieth-century followers of Ranke scoured the public archives for diplomatic and ministerial documents that would reveal what actually happened in government policy, so cliometricians have been scouring archives anew, this time searching for quantitative evidence bearing on what actually happened in social behavior (Fogel 1983: 28).

"*Wie es eigentlich gewesen*": Stefano's discussion of cliometricians' failure as historians starts out from this very phrase. And he observes that "the evolution of our culture has destroyed our comforting faith in the attainability of that goal" (11). In fact, this should have encouraged cliometricians to deal with the complex relation between sources and facts. But cliometricians have behaved as economists, who want ready-made data. Here too cliometricians prove to be lagging behind the evolution of Western culture, as Stefano points out. We could add that cliometricians overlooked the patchy nature of historical data, which according to Hicks would contribute to economic historians' reluctance to pursue quantitative analysis. And, for this reason, they failed to develop methods to cope with such patchiness.

Carlo Ginzburg, an Italian historian, provides very deep insights on the relation between sources and facts. To be sure, his field is modern history, but his methodological discussions should be also proposed in economic history classes to promote methodological awareness. Ginzburg is a staunch critic of postmodernism, of its skeptical view, of which he stresses

the serious cognitive and moral limitations. But, at the same time, he considers rudimentary and misleading the positivist idea of sources as open windows to reality.

> Sources are neither open windows, as the positivists believe, nor fences obstructing vision, as the skeptics hold: if anything, we can compare them to distorting mirrors. The analysis of the specific distortion of every specific source implies a constructive element. But construction […] is not incompatible with proof; the projection of desire, without which there is no research, is not incompatible with the refutations inflicted by the principle of reality. Knowledge (even historical knowledge) is possible (Ginzburg 1999: 25).

In many respects, quantitative economic historians have to deal with very similar methodological problems. Indeed, when interpreting historical sources to extract figures to be used for data reconstruction, we cannot see our work as simply digging out hidden information. As stressed by Stefano,

> our quantitative "data," like the historians' documents, are constructs that must be deconstructed if their relationship to "the facts" is to be understood at all. We must determine by whom, to what purpose, and how they were derived, we must scrutinize them closely for clues to inconsistency, evaluate them in the light of ancillary evidence and indeed of everything we know: we must live them and breathe them, to discover the hidden defects that surface only with extended cohabitation (13).

But let us stay for a while longer in the company of Ginzburg's methodological reflections. In 1984 he wrote the postscript to Natalie Zemon Davis (1984), *The Return of Martin Guerre*; in her book, Davis reconstructs the story of a trial which took place in France, in the sixteenth century.[9] It is the story of a famous identity fraud, which has been dramatized and studied by many artists and scholars. Leonardo Sciascia's *La sentenza memorabile* is an enjoyable report of what is known about it. It is an interesting case which, if properly documented, could shed some light on the social conditions of the lowest social classes in France, in the sixteenth century. Here, it is not important to tell the story. I just want to quote a sentence from Davis' book which Ginzburg brings to our attention, for its methodological implications:

> When I could not find my individual man or woman…then I did my best through sources from the period and place to discover the world they would have

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[9] By the way, Davis is American.

seen and the reactions they might have had. What I offer you here is in part my invention, but held tightly in check by the voices of the past.[10]

What I want to argue is that, in many respects, Davis' use of sources is very close to the one suggested by Stefano. In all kinds of historical sources, be they economic or judicial, there are holes which – with good theory, experience and wise craftsmanship – can be interpreted and mended, to obtain restored stories or data. Indeed, it would be interesting to compare how Natalie and Stefano both use ancillary evidence in such different fields of historical investigation.

Sadly enough, however, as Stefano stresses, among cliometricians such complex work, largely based on experience, is very often left to young researchers, who instead could more usefully and more efficiently do more codified work, while becoming aware of the complexity of sources directly from experience and from the teaching of the elderly; and also by interacting with scholars of different fields, in order to understand methodological differences and affinities in their respective disciplines. Students in economic history should become familiar with scholars from other fields, with historians of politics and society, besides economists and econometricians, of course.

My quotations from Ginzburg and Davis in this comment to Stefano's paper are no more than a *divertissement*, but they allow me to show that we should go beyond our little worlds. Such interactions and comparisons between methodologies should be developed systematically. They would benefit students in economic history but also in economics and in political and social history, I would say. The interdisciplinary forum, with which John Hicks identified economic history, should be revived.

4. Can Historians Help us Understand GDP?

Hence, a reconstructed historical time series should be considered as a historical interpretation itself, not differently from a biography written on the basis of archival documents and of oral testimonies. Reconstructing data from sources is work very similar to the reconstruction of the story behind a trial, like the one studied by Natalie Zemon Davis, at least if we don't want to behave as "economists who deal with yesterday's numbers rather than today's" (11). The relation between historical data and the underlying sources is a very important issue, in particular, for "our measure of the economy as a whole" (15), that is, GDP.

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[10] Quoted in Ginzburg (2012: 57).

The main point raised by Stefano here, I think, can be summarized as follows: both as historians and as economists, we know that GDP is not really a measure of something which we can observe or identify before we measure it. At least we should know that. GDP is not a measure of the economy. Rather it is a definition of what we assume the economy is. As Diane Coyle (2014) explains in her beautiful book *GDP. A Brief but Affectionate History*, "there is no such entity as GDP out there in the real world waiting to be measured by economists. It's an abstract idea".[11]

Based on such considerations, in my comment here I want to show two implications deriving from considering GDP as a theoretical, social and political construct, not exactly a measure. The first implication is operational; the second may be defined as political.

The operational implication is that in order to reconstruct historical national accounts a preliminary assessment should be made to choose the national account system – i.e., classifications and definitions – which best suits the economy over the period under study. In the Bank of Italy-Istat reconstruction of Italy's Historical National Accounts (Baffigi 2013 and 2015) we followed Guido Rey's reflections on this issue (Rey 2002): we chose the Italian (Istat) version of SNA 1953; among those available (all of them more recently issued), it was convincingly considered as the most suitable to provide a methodological "compromise" to describe the economy over a long period, during which it underwent great transformations, as that stretching from Unification (1861) to the second post-war period. In particular, the SNA 1953 system reflected the structure of market economies in which the development of the tertiary sector had not yet materialized, especially in the financial sphere: it mainly focused on the real part of the economy. The idea is that the yardstick we choose to measure an economy cannot be independent of its structure and of its degree of development.

Economic historians should look inside the yardstick they use. And this brings us to the second, political, implication. It is related to the point made by Stefano with his story explaining why US GDP per capita was higher than that of any other country, the real reason being that the measure was invented in the USA (15). It is not a joke, actually, and it is still an issue today. An interesting analysis, along these lines, for example is the one recently proposed by Assa and Kvangraven (2018). The two authors claim that "changes made to GDP measurement over the past two decades have a bias towards countries traditionally in 'the West'" (Assa and Kvangraven 2018). They point out that

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[11] Coyle (2014: 24; see also 144). For a deeply documented study, with rich archival research, on the evolution of national accounts systems see Mitra-Kahn (2011).

countries that developed based on the development of manufacturing but that are now shifting into services and other economic activities, redefine the key measure of development – GDP – to favor their new areas of specialization and move ahead of developing countries which are arguably taking over the manufacturing mantle.[12]

Such changes in measurement criteria have important political consequences as long as a construct like GDP impinges on international comparisons and, through that, on the hierarchy of countries in the international arena.[13] Historians cannot influence the current National Accounts' political economy, but can allow for it when interpreting economic history.

5. Conclusions. Really a Failure?

I agree with Stefano. On all three facets of cliometricians' activity – economics, history, and economic history – we can spot the sign of a failure. But can we conclude that the cliometric school failed to reach the broad objectives that were set out in the early years of the discipline? Did it fail to provide economic history with a more solid scientific status? Arguably, cliometrics showed the right way for scholars to go: economic history needs economic theory to make progress, to reach a deeper understanding of the very object of its studies; moreover, in order to apply economic theory to history we need data. An historian like François Furet in 1971, discussing the perspectives of the recently born quantitative history (Furet 1971), stressed one central innovation of the new approach: "*la substitution de la série à l'événement*" (67). Documents do not exist for themselves, Furet observed. Not anymore. We have to interpret them taking into account their relative position within a time series, i.e., in the light of the data which precede and those which follow, also by using probabi-

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[12] Assa and Kvangraven (2018: 2). "Many economic activities – financial intermediation, owner-occupied housing, research and development and the production of weapons – were previously excluded from GDP as either non-productive or as constituting productive inputs to other outputs (hence deducted as intermediate consumption). The inclusion of these economic sectors in the production boundary since 1993 and 2008 has added disproportionally to the GDP of developed countries, which have in recent decades specialized in these activities and moved away from traditional pillars of development such as manufacturing and infrastructure-related services" (Assa and Kvangraven 2018: 5).

[13] For instance "an economy's size relative to world GDP helps determine the country's voting rights in international organizations such as the World Bank and IMF, and its level of per capita GDP determines its eligibility for concessional foreign aid" (Assa and Kvangraven 2018: 2).

listic analysis. They are not in relation to an elusive "real" substance.[14] In fact, this is the core of the cliometric revolution. Furet defined quantitative history as «*une révolution de la conscience historiographique*» (*ibid*.: 71) which, besides referring to data time series, makes historians pass from an "implicit" to an "explicit" logical framework, or model, with which they read their sources.

Today, we can say that even Furet was overly optimistic, though – we have to recognize – he used many caveats in expressing his views. If we put together all the threads which somewhat chaotically intersect in the previous sections, I think we can say that it was a mistake to call *l'histoire quantitative* a revolution, to interpret it as a sudden and radical change which sweeps away old ideas and practices. As a matter of fact, cliometricians believed they were revolutionary and they behaved consistently. But they were wrong. More modestly and more importantly, cliometrics should have been regarded as a remarkable basis for a fruitful and long-lasting reform. It could have found its way by adding new ideas and new practices, pragmatically, by being aware of its limitations and of the need to understand how and when old practices are still necessary. It didn't happen, and that significantly damaged cliometrics, limited its potential success in understanding the past.

For this reason, now we can speak of failures for the standard cliometric school on two grounds: because of their Whiggish and positivist interpretation of economic thought and because of their usually naïve interpretation of the process of transformation of historical sources into what we usually call data.

Context is all in history but, as we have argued, it is all in economics, too (Rodrik 2015: 67). Economic reasoning proceeds by analogy, that is by referring to cases deemed similar to the one under scrutiny. This is quite different from the strategy suggested by a cliometrician like Robert Fogel. Pragmatism should accompany our research. Economics is mainly a case-based discipline. Cases are made up of a mix of empirical evidence and of economic logic. Economic models do not mirror reality, rather they imitate it, under some conditions which good users should recognize if they

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[14] "Le document, la donnée, n'existent plus pour eux-mêmes, mais par rapport à la série qui les précède et les suit; c'est leur valeur relative qui devient objective et non leur rapport à une insaisissable substance 'réelle'. Ainsi se trouve déplacé, du même coup, le vieux problème de la 'critique' du document historique. La critique 'externe' ne s'établit plus à partir d'une crédibilité fondée sur la comparaison avec des textes contemporains d'une autre nature, mais à partir d'une cohérence avec un texte de la même nature situé différemment dans la série temporelle, c'est-à-dire avant ou après. La critique 'interne' s'en trouve d'autant plus simplifiée que beaucoup d'opérations de 'nettoyage' des données peuvent être mises en mémoire d'ordinateur" (Furet 1971: 67).

want to make fruitful use of them. Not recognizing such an important characteristic of economics may lead us astray, out of the scientific realm. Models allow us to ask well founded questions and to look for sensible answers; they allow us to check for logical consistency and to know what kind of empirical evidence we need to support our interpretation. They may offer detailed indications to clarify the specific points on which the participants in a debate disagree.

In this perspective, Stefano's cliometrics does not fit the standards of that discipline if we believe them to be those claimed by Fogel. Take his interpretation of Italy's industrialization (Fenoaltea 2011): he proposes a logical frame which comprises indications about the sources which should be used to underpin the proposed reasoning and show us the way he transformed them into data. To be sure, one can criticize his logical frame, possibly on consistency grounds or by claiming that more, or different, data are needed. One can come up with new sources or a different interpretation of the old ones, and so on. Stefano himself has repeatedly extended and revised his estimates and interpretations. In his cliometric approach, interpretation, data and historical sources form a continuum, quite differently from Fogel's methodology.

And what about GDP? Was Stefano's research useful to improve our knowledge of the crucial post-Unification five decades? I think that everybody agrees that it was, even though, now, I suspect that refinements of GDP estimates for the period 1861-1913 have run into diminishing return. A Fenoalteian GDP profile is almost generally accepted by scholars, and this is a success for cliometrics. The disputes, if anything, are almost totally about its interpretation. To be sure, Stefano would reply that now we need more, better and deeper disaggregation in order to reap its "fruits". This is one of the objectives of his most recent works (Fenoaltea 2017).

My general conclusion is that cliometrics, broadly understood, has not failed, after all. There is not just one way, the "provincial" and whiggish standard way, to be a cliometrician. Cliometrics for Italy's history is an example. There are scholars at work in fields which in fact belong to this discipline. They have produced very important results, a better knowledge of our history, and have provided a methodological example potentially useful also in other contexts. And if we connect the lines of that *paciente laberinto*, Stefano's face is there.

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