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**No. 14**

DECEMBER 2018

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by

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**HISTORICAL HOUSEHOLD BUDGETS**  
**WORKING PAPER SERIES**

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*Please cite this paper as:*

Fenoaltea, S. (2018). 'Spleen: the failures of the cliometric school', *HHB Working Paper Series*, No. 14, December 2018, Rome.

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HHB Project  
Via Columbia, 2  
00133 Rome  
Italy  
<http://hhbproject.com/>

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# Spleen: the failures of the cliometric school

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December 2018

## Abstract

This paper argues that we cliometricians have failed as economists, because we did not drag the profession out of the nineteenth century and into the twentieth; that we have failed as historians, because we do not take measurement seriously, and misapprehend “the data”; and that we failed signally as economic historians, because we backcast “GDP” as if it measured gross domestic product.

**Keywords:** economics; economic history; cliometrics.

**JEL classification:** A10, B40, N01.

\* The author thanks Pierluigi Ciocca, Riccardo De Bonis, Giacomo Gabbuti, Alfredo Gigliobianco, Aurora Iannello, Gianni Toniolo, and Giovanni Vecchi for their at times extended comments. The usual disclaimer applies.

... l'Espoir,  
Vaincu, pleure, et l'Angoisse atroce, despotique,  
Sur mon crâne incliné plante son drapeau noir.  
Charles Baudelaire, *Spleen*

As we all know, “cliometrics” is the originally humorous name applied to what was also called the New Economic History. Humorous, and inexact: quantification was a mere servant, the mistress of the household was economic theory. That recovery of economic theory was of course well overdue, as mainstream economic history was embarrassingly ignorant of it: the throwaway distinction between “economic historians” and “competent economists” by Lionel Robbins (1939, p. 9) was harsh but not unjustified.<sup>1</sup>

So it was as economists, proud of our training, that we cliometricians fought our battles, against the “old” economic historians, against Polanyi and his epigoni. The 1978 *Journal of Economic History* included a piece by (then) Donald McCloskey on “The Achievements of the Cliometric School”: a triumphalist piece, well displaying the pride and confidence we felt at the time.

At the time, as *juvenes*. The present pages, penned four decades on, are a counterpoint to those. These are *scripta senectutis*, but not, I trust, another threnody, another *complainte* that the discipline is no longer what it used to be, another confession, in fact, that as the discipline evolved the author was left behind. Nor are they yet another tiresome outsider’s attack on the principles that define the cliometric approach: I confirm my faith in them, and would never deny that we, as a school, have contributed much and have much more to contribute. But to my mind our collective practice has fallen short of our principles, and to a church door I nail these theses: that we cliometricians have failed as economists, that we have failed as historians, that we have failed as economic historians.

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<sup>1</sup> The “New Economic History” traces its roots to the Meyer and Conrad (1958) paper on slavery. That was the voyage of Columbus: well earlier Luigi Einaudi (1936a, p. 158) had insisted that economic history should use the most up-to-date tools provided by economic theory, but his example was not followed, and it remained Leif Erikson’s voyage to Vinland. The “new” economic historians knew nothing of Einaudi: Schumpeter, who did, had died years earlier, and Gerschenkron was a poor substitute (much given to advertising his European culture but something of a bluff, and taken aback when I pointed out that the Einaudi he cited was actually Luigi and not Giulio; see Gerschenkron 1962, pp. 84-85).

I now believe, in sorrow and in anger, that we fought the wrong battles: not because the “old” economic history and the Polanyi approach were not to be condemned, but because they were doomed in any case by their own obvious deficiencies; because in fighting the enemy without we overlooked the more insidious enemy within, the inherited weaknesses in our own intellectual baggage. 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.

## 1 Our failure as economists

We contemporary economists are proud of our discipline, our “queen of the social sciences,” our economic *science*. We practice it as scientists: we favor powerful, parsimonious models, we use mathematics, we test our hypotheses against empirical evidence, against “the facts.”<sup>2</sup> We produce it as scientists: we publish peer-reviewed articles, we embrace bibliometric scores. We teach it as a science: the history of economic thought 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 or of Hippocratic humors to a physician. Leading economists have been heard to brag that they have their students read nothing over three years old; in the early 1960s I myself heard Paul Samuelson declare that “any graduate student today is a better economist than Keynes.”<sup>3</sup>

My layman’s take on this is the following. One, we humans seek status, if only because “in the environment of our evolution” higher status meant greater reproductive success (Wright 1994): economists consider themselves scientists, and want to be seen as

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<sup>2</sup> Thus the canon, but it is hard to see what is peculiarly “scientific” about it. The *human* mind, every last one of us, favors powerful, parsimonious models: think of the fellow who discards the complex set of hypotheses that supposedly explain his wife’s repeated tardiness in returning home from work in favor of the single, powerful hypothesis that she is seeing somebody else. Mathematics are a wonderful tool, an inherently error-proof way to draw out the implications of one’s assumptions, as effort-saving as power steering. The testing of hypotheses that “as scientists” we can reject or not reject, but never “accept,” is instead so much cant: we do not consider Archimedes’ principle a “not (yet) rejected hypothesis,” we accept it as fact, a law of nature (and we write to have our hypotheses *accepted* by the reader, McCloskey 1983). Many of our facts are actually no more than beliefs, but more on this anon.

<sup>3</sup> Jean-Christian Lambelet was also present (at a graduate class at Harvard ca. 1964, where Samuelson appeared as a guest lecturer), and confirms the episode. I remember it clearly because I was appalled by his sense of the discipline: to my mind he might as well have said “any art student today is a better painter than Raphael,” presumably because we now have acrylic paints.

scientists, for the status, the prestige that label confers. Two, with her self-inflicted World Wars Europe lost her economic and military hegemony, and in good part her cultural hegemony too, which passed to the United States (and a Good Thing too, given the alternatives Europe offered up by the 1930s); economics in particular has become overwhelmingly American, and Europe's doctoral programs now mimic American models. Three, the attribution of the highest prestige to *science* is a peculiarly American trait, only America could spawn a Church of Scientology. This no doubt reflects a pioneering society's natural concern for the practical; methinks it also specifically reflects modern America's (consequent?) abandonment of classical education. In Europe, "in the environment of my own evolution," the bright kids did classics, the scientific curriculum was for second-raters. And in Western culture at large the greatest of the greats, those known by their first name alone, are (hairdressers aside) not scientists but *artists*: we remember *Raphael*, we remember *Michelangelo* (but the less eminent *Vasari*), we do not remember *Enrico*, or *Albert*, or *Marie*.<sup>4</sup> In short, economics now presents itself as a science because it has become American: the discipline has internalized values that are distinctly American and, on a broader stage, distinctly provincial.<sup>5</sup>

But America is distinctive, and provincial, in other ways as well, only America could spawn a Joe McCarthy. Only Americans *approve* of capitalism, everywhere else it is a dirty word: no European with even a folk memory of history, no European who has read Zola can do more than *accept* capitalism reluctantly as the least bad practical alternative, can view the failure of more ambitious schemes as anything but tragedies for the human race. Successful European leaders sold communism, socialism, *national* socialism, corporativism; *capitalism*, as such, had no mass constituency at all.<sup>6</sup> In Europe, Marxism is respected, if only for the nobility of its aspirations; in America it is

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<sup>4</sup> My own take, as the reader will have gathered, is that to consider me a scientist diminishes me: I cannot be considered an artist, but want to be considered at least a craftsman, with technical skills but also a creative, aesthetic dimension that makes me unique and not fungible. And once we recognize the aesthetic dimension of our "scientific" creations, we see invasive peer review for the aberration that it is: "We will hang your *Mona Lisa* in our museum, Mr. da Vinci, but only if you correct her mouth so that she bares her teeth as she smiles." *O tempora! O mores!*

<sup>5</sup> As those who know me know, I am at once, and in roughly equal parts, both American and Italian: all my criticism, whithersoever directed, is in principle self-criticism (unless, Italian-born, I simply inherited the House of Savoy's practice of choosing its allegiances from case to case and moment to moment, ever ready to rat, and re-rat, as circumstances warrant).

<sup>6</sup> The aforementioned Luigi Einaudi was a classical liberal; but he was in favor of high inheritance taxes (confiscatory over very few generations, in the absence of further, successful enterprise). If capitalism is, as Schumpeter defined it, the civilization of the family fortune, even Einaudi was against capitalism.

anathema. American economists are not exposed to the Marxist literature, they never see themselves called *bourgeois* economists, never hear the warning that they are sectarian, imprisoned by their beliefs, more nearly religious thinkers than scientists; even less do they grasp how much they play into the Marxists' hand as they proscribe their writings, as they avoid reading heresy to maintain the purity of their faith. Only in America could economists *want* to be considered scientists, only in America could they stop their ears with wax and not hear anything to the contrary; only we American economists, we Americanized economists, could be so provincial.

We economists are not only provincials, we are primitives. Our view of our “science” is that of nineteenth-century positivists, blithely confident that we can observe reality, establish “the facts.” Western culture, led by French literary criticism and philosophy, has meanwhile moved beyond that, to postmodernism. By now we all know – all of us, save only those entirely cut off from our broader culture – that our vision of reality is inevitably distorted by our biases and prejudices, that we see our facts through the prism of our theories; the more radical among us doubt the very existence of the objective reality an economist might wish to observe, an historian to reconstruct. We know that our social “sciences” are not the cumulation of objective knowledge, but the contemporary form of the stories our distant forefathers would tell when they gathered around the campfire.<sup>7</sup> Our theories, our facts – our stories, like their stories – 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 (Fenoaltea 2011, p. xix).<sup>8</sup>

The most obvious case in point is our history of the human race. In the nineteenth century England and the Western World experienced mass progress, that had been all but inconceivable. Progress became the new religion: in Its name, as formerly in that of the True Faith, the West justified colonial conquest and the new imperialism. That same

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<sup>7</sup> Our *forefathers*, our *male* ancestors. My sense is that history was always *male* history not because of our dominance, but to compensate for our weakness. The female, the ovum, the queen bee are rare and precious, the key to the survival of the species; males, sperm cells, drones are vastly superabundant, as worthless (at the margin) as any superabundant resource. Only we males felt the need to invent stories to glorify ourselves, to give a meaning to our essentially pointless existence; to be female was glory enough (again, e.g., Wright 1994).

<sup>8</sup> The closer a subject is to our hearts, the less we can view it objectively: to an informed mind a “social science” is an oxymoron, a contradiction in terms. The same problem once plagued the natural sciences: we will never know whether Copernicus was the first with the genius to see the explanatory power of the heliocentric model, or merely the first to combine that genius with a misanthropic willingness to demote mankind from its rightful place at the center of God’s creation.

faith defined Man as the tool-maker (not the picture-painter, or story-teller, or god-worshiper, or clothes-wearer, or anything else, equally distinctive); it defined the past as the history of progress, specifically of technical progress, of that particular progress that was the pride and miracle of the West. The triumph of that ideology has been complete: we absorb it in our grade-school texts, we have no mental categories to describe the vast sweep of human history other than the stages of technological progress, from the “stone age” on. But this is only *an* interpretation of history, the Whig interpretation of history, an interpretation (“palpably”) designed to portray us, we modern Westerners, as the pinnacle of human accomplishment.<sup>9</sup>

Anthropology has kept pace with Western culture – in America too, thanks perhaps to the independent strength of the discipline in France: anthropologists realized decades ago that “progress” and “the primitive” are notions we modern Westerners invented to glorify ourselves, and spent a suitable number of years deconstructing their discipline.<sup>10</sup> Economics has instead been left behind, in splendid isolation; isolation is the royal road to inbreeding, inbreeding the royal road to retardation. That isolation again reflects the American dominance of the discipline, the lack of the European counterweight that may have rescued anthropology; it also reflects, methinks, the structure of American higher education. In small colleges the relatively few economists perforce rub shoulders with colleagues in other disciplines, and develop, in my experience, interestingly complex minds. But the tone and nature of the discipline are set by the stars of the profession in the leading research universities, the “multiversities,” as that barbarous neologism has them.<sup>11</sup> There economists are numerous, Economics Departments typically have their

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<sup>9</sup> On the Whig interpretation, now undermined, see Fenoaltea (2006). To Whig eyes everything that brought humanity closer to Us was progress. We have agriculture, its invention was progress; we have a Church and a State, their creation meant progress; we have money and markets, their development came with progress; our many Revolutions, political and economic, were all steps in the path of progress. To me the eye-opener was, perhaps paradoxically, Polanyi (1944). I grew up an orthodox Whig – Fisher (1936) was long my *livre de chevet* – and on a first reading, in college, *The Great Transformation* merely puzzled me. Some years later a dim bulb sent me back to it, and I grasped its deeper import: simply by *not* seeing the “capitalist revolution” as progress Polanyi showed that the contrary view was not fact but (an alternative, also ideologically-charged) interpretation.

<sup>10</sup> I witnessed that at first hand at the Institute for Advanced Study, in 1987-88. A concentration of economic historians had been invited the previous year (omitting me, to my humiliation). When I was there I was the lone economic historian; the mass of annual visitors was made up of anthropologists, and theirs were the seminars I attended. All were introspective, a reflection on the nature of the discipline; and I believe I benefited far more from that crash course in deconstruction than I could have from a year with my fellow cliometricians.

<sup>11</sup> In Italy, universities are called the *università degli studi* of this or that city: not a pleonasm, but a recognition that when our universities were born “university” meant association, guild, that the university

own buildings; our leading economists can spend their entire working lives never talking to anyone who is not in fact a carbon copy of themselves. We economists are self-referential, we set our own fashions, we continue to wear our narrow ties when all around us have abandoned theirs. Push the metaphor, think of the world's *intelligentsia* meeting as Ascot race-goers in the 1930s: magnificently elegant, the epitome of sophistication. And there in a corner of the grandstand we find the economists, a rowdy group in coveralls, engaged in our own contest, the spitting of watermelon seeds. How do we appear, if not as yokels? How could we avoid the broader community's bemusement, horror, and withering contempt? To be an economist today, in a company that is not restricted to our fellow economists, is to suffer embarrassment.

A counterfact comes readily to mind. 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, joined them in spitting watermelon seeds; 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: economists without an independent contribution to the field, necessary perhaps to staff some undergraduate courses, but of very low priority in filling senior positions.

To our shame and cost we cliometricians have totally neglected the most important contribution we could have made to economics: we have failed as economists.

## 2 Our failure as historians

Historians who were not consciously apologists aimed no doubt to portray the past *wie es eigentlich gewesen*. The evolution of our culture has destroyed our comforting faith in the attainability of that goal; but that loss should affect our scholarly self-esteem

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of those who studied set itself apart from the universities of those who baked bread, butchered animals, whatever.

more than our scholarly practice, we should still reconstruct the facts to the best of our ability, however conscious we may be of the epistemological limits to that ability. But as a profession, we cliometricians do not do so at all: as a profession, astonishingly, incredibly, we hold the past in contempt, and care little for the facts. As a profession, our emphasis is all on the sophisticated *manipulation* of the data, not at all on the quality of the data themselves: we pride ourselves on, and reward each other for, the cut of our silk purses, whether the material is thread of *bombyx mori* or sow's ear does not concern us at all.

We are, in this too, economists. That economists hold the reconstruction of the facts – *measurement*, in our naturally quantitative discipline – in low esteem has been noted, and eloquently lamented, by Richard Easterlin (2004); and his complaint is supported by overwhelming evidence. The coin in which we reward the research we value is publication; in my own experience almost every article that sought to establish “the facts” by *proper* measurement – typically pointing out the methodological errors of common practice, and how these distorted “the data” we use – was rejected by the flagship Anglo-American and pan-European journals to which I submitted them, with suitable expressions of disinterest.<sup>12</sup> One referee advised the author to “put his series on his web-site”: as if measurement were idiots' work that doesn't merit journal space, or even peer review.

The prevalence of that view is confirmed by further evidence, anecdotal and general. Decades ago, as newly minted economics Ph.D.s, Joe Reid and I were both involved in the University of Pennsylvania's doctoral program in economic history. Many students were first-rate, and went on to distinguished careers; but others were not. Joe and I, with the unthinking cruelty of youth, frequently advised those who seemed incapable of grasping basic theory to leave the program, which they could not possibly complete; the standard response was “I'll do a measurement thesis,” as in “even granting for the sake of argument that I am an idiot, I can earn my doctorate doing idiots' work.” Easterlin

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<sup>12</sup> I refer here to sector-specific reconstructions (and not to the comprehensive “national accounts,” discussed below). Fenoaltea (1982), in the *Journal of Economic History*, is an exception that proves the rule: if memory serves McCloskey had just inherited the *Journal* from the previous editor, who proudly told him he had worked off the queue of manuscripts in process, and left him scrambling for papers. The crisis over, he too rejected such material (with his customary grace, but that is beside the point).

himself played in that program a far more senior, influential role than we; but that was the prevailing attitude, all the same.<sup>13</sup>

A decade or so later, the National Science Foundation refused to renew a generous grant that allowed me to pursue my research in Italy; the referee's complaint was that it was pointlessly expensive for me to "collect the data" myself, as a mere research assistant could as readily do it in my stead. That referee was a representative economist. Weaned on pap and raised on pabulum, the pabulum of ready-made, government-supplied statistics, economists believe historical data are simply to be found, photocopied, and uploaded, that measurement requires no more than the most elementary human skills. It was not always so – more on that later – but it has been so now for generations: most serious efforts to reconstruct the past, to *measure*, are *first* efforts, one-offs, typically their authors' doctoral dissertation (after which they turn to more publishable work); certainly in all research projects the P.I.s reserve the analysis unto themselves, and leave the preparation of "the data" to their r.a.s. Measurement, the provision of "the data" for the subsequent analyses, is (overwhelmingly) the work of unskilled beginners: because it is considered work suitable for the unskilled, in the vernacular for idiots.

Nothing could be further from the truth. Historians know that their sources are complex constructs, opaque to the inexpert, often meretricious, that one learns to extract their factual content only slowly, through long exposure. The quantitative sources we cliometricians use are no different, save that they are utterly treacherous: they present numbers clearly labelled in our native language, they positively *invite* us to take them at face value. And so we do, at our peril: to take an example from my own field of study, there is a long literature that takes Italy's industrial employment in 1911 directly from that year's industrial census, never cottoning to its gross incompleteness (Fenoaltea 2015, 2016, and references therein). 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

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<sup>13</sup> An eye-witness account of our misdeeds can be obtained for example from Elyce Rotella ([rotella@indiana.edu](mailto:rotella@indiana.edu)).

and breathe them, to discover the hidden defects that surface only with extended cohabitation.<sup>14</sup>

The historical data must be vetted, the Italian sources on which I have worked provide myriad examples, of diverse origin and content. The ore mined at Agordo was counted first as sulphur ore, later as iron pyrite; silk production was so badly underestimated that reported output didn't even cover net exports; the exceptionally low expenditure on public works in 1871 was a fiction due to an accounting change; the zero office space attributed to Ferrara in the 1911 room census reveals that the counted rooms were only those in residential buildings (Fenoaltea 1988a, p. 120; 1988b, p. 278; 1986, p. 7; 2017a, p. 36); and so on and on.<sup>15</sup>

Add to this that all too frequently the desired data do not exist at all. The time-honored gambit, in estimating production series, is to have the documented components represent the entire sector: a fool's gambit, that survives only because we "cliometricians" do not take measurement seriously. A moment's thought reveals that the procedure is doubly absurd: because "sectors" are arbitrary, and because industries producing substitutes may react similarly to demand shocks, but certainly not to industry-specific supply shocks. Imagine, to simplify things, that the only textile industries process cotton and linen; that neither is covered by output data; and that the apparent consumption of raw materials documents the growth of the first (because raw cotton is imported), but not of the second (because flax is home-grown). The growth of the textile sector is therefore represented by that of the cotton industry, in effect assuming that the linen industry matched its growth. A more palpably wrong-headed assumption is hard to imagine: we know from our "old" economic histories that the cotton industry was the first to be mechanized, that the linen industry was successfully mechanized over a century later; that technological change did not affect the cotton and linen industries together, but long favored the first *at the expense* of the second. That they could have grown in tandem beggars belief; but we cliometricians do not care for the facts, measurement does not interest us, whatever is available is good enough for our econometrics.

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<sup>14</sup> Economist economic historians once knew that too: Luigi Einaudi, the self-same one, warned us of exactly that (Einaudi 1936b, p. 7), but here too his teaching was forgotten.

<sup>15</sup> The auxiliary sources that may answer the questions raised by the data cannot be identified before those questions come up at all, whence the need to work on the data where such sources can be turned to as needed; the obtuseness of the above-cited referee for NSF still rankles.

The observed paths, documented by direct evidence, cannot represent the unobserved paths; the latter must be estimated as best one can by using *indirect* evidence. Logically, this is equivalent to locating an unobserved point in space by identifying constraints that exclude subspaces where it cannot be, or define loci where it has to be; it is essentially the logic of celestial navigation, save that the relevant stars are neither obvious nor tabulated. It takes a good, trained eye to identify the evidence at all, an Indian scout to see tracks where the cowboy sees only dust, a Holmes to see clues where Watson sees nothing, an old county vet to sort out the symptoms of the beast and reach the correct diagnosis. Change the metaphor, think of measurement as an engineering problem, the building of a bridge to span a gap in our knowledge: the engineer must identify the points in the terrain that can support it, and imagine the structure that exploits them. A richer set of supporting points allows a richer set of structures, and a richer set of structures allows the exploitation of a richer set of supporting points; both steps depend heavily on the creative powers, and experience, of the engineer. Set metaphor aside: the reconstruction of the past from our poor stock of inherited evidence reflects the observer's feel, intuition, above all knowledge – of the broader corpus of historical sources, of economic logic, of the relevant technology, institutions, and mores; it reflects, in a word, the observer's talents and experience.<sup>16</sup>

Four conclusions can immediately be drawn. The first is that measurement – arguably the most critical part of our (historians') craft, for it defines “the facts” that will then be interpreted – is difficult, uncertain, highly personal work: much more so than the subsequent analysis, facilitated and constrained by codified economic theory and econometric technique. The second follows immediately. When we P.I.s work on the measures provided by our r.a.s, we do not work on the evidence, on “the facts,” but on an idiosyncratic rendition of the facts, idiosyncratic if the r.a. tried in fact to reconstruct the past, idiosyncratic too, and as noted idiotic, if the r.a. merely vacuumed up the surviving (meretriciously) direct evidence: we may be master story-tellers, but if what we are given to work with involves three pigs and a wolf, we will never come up with *Henry V*. We accept these constraints, one presumes, because we do not recognize them at all; and we do not recognize them because we are so little concerned with measurement, with getting “the facts” right before we proceed with our elegant

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<sup>16</sup> See for example the evolution of the author's estimates for the engineering industry, recounted in Fenoaltea (2017b). *Quam possum facio* – and no, Virginia, we are not discussing marsupials.

(hi)stories. The third is related. Theory and technique are as noted codified; and we teach our students well, without holding back the secrets of our trade. It follows that our juniors are as well-equipped as we are (if not better, given their sharper wit) to process “the data,” to spin a story that fits and “explains” them; but only we their elders, suitably engaged, can trump them on experience, do much better than they can in conjuring up “the data” in the first place. We are economists, we teach and presumably understand comparative advantage, yet our behavior violates our very teaching; forgive us, for we know not what we do. The fourth brings us back to our starting point: the thread that runs through all this is that we cliometricians behave as economists, unconcerned with establishing “the facts” because we expect to have “the data” handed to us ready-made. With such a mind-set, we could hardly recognize, and avoid, our failure as historians.

### **3 Our failure as economic historians**

Our failure as economic historians has again to do with measurement, this time with our measure of the economy as a whole, what we call Gross Domestic Product: familiarly, GDP. Long ago, when teaching Economics 1 in the United States, I would end my presentation of the national income accounts with the question, “why does the U.S. have the world’s highest per-capita product?” (as it then was). The students answered with obvious references to advanced technology, abundant resources, “capitalist” efficiency (no comsymps there). Those reasons, I would answer, were true but superficial: “the real reason,” I would say, “is that the measure was invented here.” The point, of course, was that measured income was not a fact but a construct, one of many possible constructs.<sup>17</sup>

Our particular construct was defined by its particular genesis: who built it, to what purpose, and of what materials. The U.S. national accounts appeared *in utero* in the 1930s at Wesley Clair Mitchell’s National Bureau of Economic Research, an institution

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<sup>17</sup> This was before my year at the Institute, and no, I was not a protopostmodernist. Altogether more simply, I think, our (culture-bound) constructs serve the hegemonic group, typically a national patriarchy. Those who identify with that group have no reason to question those comforting constructs, but to outsiders (in this case, my Italian side) they do not ring true, and appear altogether more readily as the tendentious stories they are than as the facts they purport to be. That the scholars who undermined the Whig interpretation were disproportionately women (and from small countries) is not, to my mind, a coincidence (Fenoaltea 2006).

marked at once by its atheoretical approach, and by its specific interest in cyclical fluctuations (e.g., Lerner 1947); they emerged as official statistics in the U.S. shortly thereafter, and world-wide, essentially on the American model, in the aftermath of the Second World War.<sup>18</sup> They came of age in a world marked by the Great Depression, when it was widely believed that mature capitalism tended inevitably to crisis and mass unemployment, that rearmament and war had been only momentary, dreadful remedies, that the next great slump was just around the corner. Governments therefore took on the task of stabilizing the business cycle, and maintaining employment, with the tools suggested by the *General Theory*; but to employ them to good effect they needed timely evidence on the path of the economy. The national accounts were to provide that evidence, with minimal delay: they had to be calculated quickly, even if approximately, using statistics that were already available or easily obtained; they were to document the current path of the economy, its likely impact on paid employment.

The official accounts were shaped by Simon Kuznets, a protégé of Mitchell's. In his measure Kuznets included all agricultural production, for the market and not, because the available data were based on observed acreages and yields. He included industrial production only for the market, and counted its value added, or its value, depending on what data were already provided by the Department of Commerce.<sup>19</sup> Of the services Kuznets again counted those sold on the (legal) market, but also the imputed rental value of owner-occupied housing, again because the underlying ready-made statistics refer, as in the case of agriculture, to the aggregate stock. *Nada mas*: Kuznets gave us an empirical aggregate to solve a practical problem, a creature of the Bureau with no theoretical basis at all. It is not a *measure* of anything, it is at best a rough index of paid-

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<sup>18</sup> The success of the American model again owed more to hegemony than to technical merit. Istat (1957) had followed the Italian conventions, and excluded *intermediate* government services from aggregate final product; the Fuà team was funded by the Ford Foundation, and their estimates included them (Fuà 1969), as do our more recent ones.

<sup>19</sup> For most industry the Department had long (and laboriously) evolved measures of value added (Fenoaltea 1976); but the Department lacked information on the value of the sub-soil resources the extractive industries consumed, and Kuznets simply counted the mining firms' sales rather than their value added. The drawing-down of (underground) stocks is simply ignored; in strict logic, the mining sector is treated as if the goods it sells were created out of thin air rather than extracted (Fenoaltea 2005, pp. 306-307), whence of course the sky-high per-capita "product" of oil-producing deserts. To be precise, in the national accounts the mining firms' "value added" is computed by deducting from sales only the cost of purchased fuel and similar ancillary materials. An analogous "value added" for the transportation industries would deduct from the (c.i.f.) delivered value of the goods only the cost of purchased fuel and the like, and *include* the (f.o.b.) value of the goods at the point of origin. This mixing of value added and value demonstrates that the national accounts do not consistently measure production on a value added basis to avoid duplication (and sensitivity to vertical integration), as we tell our students: the underlying motivation was not theoretical but practical.

employment-generating production, an even rougher index of total product: and that in the short run, when the *ceteris paribus* clause may be a reasonable approximation.<sup>20</sup> It is not a fact, not an observation, but a construct, in fact a muddy one, good enough for government work.

And government work it became. A hundred years ago, the aspiration of the profession to empirical relevance led to the creation of *The Review of Economic Statistics*. That title contained a research program: the profession was asking itself what measures we wished to have, and how to construct them. Within thirty years that search was abandoned, that flag hauled down, that journal blandly renamed (“*The Review of Economics and Statistics*”): national and supranational bureaucracies took on the task of producing (and refining) Kuznets’ measure, and the economics profession left to them not only the administrative burden of collecting and processing the raw data, inevitably theirs, but the intellectual burden of defining the appropriate statistics, quintessentially ours.<sup>21</sup> What we do in the small, when we P.I.s take “the data” from our r.a.s, we do in the large, as a profession: our economic “science” did not jealously reserve unto itself the generation of its empirical evidence, we altogether relinquished responsibility for our “data.” The only parallel that comes to mind is astrology.

Kuznets rendered the profession a great service, and a great *disservice*: he called his construct not “an index of predominantly market-oriented, paid-employment-generating economic activity,” as he could and perhaps should have, not even “an index of gross domestic product,” which seems the least demanded by intellectual honesty, but, notoriously, “gross domestic product” *tout court* (actually “gross national product,” at the time, but that is here irrelevant). Kuznets himself knew perfectly well what it was: he was aware of its cultural specificity, and used it intelligently, for example seeing the post-bellum cost of the World Wars in the *loss* of GDP from unemployment in the wake of the First, and the *diversion* of GDP to armaments in the wake of the Second.<sup>22</sup>

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<sup>20</sup> The services of owner-occupied housing generate product but not paid employment; make-work projects, digging holes and refilling them, generate paid employment but no product; and so on, about which more below. A specialized index of paid employment and a specialized index of production are different tools; Kuznets’ all-purpose Swiss army knife does everything, badly.

<sup>21</sup> With exceptions (returned to below) that *were* exceptions, and confirm the rule.

<sup>22</sup> “[Kuznets] recognized that at very high levels of per capita product, preferences for leisure and immaterial products omitted by NIPA [the national income and product accounts] might come to predominate in an economy... Other items omitted from the NIPA accounts included improvements in health and increases in longevity...” (Fogel 2000; also, e.g., Higgs 1992). It was no doubt Kuznets’

Not so, however, the profession at large. We all know that GDP falls if a man marries his housekeeper, even if there is no change in her activities (*honi soit qui mal y pense*), in her product, and therefore in *total* product, *ceteris paribus*; we all know, or should know, that “GDP” is not the measure its label suggests. But in all our empirical work we use GDP to measure aggregate economic performance, I have never heard of a paper rejected by a journal for doing so. Why we so (mis)use the published “GDP” statistics I do not know. Does the practice derive from a tacit conspiracy, do we peer reviewers forgive each other’s sins to our mutual advantage? Does it derive from our religious approach, which much evidence suggests, to (“bourgeois”) economics?<sup>23</sup> Do we too believe in transubstantiation, that a statistic consecrated as a measure of gross product becomes that very thing? Do we too presume, more generally, that the faithful are only to accept the dictates of the clergy?<sup>24</sup> Or does our practice reflect, as other evidence suggests, the literal-mindedness of the verbally challenged, of those who do not grasp the complex relation between words and concepts because they never struggled, in their formative years, with Latin and Greek?<sup>25</sup> The cause is obscure, but the effect is clear enough: an economist is one who uses a government-issue screwdriver to hammer nails because it says HAMMER right on the handle.

We economic historians, in particular, have no interest in an index of the economy’s current path, in “GDP.” We want to gauge the evolution of economies over decades and

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confidence in his ability to see *through* his measure of GDP that led him simply to extrapolate it when his interests turned to long-term growth.

<sup>23</sup> It is as a religion that we defend it, that Marx is anathema has already been noted. Near a decade ago, the *European Review of Economic History*, *Fid. Def.*, drew the line very clearly: of my submitted paper the *Review* would publish the orthodox reconstruction of the expenditure-side accounts (since published elsewhere as Fenoaltea 2012), but not the heterodox call for a proper measure (largely reproduced here, and after hanging so long perhaps rather high). All this in the name of peer review, as Galileo also encountered.

<sup>24</sup> At the 2006 Bergen Workshop on Historical Accounts I argued, as I am doing here, that we should abandon conventionally-defined GDP in favor of a more reasonable measure. One colleague’s response was “We must take our criteria from Eurostat”: it is not for the faithful to define the faith.

<sup>25</sup> See the literal interpretation of “real” that plagues the entire literature on “real” measures of product, save the contributions of the classically educated (e.g., Sims 1969, Arrow 1974; contrast Fenoaltea 1976, Fuà 1993). See also Easterlin (1974), which fails to recognize how words function, that “happy” is a word like “tall,” that progress can no more raise the mean self-evaluation over time than it can bring everybody into the top quintile of the current distribution. See too, on a broader canvas, the great controversies internal to the cliometric school, on the “importance” of railroads and the “efficiency” of slavery, both sparked by, and giving Nobel-winning resonance to, the work of Robert Fogel: both in fact false controversies Fogel provoked by apparently intentional obfuscation, by verbal sleights of hand his (“verbally challenged”) critics did not see through (Fenoaltea 1981). Fogel was a scholar of exceptional background (years in the Communist Party), exceptional ability, exceptional ambition – and arguably, in furthering his career, an exceptional lack of ethical scruple (he once told me he published his claims with minimal supporting evidence, keeping his stronger results in reserve, so as to invite criticism and then win the subsequent argument).

more, we want to compare them to each other as well as to themselves earlier or later; and to do that we need a proper measure of the economy's product, a measure of the opportunity set, in goods-space, it made available to those then alive (over their expected lives, at that, and not in any one year, think of the later fourteenth century). A number of considerations come immediately to mind. Market exchange and paid employment are, as such, simply irrelevant (Pollak 1985): our measure must count unpaid "family production" (typically the work of women, there is more than one battle to be fought here), the unpaid services of durables, including both consumer durables (not just owner-occupied housing but also, e.g., the appliances that allowed housewives to work also outside the home, Gordon 2016) and common-use infrastructure (the piazzas their Italian "owner-occupiers" enjoy daily, and Americans cross an ocean to see, which is of course where I came in), and obviously leisure (corrected for morbidity); and it must count the all-important gifts of nature, that vary from time to time and place to place. By the same token, our measure must exclude not just product-less make-work projects but "social intermediates" (armaments, by extension the police and the judiciary, perhaps the legal professions), and allow for negative externalities: production externalities (environmental costs, including if we want to count it here the reduction of our subsoil assets), and consumption externalities too, those caused both by congestion (the crush of tourists that has rendered our favorite piazzas quite unlivable) and by social rivalry (which turns increasing consumption into a zero-sum game, Veblen 1899, and may well destroy much of what we call "modern economic growth").<sup>26</sup>

Some of the above points are of course long familiar, made even by eminent mainstream economists (most famously Nordhaus and Tobin 1972; also Kuznets himself, above, footnote 22; also, e.g., Baran 1957); but what is striking is the broader

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<sup>26</sup> This paragraph could easily be expanded into a book, but a few points bear immediate notice. One is that the flow account must be complemented by a stock account, with the former incorporating the period changes in the latter; the current product includes investment, by firms and households (as the present value of future services), and excludes disinvestment (the drawing down of stocks due to obsolescence, catastrophe, depletion, and depreciation: our fixation with gross rather than net product may reflect the original concern with paid employment, or a deeper concern that the available depreciation data reflect tax-accounting rules rather than any underlying reality). Another is that the value of free goods cannot be gauged by their market price, sending us back to Dupuit. In the presence of free goods, it may be noted, our "GDP" figures vary in the wrong direction altogether: the opportunity set of people who must arm themselves against a threat, or heat their houses, is smaller than that of those who have no need to, *ceteris paribus*, but their "GDP" is greater. Our measure should grow, and not decline, as we approach Eden, or Marx's communism. A third is that consumption externalities may well validate the essential message of Easterlin (1974), despite the ambiguity of the evidence it adduces (footnote 25).

literature. Even the improvements proposed by some of the best of us fell finally on deaf ears, the deaf ears of our profession: *envers et malgré tout* the measure of “GDP” we economists use has remained Kuznets’, with the touch of lipstick added by the bureaucrats.<sup>27</sup> And in that broader literature the cliometric school is the dog that didn’t bark: *as a school* we should have been the spearhead of a movement to develop a proper measure of total domestic product, for ourselves and, derivatively, for our fellow economists. That the initiatives in that direction involved not the cliometric school but individual economists, and then the Elysée, condemns us all: we fell short of our professional responsibilities, doctors at the scene of an accident who sat on their hands and let the boy scouts deliver first aid.

Our failure to act may be considered another of our failures as economists. Our failure as economic historians is more specific, for we did worse than nothing: we extended back in time the ordinary pseudo-measures of “gross domestic product,” aiding and abetting, as it were, the enemy within. Individuals and more or less extensive teams produced conventional “historical national accounts” for one country after another; and they were pulled together and extended by Angus Maddison – in his formative professional years an international bureaucrat – and his successors’ “Maddison Project.”<sup>28</sup> As noted above the series Maddison collected may grossly distort the empirical record, and his own contributions were no better; but economists and economic historians take their evidence ready made, and have happily run amok exploiting these *comptes fantastiques* in studies that span the ages and the continents.<sup>29</sup>

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<sup>27</sup> Nordhaus and Tobin labelled their statistic a “measure of economic welfare,” rather than “a correct measure of domestic product.” That implicitly accepted conventionally measured GDP as correct in its own domain, and gave much too much away. Both environmental damage and the extraction of subsoil resources are forms of disinvestment, the drawing down of stocks, to be deducted on utterly standard grounds.

<sup>28</sup> I too have contributed to that literature (Fenoaltea 2005, 2012, 2017a, 2018a, 2018b) – in the belief that if we are to calculate “GDP” at all we should do so without obvious error, and in the certain knowledge that the components of our GDP estimates are of interest in their own right – *mais sans être dupe* (Fenoaltea 2018c).

<sup>29</sup> Two of Maddison’s contributions come particularly to mind. One is his revision of the early Italian estimates for the decades from Unification (1861) to World War I (Maddison 1991; see Fenoaltea 2005). He believed these underestimated growth, and that the early backcast figures were correspondingly too high. To increase the measured growth rate he replaced the extant series for industry with one that grew much more rapidly, which he constructed using various series I had produced, *excluding the estimates for the less dynamic artisanal sectors*; and he combined his sector series “at 1870 prices,” *attributing to industry the (large) backcast share implied by Istat’s slowly growing series rather than the far smaller share implied by his own* (criticisms made and acknowledged prior to publication, in correspondence that survives). The other is his extension of GDP estimates to early times by inferring the surplus over subsistence from the urban share of the population: another fool’s gambit, based on the Whig assumption

Our backcasting of what we call “gross domestic product” is in fact intrinsically laughable. Imagine us in our Valhalla, imagine our conversation with economic historians yet unborn, imagine that they ask us what our generation did. Shall we be allowed to answer “We reconstructed the historical national accounts” (“Oh, wow!”)? Or will Valhalla admit only the unvarnished truth? “We reconstructed the short-term indices of paid-employment-generating-production that would have helped past governments implement their stabilization policies, had they had our statistics and had they had such policies” (“You did *what???*”). We cliometricians are climate historians who reconstruct not past weather, but past weather forecasts: we have failed, sadly, embarrassingly, even as economic historians. With luck Valhalla does not exist at all, and our embarrassment will end with our death.

#### 4 Envoi

I have a dream.

I dream of a day when economics is not an oxbow off the flow of Western culture, a day when economists are educated as well as trained, a day when we understand the nature of what we call our “science.” I dream of a day when the reconstruction of “the data” is recognized for the serious, delicate, creative endeavor that it is. I dream of a day when economic historians’ measures of past economies’ aggregate product do not invite ridicule.

I dream that we cliometricians can take history and the humanities as seriously as we take economics, and lead us to the promised land.

Not a threnody, rather a brow-beating, a call to action, a philippic: mercifully, perhaps, at an end. But I have a dream, and now that you need him James Earl Ray is no longer around.

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that civilization arose when technical progress generated such a surplus (Fenoaltea 2006), and which further assumes that that surplus was consumed entirely as urban goods, and not overwhelmingly, as is far more likely, as leisure.

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