

For the session “Wither the Future of Economic History?”

8:00 am-10:15, Saturday, Jan 5

Marriott Philadelphia Downtown, Meeting rm. 308

ASSA Convnetion 2018

## **Economic History as Humanomics, The Scientific Branch of Economics**

Deirdre Nansen McCloskey

Emerita, University of Illinois at Chicago

[deirdre2@uic.edu](mailto:deirdre2@uic.edu)

deirdremccloskey.org

Sessions in any field of the intellect about “whither the future of X” have a deep intellectual problem of an economic character. The problem is that if you or I were so smart, then you or I would be rich. If anyone could predict the future of, say, mathematics, she could arbitrage between the present and the future. As Tom Lehrer sang long ago, she would “publish first.” She would achieve riches in a coin relevant to her preferences, namely immortal fame. She would be the Euler of the 21<sup>st</sup> century.

The principle is identical to the more obviously economic one that predictions of the stock market or housing prices or hem lines of skirts are useless. As they say in Hollywood, nobody knows anything. That *Rocky* was a hit doesn’t mean that *Rocky 2* or *3* will be. We have to make predictions, of course, and necessarily we place bets on them. The future is coming, whether we like it or not, and our bets as producers of movies or of mathematics will determine how we personally do. But if good predictions--better than what the average punter makes with his bookie or in the forward markets--were achievable by studying econometrics or by following Warren Buffett, we would all be above average, as in Lake Wobegon. It ain’t happenin’.

So in sober truth such sessions are actually about “What Do I *Want* Economic History to Become.” I am therefore to be allowed to make unrealistic “predictions.”

§

I actually think, to be realistic for a moment, that it is probable that economic history will continue for the next decade or so to be dominated by scientism. Scientism is the belief that you are only Scientific if you follow a Method of Science laid down by amateur philosophers fifty or a hundred or two hundred years ago. In cliometrics everything is supposed to be quantitative, because then we are scientists. In science generally the method is supposed to be Baconian, expressed by Sherlock Holmes in "A Study in Scarlet" as "it is a capital mistake to theorize before you have all the evidence. It biases the judgment." In history the method comes from Leopold von Ranke's first book, in the 1824, in the form of *wie es eigentlich gewesen*, "as [the past] actually was," and in American history from the 1880s to the present in the form of "that noble dream" of an objective historical science.<sup>1</sup> In economics the method comes from Lionel Robbins in the 1930s, influenced by an Austrian logical positivism already under devastating attack by actual philosophers. Nonetheless the illogical method of logical positivism was enthusiastically seconded by Samuelson in the 1940s and Friedman in the 1950s.

The method eventuated in the official constitution of Samuelsonian economics, drafted by Tjalling Koopmans in 1957, *Three Essays on the State of Economic Science*. Koopmans (whose name, by the way, means "salesman") recommended a theoretical/empirical specialization. He recommended that theorists spend their time gathering a "card file" of *qualitative* theorems attaching a sequence of axioms  $A'$ ,  $A''$ ,  $A'''$ , etc. to a sequence of conclusions  $C'$ ,  $C''$ ,  $C'''$ , etc., *separated from* the empirical work, "for the protection [note the word, you students of free trade] of both," both the theorist and the empiricist. Then the empirical econometricians down the basement will get to work to see if in the world  $A'$  leads to  $C'$  or to  $C'''$ .

The official method would be fine if the theorems were not merely qualitative, the way Samuelson in *Foundations* had laid down they could be. If they took instead the *quantitative* form of the math used by physicists or geologists, in contrast to the on/off existence theorems that mathematicians and economists love so much, good. Then the duller wits like McCloskey the economic historian could be assigned to mere observation, filling in the blanks in the theory. But *there are no blanks to fill in*, no How-Much questions asked, in the sort of theory that economists admire and that absorbs much of their waking hours (in recent years a little less, I am glad to acknowledge, in favor of quantitative simulation, praise the Lord).

I am here to tell you that the Samuelson-Friedman-Koopmans method will go on being used in economic history for a while, until economic historians realize that whatever its prestige in economics, and its power to overawe in history, it is bankrupt.

In its theoretical branch the excess of liabilities over assets in the method is well illustrated by non-cooperative game theory. For one thing, experimental economics has shown over and over again that the premise of non-cooperation is factually mistaken in humans. For another, finite games unravel, and infinite games have infinite numbers of

---

<sup>1</sup> Novick 1988 on American history. Novick argues that *eigentlich* should actually be translated "essentially," which gives the phrase a less naively Baconian essence.

solutions. No one is against theory, if it means economic ideas. Informational asymmetry. Computational general equilibrium. Good. But if all we have is Koopmans' card file of qualitative theorems out to  $A^{100}$ , none tested, even in the rare cases that they can be, what do we have, scientifically speaking?

Ah, but you will reply, we *do* test, with econometrics. No we don't. Name the important factual economic proposition since the Second World War that has been rejected or accepted by econometric test. Robert Fogel subtitled his book of 1964 on railroads *Essays in Econometric History*. But Bob did not use econometrics, even by the definition of 1964. He used simulation. Rich Weisskoff and I were the RAs for John Meyer about that same time, incompetently helping edit his essays with Alfred Conrad for a book entitled *The Economics of Slavery: And Other Studies in Econometric History* (also 1964). It was in fact simulation and accounting. One of Meyer's simulations, an input-output study of British growth in the late nineteenth century, led me out of using Keynesian aggregate demand for the long run, when I realized that John had done so and that it did not make a lot of sense in terms of opportunity cost.

Three terms of econometrics, such as I took (with Meyer in one course and Guy Orcutt, that pioneering simulator, for the rest), with no graduate training in other empirical methods--such as simulation, archival research, experiment, surveys, graphing, national income accounting – makes modern PhDs into savants of tests of statistical significance. Test, test, test says David Hendry. The trouble is that such tests are themselves bankrupt, as for example Kenneth Arrow noted in the same year as Koopmans' constitution (Arrow 1957). You will not believe that null hypothesis testing without substantive judgment of magnitudes is bankrupt, I know. Perhaps you can believe the report in 2016 of an official committee of the American Statistical Association (ASA 2016). I predict that someday you will get it, and will give up mechanical tests of statistical significance at the .05 level and will start doing actual science.

On the side of theory and Koopmans' card file, I worry in economic history about "analytical narratives," which seem to be popular with neo-institutionalists of the Northian tendency. The trouble again is the lack of quantitative testing. It doesn't seem to happen to a high standard. If the analysis is "consistent with" some little piece of economic history, all is said to be well. What one would like to see is quantitative oomph, or else the humanist's substitute for quantity, comparative histories. Either or both would do.

I hesitate to cast the first stone, because I am not without sin. True, as the men caught in the Me Too movement nowadays often say in extenuation, the sinning was a long time ago. Still, by confessing my own sins here and now I can sidestep the impoliteness of naming particular works by my beloved colleagues in economic history that routinely misuse analytic narratives – that is, existence theorems, weakly "consistent" with the data, and not comparative, either. It would not be difficult to name them. It would be even easier to name colleagues who use tests of statistical

significance – also weakly “consistent” with the data, at low power, and anyway usually irrelevant to the economic and historical question at issue, which is almost always not fit but coefficient size, substantive oomph

Bless me, father, for I have sinned. It has been half a century since my last confession.

In 1971 I gave a paper to the meetings of the Economic History Association, published the next March as “The Enclosure of Open Fields: Preface to a Study of Its Impact on the Efficiency of English Agriculture in the Eighteenth Century.” Sounds swell, eh? But I remember uneasily that my commentator in the session at the meetings, the law professor and student of environmental law and economics, Earl Finbar Murphy, complained that I had not shown in the paper that my very clever analytical narrative had quantitative oomph. I was distressed at the complaint, and, in the way of young scholars, angry. But I must have taken the complaint to heart, because the next time I ventured into the open fields and their enclosures I made sure to provide the quantitative goods, in a paper in 1976 called “English Open Fields as Behavior Towards Risk.” When my friend Stefano Fenoaltea challenged the argument with another analytic narrative, also not tested for oomph, and uncharacteristically for Fenoaltea lacking the quantitative goods, I wrote with a student at Chicago, John Nash, a paper measuring the oomph of storage of grain as insurance alternative to scattering of plots of land (McCloskey and Nash 1984). Oomph.

And in that paper with John, to speak of econometrics, in order to isolate the cost of storage per month, including interest, we regressed changes in the prices of grain in one location against the number of months the change was measured over. The slope is what mattered. We had the sense not to use tests of significance to “test” what any economist already knows, that prices rise after a harvest by the cost per month of storage. They have to, for elementary reasons of arbitrage. Our  $R^2$ s were derisory. But so what? We were filling in the factual blanks in a quantitatively specified theory.

And again about the same time (the light was dawning in me slowly, slowly) I wrote a paper with J. Richard Zecher on “How the Gold Standard Worked” (1976), which used regression analysis to articulate a quantitative standard of what “one market” means. We did not do what studies of market integration routinely do down to the present, which is to “test” against a .05 standard of significance “whether or not” separate markets were integrated. Such a test is meaningless. There is no sense, Dick persuaded me, in which on/off, yes/no is a scientific standard. One has to have a comparative standard, such as within-USA integration of the market in bricks compared with international integration, USA vs. UK, for example. It’s true in physics and it’s true in economic history.

§

Enough of reality. What do I want economic history to become? What are my unrealistic predictions? In brief: I want it to continue to be the scientific part of economics and of history, but to get even more scientific than it is now.

Many economic historians trained as economists lack self-confidence in the face of their proud if ignorant colleagues in theory and econometrics, and therefore do not realize that economic history is the Darwinian-scientific part of economics. Economic historians trained in other fields such as sociology or history itself or in departments of economic history in the Old World are less inclined than their cliometric colleagues in the United States to whore after the latest “insight” from the theorists or the latest “technique” of the econometricians identifying the number of angels on the head of a pin. But anyway, I say again, with pride at my colleagues’ accomplishments worldwide, economic history is the almost completely *scientific* portion of economics and of history.

Realize, though, that the word “science” is a big problem in English, and is misleading economic historians to try to imitate what they imagine happens in physics. In all other languages, from French to Tamil and back, the local science word means merely “systematic inquiry,” as distinct from, say, casual journalism or unsupported opinion. In German for example *Geisteswissenschaften*, which means literally in English a spooky sounding “spirit sciences,” is the normal German word for what American academics call the “humanities,” the British “arts.” The Dutch to this day speak of *kunstwetenschap*, “art science,” which English speakers now would call “art history” or “theory of art” and place firmly in the humanities arrayed against science. An Italy a proud mother of a 12-year old girl who is doing well at school speaks of *mea scienziata*, which sounds strange indeed in recent English, “my scientist.”

In earlier English *Wissenschaft* or *wetenschap* or *scienza* is what “science” also meant in English. Thus Alexander Pope in 1711: “While from the bounded level of our mind / Short views we take, nor see the lengths behind: / But more advanced, behold with strange surprise / New distant scenes of endless science rise!” (Pope 1711, *Essay on Criticism*, lines 221–224). Then in the mid-19<sup>th</sup> century, as a result of disputes over chairs of chemistry in Oxford and Cambridge, the word was specialized to the systematic study of the physical world. In the *Oxford English Dictionary*, the new meaning, slowly adopted from the 1860s on (Alfred Marshall never did adopt it, but by the time of Keynes everyone had), became sense 5b, the dominant sense now, the lexicographers of Oxford inform us, in ordinary usage.

The usage of the last century and a half makes for endless yet silly disputes about whether economics is a science, and gives natural scientists permission to issue lofty sneers about social *science*. Yet what would it matter to the practice if after learned dispute we decided economics or economic history were *not* sciences? I suppose we would be banished from the National Science Foundation or the National Academy of Science, which would be sad and unprofitable. But would the banishment change the actual practice of economic or historical science?

In actual practice, indeed, the sort of categorical issues that occupy *humanistic* sciences are an essential step in any systematic inquiry, whether of physical or social or conceptual matters. The humanities – such as literary criticism in the Department of Literature, and number theory in the Department of Mathematics, and transcendent meaning in the Department of Theology – study categories, such as good/bad, lyric/epic, 12-tone/melodic, red giant/white dwarf, hominid/Homo sapiens, prime/not, God/gods, exist/not. The crucial and neglected point in the battle of the Two Cultures is that such humanistic and human categorization is a *necessary initial step in any scientific argument*. You have to know what your categories *are* by well-considered definition, such as Homo sapiens sapiens/Homo sapiens neanderthalensis, before you can *count* their members. This is obvious – though not to the George Stiglitzers or Michael C. Jensen or Murray Rothbards of economics.

For example, economic theory is humanistic, dealing in definitions and their relations, sometimes called "theorems" or, more usefully for an empirical science, "derivations." Theory makes remarks about categories--as Coase did: Transactions costs may be important here, and this is how they should be defined. Or, as Irving Fisher and Milton Friedman said,  $MV = PT$ . Or, as Edgeworth and Samuelson said,  $(dU/dx)/(dU/dy) = P_x/P_y$ . Or, as the Austrian economists say, markets may be more about events out of equilibrium than about equilibrium. Or, as Israel Kirzner and now Deirdre McCloskey might put it, discovery may be more important for human progress than is routine accumulation or routine maximization of known functions.

At the level of economic theorizing, such folk are humanists, dealing in categories and derivations, in advance of and sometimes in lieu of examining the history of actual markets. I recently spent some time browsing through Jean Tirole's textbook on the theory of finance (2006; Nobel 2014). The book gathers some hundreds of theories, with no evidence supplied about which of the theories might apply to actual financial markets. It is as much an exercise in humanism as is Kant's *Critique of Pure Reason* or Ramunajan's notebooks on number theory.

Some definitions and their corresponding theorems are wise and helpful, some stupid and misleading. The humanities, and the humanistic steps in any science, study such questions, offering more or less sensible arguments for a proposed category being wise or stupid, short of counting or comparison or other factual inquiry into the world. The humanities study the human mind and its curious products, as for example John Milton's *Paradise Lost* or Mozart's Flute and Harp Concerto in C (K. 299) or the set of all prime pairs or the definition of GDP. The studies depend on categories, such as enjambed/run-on lines or single/double concerti or prime/not-prime or marketed/unmarketed, such as we humans use.

In 1910, for example, many economists and other scientists such as the great statistician Karl Pearson believed that the category "Aryan race" was wise and helpful

in thinking about the economy and the society.<sup>2</sup> Around then the American Progressives, and especially the leading economists among them, believed passionately in racism, and advocated policies such as immigration restrictions and the minimum wage to achieve eugenic results in favor of the Aryan race (Leonard 2016). Later we decided, after some truly disturbing experiences and more reflection, that "race" aside from *Homo sapiens sapiens* was actually a stupid and misleading and even evil category. The decision itself depended on reflections on the humanistic categories of helpful/misleading, wise/stupid, good/evil.

The necessity of the humanistic first step, note well, applies to physical and biological sciences as much as to *les sciences humaines* or *die Geisteswissenschaften*. Meaning is scientific, because scientists are humans asking questions interesting to them about the meaning of  $\beta$  decay. Such is the main conclusion of science studies since Thomas Kuhn. The Danish physicist Niels Bohr wrote in 1927, that "It is wrong to think that the task of physics is to find out what the world is. Physics concerns what we can *say* about it."<sup>3</sup> We. Humans. Say. With words. About such *geisteswissenschaftliche* categories the German-American poet Rose Äuslander wrote, "In the beginning / was the word / and the word was with God / And God gave us the word / and we lived in the word. / And the word is our dream / and the dream is our life."<sup>4</sup>

We dream of categories, in our metaphors and stories, and with them make our lives, especially our scientific lives, saying the world. The poet Wallace Stevens exclaims to his companion, walking on a beach in Key West, "Oh! Blessed rage for order, pale Ramon, / The maker's rage to order words of the sea," the human arrangement of words imposing order on the world: of the woman they had heard singing, "when she sang, the sea, / Whatever self it had, became the self / That was her song, for she was the maker."

There is nothing scary or crazy or French or postmodern or nihilistic about such an idea. The "hardest" sciences rely on human categories, and therefore on human rhetoric and hermeneutics, the speaking and the listening sides of human conversation in the sciences. The category of "capital accumulation," for example, can be defined in an aggregate, Smithian-Keynesian way. Or it can be defined in a disaggregated, project-specific Austrian way. It matters to the science. The humanistic job of economic theory is to ponder the categories, to see their internal logic, to criticize and refine them,

---

<sup>2</sup> A late example of his views is Pearson and Moul 1925, "Taken on the average, and regarding both sexes, this alien Jewish population is somewhat inferior physically and mentally to the native population." And an early one is Pearson 1892 (1900), pp. 26-28, "From a bad stock can come only bad offspring. . . ."

<sup>3</sup> Quoted in *Niels Bohr: Reflections on Subject and Object* (2001) by Paul McEvoy, p. 291. The provenance of the remark is a little hazy, but it is well known. In Danish, the philosopher Hans Siggaard Jensen informs me, it was something like "*Fysik er ikke om hvordan verden er, men om hvad vi kan sige om den.*"

<sup>4</sup> *Am Anfang/war das Wort/und das Wort/war bei Gott/Und Gott gab uns das Wort/und wir wohnten/ im Wort/ Und das Wort ist unser Traum/ und der Traum ist unser Leben.*

just as in the departments of English and of physics.

But the humanistic step – though I am saying it is quite necessary for scientific thought – is of course not in a factual science like economics the whole scientific job. It is a point that economists regularly miss in their fascination with the blessed rage for order. Theory is not science *tout court*. One could have a theory of epics or concerti that never applied to any actual epic or concerto, and indeed foolishly misrepresented them as they are in the actual human world. Specializing in humanistic theorizing of the sort that Kenneth Arrow or Frank Hahn did is dandy, but it does not do the entire scientific job unless it is at some point firmly attached to experiment or observation or other tests against the world, as much of the work of these two brilliant men never was. The philosopher and economist Arthur Diamond looked into the empirical uses of abstract general equilibrium theory such as Arrow and Hahn practiced, and found that there was none.<sup>5</sup> If you are making a quantitative point, as must happen in a policy science like economics or in a world-speaking science like physics or in the glorious systematic inquiry into the past of the business of ordinary life called economic history, then after the humanistic step you must proceed to the actual count or the testing comparison. Count the deaths from plague in the 1340s or compare its impact in China.

Too often in economics the count or comparison does not happen, because economists think, as I have said, that theorems offer factual "insight," and believe that statistical significance "tests" the theory against facts. The two sides, theory and econometrics, they say, therefore can specialize and specialize and specialize. Never trade. Such a procedure believes it imitates physics, without understanding how physics actually works. Physicists, as one can see in the lives and writings of Enrico Fermi and Richard Feynman, spend much of their time studying the physical equivalent of the *Journal of Economic History*.

## §

So what? Here's what. Economic history should become as humanistic as it is now anti-humanistic. It will become so if we do not keep being anxious that we might not be worthy of the white coats of the Scientists, sense 5b.

The word is "humanomics," coined by the indubitably scientific experimental economist Bart Wilson and embodied now in a book in progress by Bart and the Nobelist and founder and first president in 1986 of the Economic Science Association, Vernon Smith. I quarreled with Vernon at the time about the appropriation of the word "science" for what is exclusively an association of laboratory experimenters. By now he and I thoroughly agree that "science" covers more than imitations of physical sciences. Humanomics does not abandon what we can learn from such imitations, and is certainly not against mathematics or statistics. But it invites the methods of the humanities into economic science. Bart and Vernon and colleagues, for example, are

---

<sup>5</sup> Diamond 1988, though Leland Yeager noted correctly that it does provide a useful "integrating factor of the whole body of economic theory" (Yeager 1999, p. 28).



increasingly studying the meaning that their experimental subjects attach to their actions, as revealed by humanistic techniques of textual analysis of what the subjects say to each other. It has been known for decades in experimental economics that letting the subjects speak to each other can radically change the results. The study of human meaning casts light on the ordinary business of life.

We in economic history are well placed to take advantage of humanomics. But to do so, clearly, we need to set aside our anxieties about the National Academy of Science, and listen to all the evidence of the economy, whether it comes in the form of statistics of exports or the themes of contemporary drama. That last, for example, is one piece of evidence that attitudes towards business were radically changing in England in the first decades of the 18<sup>th</sup> century.

Our colleagues in economics are trudging in the other direction, with a behavioral economics that ignores human meaning in favor of insisting in the manner of 1930s psychology that all that matters is external behavior; or, more extremely anti-humanistic, neuro-economics, which studies the brain but ignores the mind, as though we could understand Jascha Heifetz' fiddle playing by a closer and closer study of his muscles.

I do not wish merely to preach (although come to think of it there's nothing wrong with preaching the gospel of scientific common sense). Let me give a concrete example of the scientific payoff of humanomics.

The Great Enrichment per capita in real terms by a factor of 20 or 30 or much, much more since 1800 is the most astounding economic change since the domestication of plants and animals. Historians, economists, and economic historians have been trying to explain it since Smith. Recently some have come to concentrate on the role of ideas, as in the work of the economic historians Joel Mokyr and Eric Jones, the historian Margaret Jacob, the historical sociologist Jack Goldstone, the anthropologist Alan MacFarlane or the economist Richard Langlois, or myself, among a few others.

The Great Enrichment has usually been explained by material causes, such as expanding trade or rising saving rates or the exploitation of the poor or changes in the rules of the legal game. The trouble is that such events happened earlier and in other places. Such a point in criticism of the instinctive materialism of economic scholars after 1890 or so is itself a use of the humanist's substitute for quantification, comparison. The material events cannot therefore explain the Industrial Revolution (which in fact has earlier parallels, a mere doubling of incomes). Especially they cannot explain its astounding continuation in the Great Enrichment of 3,000 percent per capita. One can show in considerable detail (McCloskey 2010) that the material causes we study in economics do not work. One can also show (McCloskey 2016) how attitudes towards the bourgeoisie began to change in the 17th century, first in Holland and then in an England with a new Dutch king and new Dutch institutions.

One hypothesis is that if the social position of the bourgeoisie had *not* been raised in the way people spoke of it, aristocrats and their governments, or the bourgeoisie itself in guilds and mercantilism, would have crushed innovation, by regulation or by tax, as they had always done. And the *bourgeois gentilhomme* himself would not have turned inventor, but would have continued attempting to rise into the gentle classes. Yet if the material methods of production had not thereby been transformed, by 1800 or so, the social position of the bourgeoisie would not have continued to rise. One could put it shortly: without spoken honor to the bourgeoisie, no modern economic growth. (This last was in essence of Milton Friedman's Thesis). And without modern economic growth, no spoken honor to the bourgeoisie. (This last is in essence of Benjamin Friedman's Thesis.) The two (unrelated) Friedmans capture the essence of freed men, and women and slaves and colonial people and all the others freed by the development of admiration for bourgeois virtues.

The causes were liberalism (McCloskey 2016), the scientific revolution (Mokyr 2002, 2016; not, however, in its direct technological effects, which were postponed largely until the 20th century), and above all a change in the rhetoric of social conversations in Holland and then in England and Scotland and British North America about having a go. The change in rhetoric was in turn a result of accidents of politics and society in northwestern Europe from the Reformation to the French Revolution that made people bold.

One can ask how an explicitly and persuasive bourgeois *ideology* emerged after 1700 from a highly aristocratic and Christian Europe, a Europe entirely hostile – as some of our clerisy still are – to the very idea of bourgeois virtues beneficial to the poor. In 1946 Schumpeter declared that "a society is called capitalist if it entrusts its economic process to the guidance of the private businessman" (*Encyc. Brit.* 1946). It is the best short definition of that essentially contested concept and misleading word, "capitalism." (Misleading because it invites us to focus on aggregate capital accumulation rather than on the particular and Austrian discovery of ideas for betterment that actually made the modern world.) "Entrusting" the economy to businesspeople, Schumpeter explained, entails private property, private profit, and private credit. (In such terms you can see the rockiness of the transition to capitalism in Russia, say, where agricultural land is still not private, and where private profit is still subject to prosecution by the state, the jailing of billionaires, the cutting down of tall poppies.)

Yet what Schumpeter leaves aside in the definition, though his life's work embodied it, is that the society – or at any rate the people who run it – must *admire* businesspeople. Schumpeter, as Richard Langlois has noted, had no sociological theory. That is, people must come to think the bourgeoisie capable of virtue. (It's this admiring of the bourgeois virtues that Russia lacks, and has always lacked, whether ruled by boyars or tsars or commissars or Putin and his friends, ever since Muscovy long ago fended off the Mongols, at the sacrifice of commercial Novgorod.)

Attributing great historical events to ideas was not popular in professional history for a long time, 1890-1980. A hardnosed calculation of interest was supposed to explain all. Men and women of the left were supposed to believe in historical materialism, and many on the right were embarrassed to claim otherwise. But such a result of the “dream of objectivity” hasn’t work out all that well. Actual interest – as against imagined and often enough fantasized interest – did not cause World War I. The Pals Brigades did not go over the top at the Somme because it was in their prudent interest to do so. Non-slave-holding whites did not constitute most of the Confederate armies for economic reasons. Nor did abolition become a motivating cause because it was good for capitalism. And on and on, back to Achilles and Abraham pursuing their honor and their faith.

We do well to watch for cognitive-moral revolutions, and not simply to assume that Matter Rules, every time. A showing that ideas matter is not so unusual nowadays among historians. But it is another project to show that the material base itself is determined by habits of the lip and mind – *that* conclusion evokes angry words among most people on the economic side of the social sciences, and often enough from historical materialists in the humanities.

In short, the Great Enrichment is important example of the force of language in the economy – its linguistic embeddedness as the sociologists would put it. In the economy the force of language is not to be ignored. (Or that it *is* to be ignored: if the research is genuine the possibility must be lively that the hypothesis turns out to be wrong.)

Thus “humanomics.” Ignoring the burden of art and literature and philosophy in thinking about the economy is bizarrely unscientific. It throws away, under orders from an unargued and demonstrably silly method, a good deal of the evidence of our human lives. I do not mean that “findings” are to be handed over from novels and philosophies like canapés at a cocktail party. I mean that the exploration of human meaning from the Greeks and Confucians down to Wittgenstein and *Citizen Kane* casts light on profane affairs, too. A human with a set of virtues and vices, beyond the monster of interest focusing on Prudence Only, characterizes our economies, if not our economics.

And so (the hypothesis goes) an economic history without meaning is incapable of understanding economic growth, business cycles, or many other of our mysteries. A humonomic economic history would extend but also to some degree call into question the techniques of modern economics, and the numerous other social sciences from law to sociology now influenced by an exclusively Max *U* theory.

Economic history, that is, can embrace the humanities and become more, not less, scientific.

## Partial List of Works Referred To

- American Statistical Association. 2016, "Statement on Statistical Significance and P-Values." *The American Statistician* 70 (2): 129-133. At <http://amstat.tandfonline.com/doi/pdf/10.1080/00031305.2016.1154108>
- Arrow, Kenneth J. 1960. "Decision Theory and the Choice of a Level of Significance for the *t*-Test." Pp. 70-78 in Olkin, Ingram, et al., eds. 1960. *Contributions to Probability and Statistics: Essays in Honor of Harold Hotelling*. Stanford: Stanford University Press
- Bohr, Niels. in *Niels Bohr: Reflections on Subject and Object* (2001) by Paul McEvoy.
- Diamond, Arthur M. Jr. 1988. "The Empirical Progressiveness of the General Equilibrium Research Program." *History of Political Economy* 20 (1): 119-135.
- Koopmans, Tjalling. 1957. *Three Essays on the State of Economic Science*.
- Langlois, Richard.
- Leonard, Thomas C. 2016. *Illiberal Reformers: Race, Eugenics and American Economics in the Progressive Era*. Princeton, NJ: Princeton University Press
- McCloskey, Deirdre N. 1972. "The Enclosure of Open Fields: Preface to a Study of Its Impact on the Efficiency of English Agriculture in the Eighteenth Century," *Journal of Economic History* 32 (1, Mar): 15-35.
- McCloskey, Deirdre N. 1976. "English Open Fields as Behavior Towards Risk," *Research in Economic History* 1 (Fall): 124-170.
- McCloskey, Deirdre N. 2010. *Bourgeois Dignity: Why Economics Can't Explain the Modern World*. Chicago: University of Chicago Press.
- McCloskey, Deirdre N. 2016. *Bourgeois Equality: How Ideas, Not Capital or Institutions, Enriched the World*. Chicago: University of Chicago Press.
- McCloskey, Deirdre N., and John Nash. 1984. "Corn at Interest: The Extent and Cost of Grain Storage in Medieval England," *American Economic Review* 74 (Mar 1984): 174-187.
- McCloskey, Deirdre N., and Joseph Richard Zecher. 1976. "How the Gold Standard Worked, 1880-1913," in J.A. Frenkel and H. G. Johnson, eds., *The Monetary Approach to the Balance of Payments* (Allen and Unwin), pp. 357-385.).
- Mokyr, Joel. 2002. *The Gifts of Athena*. Princeton, NJ: Princeton University Press.
- Mokyr, Joel. 2016. *A Culture of Growth: Origins of the Modern Economy*. Princeton, NJ: Princeton University Press.
- Novick, Peter. 1988. *That Noble Dream: The 'Objectivity Question' and the American Historical Profession*. Cambridge and New York: Cambridge University Press
- Pearson, Karl and Margaret Moul. 1925. "The Problem of Alien Immigration into Great Britain, Illustrated by an Examination of Russian and Polish Jewish Children." *Annals of Eugenics* 1(1/2): 5-127.

Schumpeter, Joseph A. 1946. Article "Capitalism" In *Encyclopedia Britannica*

Smith and Wilson draft

Tirole Jean. 2006.

Yeager, Leland. 1999. *Review of Austrian Economics*.