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Getting Over Naïve Scientism c. 1950: What Fogel and North Got Wrong

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Cliometrics in Historical Perspective:

In Remembrance of Robert Fogel and Douglass North

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I loved Bob and liked Doug. Both men were notably amiable, which means from the Latin, you know, "loveable." Both were easy to love, as you can tell from the many people who did love them. So I can't call them "Fogel and North," not much, not the beloved Bob and Doug.

And both seemed to hold to the gospel of love. Neither of them showed the slightest sign of *hating* their scientific opponents. It is a saintly virtue that anyone with much experience knows is rare among scientists. "That idiot Smith" is the passion fueling many scientific disputes. But not in Bob or Doug, not ever, though Doug and especially Bob were often attacked. They turned the other cheek. Bob would merely amiably direct you to Table 16c. Even at the notorious Rochester conference on *Time on the Cross* he kept his cool in the face of the harshest sustained criticism I have ever seen in science. And Doug, who delighted in being earthy and direct (though a sophisticated, private-school educated francophone), would contradict you with "That's dead wrong, Deirdre," but then offer you one of his excellent wines.

To know Bob well was to love him, unless one was jealous of his success or irritated by his Talmudic disputation (McCloskey 1985) or enraged by his politics (which he described at the height of the anti-Vietnam agitation as "Scoop-Jackson conservative Democrat"; the students and I would argue with him about such a position, concerning Vietnam and Chicago politics, which he would amiably and ably defend over beer and potato chips in the bar of the Quadrangle Club). Bob hired me in 1968 at Chicago and advocated for my tenure there in 1975, so I have a lot to thank him for. Before he decamped temporarily to Unfair Harvard (his Department of History there came to despise him, absurdly, for the "racism" and for imagined scholarly defects in *Time on the Cross*), he would attend every week the Chicago workshop in economic history, which he founded when he came to Chicago from Rochester, and which I as the junior person was assigned to organize during the 1970s. His comments were always tenacious, but genial. With Ted Schultz and Margaret Reid, and a brilliant line-up of his students and mine and Arcadius Kahan's, it was an amazing intellectual experience. It taught me what productive scientific debate is.

I took to describing Bob as "the sweetest, most amiable monomaniac I have ever known." Work, work, work. When the man from the Swedish academy called him in the wee hours of 1993, Bob was not sleeping. He was wide awake, working, working, working. He was a student of Simon Kuznets at Johns Hopkins, and told me once that he worked because he

imagined that at any moment Simon would turn up and ask, with his Russian accent, "Vell, Robert. And vat are you vorking on?" I am familiar with such an imagined goad, in a Russian accent.

By contrast I knew Doug North only distantly if mostly amiably, at conferences and at one visit to Washington, Seattle and one to Washington, St. Louis (someone needs to explain why he favored institutions named after George). Both men, or perhaps their wives, Enid and Elizabeth, who I had thought had some affection for me, seemed to have had difficulty with my gender change. It soured our friendships in the later years.

But who would not *like* Doug, even if one disagreed with his science? I did not disagree at all when he was first turned towards transaction cost, by talking to Steve Cheung and Yoram Barzel at Seattle. Steve and I had been office mates for a year in 1968-69, and he turned me, too. It's why I studied in the 1970s English enclosures and then open fields.

But when Doug started claiming in the 1980s that neo-institutionalism was revolutionary, I did disagree. At first what irritated me (later it was his scientific claims) was the methodological claim of "going beyond neoclassicism," a claim that Doug's close colleague Barry Weingast has recently re-iterated in the *Scandinavian Economic History Review* (2016). I regarded it, and still do, as insulting to the glorious dead (McCloskey 2017). (I am that much of a conservative.) Their claim insulted the long tradition of examining institutional change from an economic perspective – Adam Smith on feudal lordship, for example, or Mill on *metayage*, Heckscher on mercantilism – or Fogel and Engerman on slavery, or for that matter what I consider Doug's best scientific work, productivity change in ocean shipping (this man of many, many parts served in the merchant marine during World War II; and was also a deep-sea fishing buddy of the pop singer Perry Como).

I extravagantly admire what Bob and Doug did in science. And I admire their lives, spectacularly well lived. For example both were former left radicals who radically changed their minds, coming to their senses. It's always disconcerting when a scientist, or for that matter a citizen, never, ever changes her mind. The opposition by American geologists to moving continents for half a century after it was first proposed in 1915 by a German meteorologist is one example. Another – among scores that contradict the cheerful notion that something called "scientific method" always results in the truth coming out, right away or perhaps "eventually" – is the fierce opposition to deciphering Maya glyphs as syllabic writing, maintained for decades by Eric Thompson, the leading Mayanist of his generation. As Keynes is supposed to have said (the sources are unclear on whether he actually did), "When I get new facts, I change my mind. What do you do?" Bob was a paid organizer (well . . . irregularly paid, Enid complained) for the American Communist Party, and Doug in his youth as I understand it was at least a fellow traveler. Bob told me once that when he was a Communist he expected, when the Revolution came, to become the Minister of Education. Always a scholar.

It is notable, for another example (and which should remind us of the important role of Walt Rostow in the birth of cliometrics, unacknowledged because we detested his role in the Vietnam disaster), that the preliminary outline Bob submitted for his Ph.D. thesis to Kuznets' seminar assumed that railroads *were* indispensable, just as Rostow and Schumpeter had claimed. Bob decades later showed me the tattered report, and that's what it said, offering a dozen different ways of measuring the indispensability. During the research, though, Bob changed his mind.

The modal number of times that a scholar or scientist changes her mind on an important matter is zero, and the average is well below 1.00. Bob and Doug were up in the 3.0 or 4.0 range, which I reckon is two or three standard deviations above the mean. Bob believed that railroads were indispensable, then that they were not; that the end of slavery was mainly economic, then not; that religion was causally minor, then not; that all merit resided in model-building history, then not. Doug believed in mercantilist models of regional growth, then not; in neoclassical, static economics, then not. Both believed in Marx; then not.

Splendid scientists, splendid men. As Claude Diebolt and Michael Hauptert did in their own paper in this issue, we can use Bob's characteristic method and pose the counterfactual: What would historical economics look like now without Fogel and North? It would be less, much less. What would Deirdre McCloskey or Claudia Goldin, John Wallis or Richard Sutch have been without these two? I can testify on the point. Everyone who knew them laments that they were not immortal.

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But is their work immortal? That's what we're asking here. I think both men would approve of trying out a hard-nosed, skeptical view of the matter. There are unfriendly as well as friendly things to be said about them. The unfriendly criticism of these two dear old friends of mine might help put their work in perspective, and especially – as both would certainly have approved – help us to see farther by climbing gingerly up on their shoulders and having a look (the hazard is to end up standing on their faces!).

Bob told me once that his principle of choosing topics was never to do anything that would not matter to economic or historical science in fifty years. It's a lofty standard. I was impressed when Bob started, around 1970 as I recall, with Claudia Goldin and Michael Bordo involved (?), a big project on American land policy, *and then abandoned it*. It showed me that even a superb scientist has failures. It wouldn't matter, he judged, in fifty years.

Let us count the ways.

Fogel and North received in 1993 their Nobel Memorial Prize for their advocacy – and practice – during the 1960s and 1970s of quantitative methods and especially of basic economic thinking in the study of the economic past. That *will* matter in fifty years, because by then, though its steady exercise, supplemented by the humanities, we will understand our economic past with a solidity that the fancies of the latest theoretical speculation cannot give.

It is widely supposed, by the way, that the prize to Doug was for his late-career neo-institutionalism. It was not. I wrote the bulk of the citation in 1993, and am here to inform you that it was strictly about the invention of cliometrics. For it, both men deserved the glittering prize (I was deeply alarmed when the Committee asked me to consider recommending the prize to, of all people, Fernand Braudel; I hurriedly scotched the notion). Bob and North were great teachers and great advocates for measurement.

Bob measured, always, always. Sometimes, to make an unfriendly observation, he measured pointlessly. But he always asked How Much, right to the end of his life. Short cuts were not his style. I remember a lunch we had at the Quad Club, in the Solarium (a fancy name for a routine extension of a dining room that to this day is unable to offer dining worthy of the name), during which I proposed summarizing *Railroads and American Economic Growth* in a few lines of logic and fact. It was how I taught the book to students, and thought of its findings

myself. Fact 1: Railroads reduced the cost of transport about 50% on the routes they influenced (a fact easily ascertained in a week or so of research into U.S. transport costs before and after). Fact 2: The routes they influenced in turn were about 50% of the freight routes, considering that river, canal, and coastal shipping remained quite important in the United States even afterwards (again, pretty easily ascertained). Fact 3 (this from someone trained in transport economics by John Meyer – a Doug North student at Washington, Seattle): transport is always about 10% of national income. Conclusion: The rise in national income from the railroads would be therefore roughly $50\% \times 50\% \times 10\% = 2.5\%$. The figure was identical to the conclusion laboriously derived in the *Railroads* book in hundreds of pages of hypothetical canals and of promises of linear programming calculations not delivered.

I didn't offer the back-of-the-envelope proof in the Solarium a hostile way, merely as an instance of Harberger-type estimations of a sort that filled the air at Chicago in the 1970s. Yet Bob (amiably) disagreed. He was not impressed. You had to do the scientific work. Even if the work was somewhat pointless. As Stan Engerman put it, "Fogel's strength lies also in his persistence. He would always push at the evidence as hard as he could to see what arguments could be made. He never accepts the first answer, or the easy one" (*Cornell Alumni* 2012). Maybe, I am suggesting, too hard.

Doug also started his career by measuring, sometimes pointlessly, too. But in his work on institutions he decided never to measure again, nor to use much in the way of comparison, the humanist's qualitative analog to quantitative thinking, to check his notions. It is nowhere recorded how Doug proposed to test neo-institutionalism against actual, non-imagined facts. His much-cited article with Barry Weingast on the Glorious Revolution, for example, would not have had a good time in a 1970s Chicago seminar, especially if Bob the empiricist was present. A deep evidential problem with their glorification of the Glorious Revolution, though not the only one, is that numerous societies – in fact, all of them, or else they are not "societies" but wars of all against all – have rules of property. Hunter-gatherer bands with no kings in sight have such rules. So did anarchic Iceland and the gold mining camps of California. The Code of Hammurabi early in the second millennium BCE did so. English kings, to leap over millennia of strict enforcement of property rights by Chinese and Romans, asserted in the Middle Ages the primacy of royal courts for free men over local and sometimes arbitrary authority. And the barons and the petty lords themselves were bound by traditional law, often bound hand and foot (as emphasized for example in research by the historian and geographer of medieval English agriculture, Bruce Campbell).

No society does well if it does not have such rules, and everyone knows it (except, it seems, economists such as Daron Acemoglu astounded by Doug's insistence that it is an Astounding Discovery). As the prophet Micah (7: 2–3) said in the late eighth century BCE, "The good man is perished out of the earth: and there is none upright among men: they all lie in wait for blood; they hunt every man his brother with a net. That they may do evil with both hands earnestly, the prince asketh and the judge asketh for a reward." Every ordered community since Moses or Solon or Sargon the Great or the First Emperor of China, or for that matter your family or your band or your tribe, has enforced property rights and prevented people from hunting their brothers with nets.

Even giants like Bob and Doug, that is, make scientific mistakes, and in both cases their mistakes became more evident in the decades after 1993. In some ways they are a complimentary pair in their scientific virtues and vices. Bob's late-career studies of health and

welfare, though admirably serious examples of applied economics, right to the end (I heard him at the Business School at Chicago in about 2010 give a brilliant, quantitative explanation of why the USA has such high expenditure on health, and why we should stop complaining about it), were less scientifically pioneering than his work on railways or slavery. Doug's much more influential advocacy – and very much less his practice – of neo-institutionalism, by contrast, was in my mature judgment a scientific error.

Bob realized more and more the salience of ethics in the economy, and even taught (a philosophically unsophisticated) course on business ethics at the B-school at Chicago. Doug drifted further and further from the essentially ethical underpinnings of an innovative economy, speaking of brain science rather than the mind-scanning equipment of the humanities, and led his many followers in the drift.

Right after the Prize I was asked to comment in the Newsletter of the Economic History Association. At that time I thought the main difference was that Bob believed (as I did then: now I am not so sure) that historical economics can best take snapshots. North on the contrary believed that it can best record film feature-length movies (a view that my recent work has been sliding towards). It is the difference between statics and dynamics. "Dynamics" sounds like it must always be better than (mere) statics. But not. The choice is between a sharp black-and-white snapshot of conditions at one time, of, say, the profitability of the Union Pacific railroad, or a necessarily fuzzy if engaging movie in a lightly blurred Technicolor of the march of property rights. Scholarly tastes differ. North has never been happy with snapshots – even before he discovered property rights. Bob, on the other hand, wanted to get the calculation for 1890 or 1860 right, in as fine-grained a way as he can. (Bob was in fact, like his teacher of price theory at Columbia and his predecessor in the richest endowed chair in economics, George Stigler, an inveterate snapper of photos; yet Doug – more parts of the man, and paralleled in Steve Cheung's life – was an apprentice photographer to Dorothea Lange.)

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But in one crucial respect Bob and Doug shared the *same* scientific vice, namely a naïve and unscientific view of Science.

Let's get clear the oddity of the meaning of the word in English. We English speakers should go back to using the word "science" not as "physical and biological inquiries" (which is listed as sense 5b in the *Oxford English Dictionary*, with the first instance in the mid-19th century) but in the old and wide sense of "serious and systematic inquiry." That is what it means in every language except the English of the past 150 years. Thus in Dutch *wetenschap*, as in *kunstwetenschap* ["art science," a recent English impossibility], in German *Wissenschaft* as in *die Geisteswissenschaften* [the humanities, literally to a recent English ear a very spooky sounding "spirit sciences"], or in French *science* as in *les sciences humaines* [serious and systematic inquiries concerning the human condition, such as studies of literature or philosophy or anthropology, literally "the human sciences," another impossible contradiction in recent English], or plain "science" in English before 1850 or so. I have looked into it, and in every language (such as Tamil, Japanese, Korean, Turkish) "science" does *not* mean "physical and biological science only, and especially physics." It names a contrast with casual, unstudied opinion. It means systematic studies of anything, from chemistry to theology.

Thus Alexander Pope in 1711 in his poetical "Essay on Criticism": "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!" He did not mean natural philosophy. John Stuart Mill used the science word in its older sense in all his works. Confining the word to sense 5b – which appears to have been an accident of Oxbridge academic politics about the establishment of chairs in chemistry – has tempted recent speakers of English to labor at the utterly pointless task of demarcating one kind of serious and systematic inquiry from another merely by speculating from the easy chair on its similarities or not to an outsider's understanding of high-energy physics. People doing it fancy they are engaging in serious philosophy. They aren't.

Economists for example have haunting worries about whether economics is "really" a science. Good Lord, dears! If economics were *not* a systematic inquiry – as against casual journalism or unsupported opinion, of which of course there is an ample supply on the matter of the economy – it would be a lot easier to learn and to do. Any journalistic, blogging, amateur could equal Bob and Doug in doing it. Likewise, from another direction, the lofty sneering by physical scientists against social *science* comes merely from the way the word has evolved recently in English. It has no justification in philosophy or science policy or the history of science. I can name numerous sociologists (for example Howard Becker, Mark Granovetter, Harry Collins, Viviana Zelizer) who do better science in every sense of "better" than most physicists trying to avoid being embarrassed about dark matter and dark energy and the long scientific stagnation of high energy physics (for which see Horgan 1996).

We need to stop. It is not true that there is some Secret Ingredient that makes for Science, such as the alleged Scientific Method you heard about in high-school chemistry or the positivist notion (straight from Comte in 1830) of "prediction and control" (*savoir pour pouvoir*). I have been arguing so to economists since 1983. No effect so far. But I'm an optimist. Evolutionary biology, for example, predicts practically nothing, but is a systematic inquiry into the history of life. The big bang (sample size = 1) is not something anybody can control. On the other hand, the invention of oil painting predicts a practice of rendering luscious surfaces, evident in Golden-Age Dutch pictures. And the elasticity of demand implies that we can control by taxation or price minimums the consumption of sugared drinks and the number of very poor people who will get jobs, with a uniform logic of prediction and control.

There is no point in demarcating this or that systematic study as "science" (sense 5b) in order to exclude others, such as anthropology or literary criticism. Systematic studies are systematic studies, good or bad depending on how well they are carried out, ranging from number theory to art history. The only way of judging the value of an inquiry is to read it and judge, if you are so qualified by your own serious inquiries. A field's alleged epistemological status (using criteria having no standing in modern epistemology, such as "countable" or "axiomatized") does not, as one might put it, predict its value. Above all the silliness about Science has set the "scientists" and the humanists at each other's throats, to the loss of science.

Bob and North supply examples. The scientific virtues and vices of both men, I would argue, followed from their views of what science is, in contrast in particular to the humanities. The dichotomy between the humanities and the physical sciences was dogma for scientists (and humanists) of their generation, an echo of the by-then philosophically bankrupt notions of logical positivism c. 1920. Though both men, I have affirmed, were brilliant scientists – Bob a "bench" scientist early and late, Doug lately a theoretical scientist – neither of them was sophisticated in the philosophy or history of science. I suppose they had read a little of Popper and of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), because everyone has. But I

infer from their naïveté about Science that neither had read Kuhn's collection of essays, which is more challenging to the heroic view of Scientific Method, *The Essential Tension* (1977) – not to speak of Michael Polanyi's *Personal Knowledge* (1959), which Kuhn himself put in the pantheon of books on how science actually works, or anything of the Strong Programme in Britain such as Harry Collins, or *Laboratory Life* (1979), or Lakatos' D. Phil. book, *Proofs and Refutations* (1976), or anything at all by Paul Feyerabend.

Bob, I know from many conversations, thought the humanities, such as practiced by his beloved older brother, Ephim ["Ep"], a poet and professor of English at Bob's alma mater, Cornell, were the opposite of the sciences. When Bob heard in the 1980s that I was studying the humanities, he asked me in his amiable way, with no hostile intent, though the question did startle me, whether I was "becoming a mystic." To him you were either a scientist who believed in facts and did not credit the transcendent or you were a mystic who believed in the transcendent and did not credit facts. The commissar or the yogi. In his studies of religion and ethics, Bob half understood the point that the distinction is not very sensible. But he did not drink deeply. Religious belief to him was a datum, an influence on behavior, as he emphasized in his book on the four Great Awakenings in American history. He rested with reducing art and religion to the economist's "taste," about which we should not dispute.

Doug by contrast was more actively contemptuous of religion and the humanities, and again it distorted his science. North 2005, for example, speaks of "non-rational explanations embodied in witchcraft, magic, religions" (pp. 16), "non-rational beliefs. . . belief in religions is an illustration" (18–19"), 30, 40 ("superstitions, myths, dogmas, and religions" (30, 40), "non-rational and supernatural beliefs" (41, 42, 44, 45, 72, 83, 102, 167) and elsewhere, always in repeated incantations. One is led to ask who is "irrational": on the one hand the village atheist with incantations formulated at age fourteen and unamended by any later science as systematic study; or on the other St. Thomas Aquinas, Richard Hooker, or Martin Buber.

In that 2005 book, which I confess that I did not like at all (even more than I disliked North, Wallis, and Weingast 2009), Doug claimed to want to find out how people got ideas. Well, art, literature, philosophy, religion, and their study, called in American English the "humanities," have constituted since the first writings three millennia ago a continuous reflection on how people get ideas. Yet it did not occur to Doug to inquire as a scientist into the humanities. On the contrary, in the book he claimed that it was "brain science," about which it must be said he knew little, which was the way forward in understanding how humans get ideas. Hitch people up to the house current and see what happens.

In this he participated in the recent confusion, spread by brain scientists eager to get tax money for their inquiries, between the brain, a physical organ, and the mind, a human use of the brain. Thinking that brain scans can explain the mind is like thinking that studies of the physiology of the arm can explain Sandy Koufax's pitching or Jackson Pollock's painting. The English poet Andrew Marvell expressed the problem precisely when he wrote in "The Garden" (1681) about how "the mind. . . creates, transcending these,/ Far other worlds, and other seas,/ Annihilating all that's made / To a green thought in a green shade."

Doug, for example, spoke highly of the anthropologist Clifford Geertz. It's hard not to. Doug read Geertz and his coauthors, though, as supporting the economic and anti-mind and anti-humanistic notion that in caravan trade, such as in Morocco around 1900, in North's formulation, "informal constraints [on, say, robbing the next caravan to pass by]. . . made trade possible in a world where protection was essential and no organized state existed." North

misses the non-instrumental, shame-and-honor, non-Max-*U* language in which Geertz in fact specialized, and misses therefore the dance between internal motives and external impediments to action, between ethical stories told at mothers' knees and rules of the game enforced by the monopoly of violence, between the dignity of a self-shaping citizen-not-a-slave and the merely utilitarian "constraints." The toll for safe passage in the deserts of Morocco, Geertz and his coauthors actually wrote, in explicit rejection of Max *U*, was "rather more than a mere payment," that is, a mere monetary constraint, a budget line, a fence, an incentive, an "institution" in the reduced definition of Samuelsonian and Northian economics. "It was part of a whole complex," the anthropologists actually wrote, "of *moral rituals*, customs with the force of law and the weight of *sanctity*" (Geertz, Geertz, and Rosen 1979, p. 137; quoted in North 1991, p. 104, my italics).

"Sanctity" didn't mean anything to Doug, who treated religion with an unlettered contempt worthy of Richard Dawkins or Christopher Hitchens ("Ditchkins," says Terry Eagleton). Religion to Doug meant just another "institution" in his utilitarian, subject-to-constraints sense, that is, rules for an asylum. Religion to him was not about sanctity or the transcendent, not about faithful identity, not about what Sen calls commitment and I call ethics, not about giving lives a meaning through moral rituals. It was certainly not an ongoing intellectual and rational conversation about God's love, not to speak of an ongoing conversation *with* God. Religion was to him, as it was to Bob, just another set of constraints on doing business, whether the business is in the market or in the temple or in the desert.

In this Doug agreed with the astonishing economist Laurence Iannaccone and his followers when they come to study religion—religion to them is a social club, with costs and benefits, not an identity or a conversation. Anyone who has actually belonged to a social club, of course, knows that it soon develops "moral rituals, customs with the force of law and the weight of sanctity." (Iannaccone, actually, who is a friend of mine, is more sophisticated than his theory: he does not carry the theory into his own church, and has worried deeply about the limits of the Beckerian model he employs.) I could instance as such a religious club the Chicago School of economics during its salad days in the 1970s. One of our sanctified rituals was to intone *De gustibus non est disputandum*, while passionately advocating a very particular intellectual *gustus*, according to the holy writ of Becker and Stigler 1977. Doug asserted, for example, that in a prelegal stage "religious precepts. . . imposed standards of conduct on the [business] players" (North 1991, p. 99). He spurned the worldview that goes with religious faith. His own religion of Science, of course, is in fact nothing like a mere constraint. He construes it as his identity, his moral ritual, his sanctity—in short, the meaning of his life, negotiated continuously over its extraordinary course. But consistency in ethics is not the strong suit of Samuelsonian economics.

And yet both men were thoughtful—just not thoughtful in a systematic, humanistic way. They relied on instinct and taste. Thus an interview with Bob reported that "ultimately, Fogel says, his expertise is not in crunching numbers but in finding the right numbers to crunch." Precisely.

But why should we as economic scientists care about the humanities? Here's why. Meaning is scientific, and science cannot be done without human meaning. Piling up "existence" theorems and "significant" results is meaningless. Counting something you should not or cannot meaningfully count is bad science.

For instance, one can argue that measuring the balance of international payments, besides being hard to get right, is mischievously meaningless. Trade happens between John Smith of Iowa and Tasuro Ichichi of Japan, not between imagined entities called "the United States" and "Japan." To raise the balance of payments as a "problem" is to cause, for example, Britain's gormless stop-go economic policies in the 1960s and 1970s, or to arouse nationalistic, xenophobic, and in particular anti-East-Asian racist worries about trade with Japanese in the 1980s and trade with China in the 2010s.

Measuring "happiness", to give another instance, which can't meaningfully be measured, is mischievous, too (McCloskey 2012). It leads on to "nudging," and shortly to taking over other people's lives by experts in law schools and departments of economics. As the great (American-definition) liberal Lionel Trilling wrote in 1950, the danger is that "we who are liberal and progressive know that the poor are our equals in every sense except that of being equal to us." And in another essay, also quoted by the professor of English James Seaton in his own far-reaching book in 1996, Trilling wrote that "we must be aware of the dangers that lie in our most generous wishes," because "when once we have made our fellow men the object of our enlightened interest [we] go on to make them the objects of our pity, then of our wisdom, ultimately of our coercion." Every nurse or mother knows the danger. And when she is a lover for the beloved's own sake, she fends it off.

You can't do good science without meaning. Bob and Doug often forgot this. Unlike the physical and even the "positive-economics" end of the social sciences, the humanities – such as literary criticism in the Department of Literature, and number theory in the Department of Mathematics, and transcendence in the Department of Theology – study meaning. In particular they study humanly meaningful *categories*, such as good/bad, lyric/epic, 12-tone/melodic, red giant/white dwarf, hominid/Homo sapiens, God/not, prime/not, smooth peas/wrinkled peas, exist/not.

Such humanistic and human categorization is a necessary initial step in a scientific argument intent on measurement. 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. Some categories and procedures are helpful and wise, some misleading and stupid. Thus the great Latin scholar and poet A. E. Housman delivered a scathing lecture as president of the Classical Association in 1921 called "The Application of Thought to Textual Criticism," in which he assaulted the thoughtless application of rules to improve the copied texts of Latin manuscripts. The humanities, and the thoughtful humanistic steps in any science, study such questions, offering more or less sensible arguments for a category or a procedure being wise or stupid. The American Statistical Association has recently, for example declared tests of statistical significance without a substantive loss function stupid (ASA 2016).

In 1910, for another example, many economists and other scientists believed that the category "Aryan race" was helpful and wise in thinking about the economy and the society. It was, we later decided, a misleading and stupid and even evil category, though at the time most scientists, such as the great English statistician Karl Pearson, thought it was not. Around 1910

the American Progressives, especially the leading economists among them, believed passionately in racism, and advocated policies such as immigration restrictions and the minimum wages to achieve eugenic results in favor of the Aryan race (Leonard 2016). In 1925 Pearson published an article in the inaugural number of the *Annals of Eugenics* recommending that Ashkenazi Jews be forbidden to immigrate to Britain because they had low IQs and dirty clothing. Similarly, in the 1970s there was a great kerfuffle about Black IQs. No one seemed to stop to ask whether it was ethical or policy-useful in the first place to do such measurements of difference (set aside the genetic implausibility of African-origin folk having lower general intelligence than Europeans). What? Should we shoot all the Blacks? Segregate them in camps? If we give them, more kindly, extra education to bring them up to the glorious intelligence level of Europeans (nationalism, socialism, national socialism, eugenics, World War I, World War II, the Holocaust, the atomic bomb), why not to others, considering that there is massive overlap in the measured IQs? How to deal with superior Blacks, such as John Hope Franklin or Thomas Sowell?

My point is not anti-quantitative. Bob and Deirdre and early Doug throughout recommend measurement. But the measurement needs thoughtful framing using the humanities. One of the more inflammatory of the (I repeat, idiotic) charges of racism against Fogel and Engerman was a reaction to their coolly executed measurement of how many times slaves were beaten, concluding that it was not very often. What Fogel and Engerman were trying to say, and did, is that the lash was not anything like as commonly applied as abolitionists claimed. A worthwhile point, I suppose. But of course if you yourself were beaten "only" a couple of times a year, say, you would object, and would be made an object. Humanistic inquiry could frame such a calculation better.

Nor is my point anti-mathematical. We need more mathematics in historical economics, not less. Yet what we need is mathematics meaningful in the actual economic world, such as Fourier series and general-equilibrium simulations and fuzzy logic. Not their *proofs*, which alas is what most graduate programs teach nowadays, having abandoned economic history as a requirement and thereby killed off interest in actual as against stylized facts. If you prove that on such-and-such axioms "there exists" a competitive equilibrium you have offered nothing of scientific value. I knew the mathematical economist Frank Hahn slightly. To my challenge that general equilibrium existence theorems were useless as political economy he replied that, if he could show how very many and strange were the conditions necessary for *perfect* competition, he could show "why Margaret Thatcher was wrong." Hahn was lingering in the world of thoughts without content, for an ethico-political purpose, yet not thinking seriously about his ethics or his politics, merely his proof idea (which is itself, I admit, a humanistic exercise — existence-theorem mathematics *is* part of the humanities, and is useless for a policy science, which must ask in cliometric style substantively How Much).

The necessity of the humanistic first step, observe, applies to physical and biological sciences as much as to *les sciences humaines* or *die Geisteswissenschaften* or plain Italian *scienza*. Meaning is scientific. 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."¹ We. Humans. Say. With words. About *geisteswissenschaftliche* categories the German-

¹ 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 Siggard Jensen

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.”² Truer words were never spoken. We dream of categories, in our metaphors and stories, and with them make our lives, especially our scientific lives saying the world. There is nothing scary or crazy or French or post-modern about such an idea. The “hardest” sciences rely on human categories.

Yet if you are making a quantitative point, as must happen in a policy science like economics, then after the humanistic step you must proceed to the actual count. Then perhaps you can prove Margaret Thatcher wrong. The purpose of a scientific assertion is to change minds. But it is mischievous to seek to change minds merely by the “insight” that is imagined to come from pure theory, such as the numerous solution concepts in game theory, unless you have checked the theory against facts of our world and shown that the insight is quantitatively important. Otherwise we are liable in economics to be misled by our political passions, as Hahn was. Such is the merit of numbers, as Bob and Doug affirmed. If you know that real income per head has risen in Italy since 1800 by a factor of about thirty, then your political impulse to condemn “capitalism” as impoverishing is at least disciplined. You may continue to be a socialist, but you will have to sharpen your argument in some other way than going on and on using the same false and quantitative claim of impoverishment.

In short, historical economics needs to get beyond the unfortunate dichotomy of fact and value, science and humanities, theory and metaphor. A naïve scientism has had a long enough run. It served to inspire two great scientists. Good. But in the way of unexamined philosophies it limited them and their findings, unnecessarily.

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(quite incomplete)

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informs me, it was something like “*Fysik er ikke om hvordan verden er, men om hvad vi kan sige om den.*”

² *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.*

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