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Apologia Pro Vita Sua: A History of My Economic Opinions

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By "*apologia*" for my life in economic and historical science I do not mean "I am sorry" – though, Lord knows, any life provides numberless opportunities for regret, and certainly requires "apologies" in the way the word is now used in English (only: the Romance word for "sorry" is as in Italian *scuse*, and the Germanic as in Dutch *verontschuldiging*). I mean instead what in 1864 John Henry Cardinal Newman (1801 – 1890) meant, in using the Latin-from-Greek word in the title of his spiritual autobiography, a "defense" as in a court of law, against the charge of changing his mind. Newman was raised as a Protestant Anglican, becoming at Oxford what is known as "High-Church," and then explicitly "Anglo-Catholic." And finally he converted to Catholicism, and at length Leo XIII made him a prince of the Church, and in 2019 Pope Francis made him a saint.

I am no princess of any church, and certainly no saint (though by the way in 1998 I became a decidedly *Low*-Church Anglican, changing my mind, too, about the agnostic scepticism in which I had been raised). But like Newman about the church, I have changed my mind about economics, and repeatedly. I was raised as a Democrat and then became a soft socialist and then a Keynesian and then an economic engineer and then gradually a classical liberal economist, though never what could be called a European-style conservative. And finally I became an exponent of the humanities in economic science, though continuing to look with favor on numbers. Raised up at Harvard as a student in the 1960s to believe that *only* numbers matter, I gradually realized in the 1980s that words matter, too, at least for economic science itself. And then in the 1990s I started to realize that words mattered also for the actual economy. Raised up in the 1970s as a young Chicago-School professor to believe that prudence understood as the prudent maximizing of utility is all you need for economic science, I gradually realized in the 2000s that faith, hope, love, courage, temperance, and justice matter also, and not merely as entries into a prudential utility function as *F, H, C, T, J.* Then in the 2010s I realized that such a human science suffices to explain liberalism and modern economic growth.

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My colleagues who are puzzled or irritated or outraged by this history of changing one's mind are entitled to an *apologia.* (And maybe an apology, too. So here it is, as the politicians say: I apologize if I have ever offended you.)

Perhaps something can be learned about the reasons to arrive, as I finally did, at an economics with the humans left in, a "humanomics," one that does not abandon mathematics or statistics or experiment or observation, but considers, too, the categories, comparisons, and human meanings revealed in language.[1] Maybe something, too, can be learned about how scientists change their minds, the "rhetoric" of science. Newman spoke in 1870 of a "grammar of assent," quite contrary to the official, pre-Kuhnian account of scientific rhetoric as axiomatic deduction and hypothesis rejection. What Cardinal Newman said about the 19th-century's understanding of logic and testing can also be said of its descendant, the 20th-century's positivist method – for example, the axiomatization and econometrics that I was trained in. Newman observed that such a method is "loose at both ends" (1870, 272). The mechanical method summarized in 1957 by Tjalling Koopmans's *Three Essays on the State of Economic Science* requires undefended premises at the one end, and at the other end does not arrive by itself at useable conclusions for actual science or actual life. A high-brow example in economics is the Lucas Critique of macroeconomic policy. One estimates econometrically how much inflation rises when the Fed increases the money supply. But the human meaning of the policy can change as a result of the very increase, making the estimate on the basis of past meanings meaningless. Lucas called it "régime change," but didn't draw the correct conclusion – that mechanism must yield to a broader humanomics, tightening the looseness at both ends.

As the literary critic Wayne Booth formulated the matter in *Modern Dogma and the Rhetoric of Assent*, a complete practice such as economic science depends on a rhetoric of assent, a practical wisdom (*phronēsis,* said the Greeks), at both ends, rather than merely the mechanical scepticism (if regularly feigned) about the middle bits. Booth's rhetoric of assent and Newman's grammar of assent constitute, Booth wrote, an "art of discovering good reasons, finding what really warrants assent because any reasonable person ought to be persuaded by what has been said" (1974, xiv; cf. Booth 2005). To express it in a way any economist will understand, Newman in 1870, Booth in 1974, and McCloskey in 1983 came out for Bayesian inference. Whether in science or in life, for any actual human action you need beliefs (and their warrants) about priors, and then you need judgments (and the courage for decisions) about posteriors – at both ends. For example you might assent (or not) to a proposition that God does not play dice with the universe, or that governmental coercion is an imperfect substitute for mutually beneficial deals. The middle bits in such assents, or dissents, though necessary, and lovely, and dead easy to teach, do not suffice.

[1] Bart Wilson coined the term. See Smith and Wilson (2019) and McCloskey (2011).

And then it turned out that all this led to a humanistic economics, which further turned out to have a killer app – a true scientific explanation of the nature and causes of the wealth of nations.

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It's an advantage for intellectual development to be a little stupid, or at best earnestly naïve, as on both counts I am. Natural economists – natural because their personalities make it a snap for them to grasp how a maximizing person would behave – find it hard to develop. They are too smart at the outset. They get it immediately, and then keep it, forever (cf. McCloskey 1992). On the contrary, I am not a natural economist. It was hard for me to learn economics of the Samuelsonian and Friedmanite and Koopmanslijk sort. It didn't fit my personality or ethical upbringing. But I declare on oath here that at each stage of learning, from child to old lady, I was stupidly, naïvely sincere, believing earnestly in each of the half-truths grasped along the way. Really.

My little witticisms, to which I am addicted, give people sometimes the opposite impression, of an insincere ironist of the sort the great Joseph Schumpeter was. He was unable to quite let go of his early (and sound) understanding of markets, or then of his youthful (if disproportionate) admiration for a Marxian sociology, or then of his mature (if premature) findings on economic and intellectual history, all of them busily making ironic little jokers about each other throughout his book, *Capitalism, Socialism, and Democracy* (1942).[2] In my case, no, believe me: I am a serial believer. Contradictory as are the successive economic opinions I have held, I was stupidly loyal to each.

To begin at the beginning, all of us begin with what Schumpeter called one or another "pre-analytic vision" even about the economy. Another expression would be "pre-judgments," "prejudices." I used the word, so to speak, without prejudice, because we cannot but have them, for good or ill – those Bayesian priors, and then the human judgments about the resulting Bayesian posteriors. They are the rhetoric with which we say the world, in physics or in theology or in economics. There is no view from nowhere. The prejudices with which a child or a scientist views the economy are bundles of metaphors, narratives, interests, viewpoints, conventions, ethical evaluations, empirical pre- or post-judgments that necessarily start and help finish any thought about the world. They are provided at both ends largely by the arts and humanities, the latter being what German speakers call *die Geisteswissenschaften*, "the spirit sciences," which therefore are essential to any science, *physisch* or *sozial.* In physics for example one might start – it has been amazingly productive – with the prejudice that the world is mathematical (cf. Wigner 1960).

[2] The evidence is given in McCloskey (2020).

In economics one might declare at the start that the economy is an input-output machine, a production function. Or one might declare that it is a system of exploitation. Or that it is Mama's grace. We all have such prejudices, what Kenneth Boulding (1956) called images. I did have them – and now do, though different ones than the ones I had as a child or as a young economist. The task is to get better ones, in which case our sciences come out better. When the prejudice of an American geologist was that continents were stable, he could not explain mountains, rather a serious flaw in such a science. When the prejudice of a Mayanist was that the carved glyphs were decorations, she had no chance of coming to read them. When the prejudice of a German astronomer was that what we now call the Milky Way galaxy was the whole universe (contrary to Kant's prejudice on the matter), he could not come to see that the smudges called nebulae were some of the 200 billion other galaxies. When the prejudice of an economist – and we are all at least amateur economists if we have any thoughts about the economy – is that trade is zero sum, she cannot explain mutual advantage, much less measure it.

A very young child, I do recall, views the getting of goods and services as manna from heaven, or more exactly from Mama. Some adults, and even some adult economists, cling to such an ur-vision. The anthropologist Alan Page Fiske (1961) argues persuasively on psychological and cross-cultural evidence that children develop in succession four "elementary forms," which then haunt the minds of adults.[3] The earliest is communal sharing (you get meat because you are part of Our Crowd, or of My Mama's Family), then authority ranking (I am the chief, or the father, so I get more meat: see Terrible Twos for early instances), and then (part of a child's earliest memories at about age 4 or 5: "Play nice, Bobby") equality matching (we're all in this together, so let's make the amounts of meat exactly equal for everyone).

None of these first three ethically approved forms of allocation involve prices, that is, exchange rates between two different kinds, meat for milk, say, or arrow points for cave paintings. Fiske says that the fourth elementary form, "market pricing," seems ethical only when a child is about eight years old, exchanging my frog for your jackknife. Some adults never get over their pre-eight-year-old disapproval of exchange, and cling to the elementary forms one through three. A baseball commentator said of an exchange of players between teams, "Getting four hot prospects for an old player is a win." The other commentator replied, using the wise cliché in justification for market pricing, "Well, I call it a win-win."[4]

The pre-judgment of unequal exchange, of one side winning and the other losing, of the sports-and-business talk of nations "competing," runs through the history of

[3] Fiske cleverly points out that the succession of the four corresponds in the theory of scaling to categorical scales (in/out), ordinal scales (higher/lower), interval scales (same amounts), and ratio scales (such as Fahrenheit temperature). My friend Rick Wicks of Gothenburg University put me onto Fiske.

[4] The phrase win-win was coined in the 1920s by the management theorist Mary Parker Follett.

economic policy like original sin. It came out recently in Peter Navarro, former President Trump's advisor on foreign trade, picked by Trump because Navarro – a Harvard PhD, for shame – fed back to him Trump's mafiosi-style prejudice that exchange is cheating backed by coercion, an offer you can't refuse. The economists of unequal exchange from Friedrich List to Raul Prebisch to Peter Navarro never get to the pre-analytic vision of the eight-year old, of win-win. And even some of the best among us retain an inchoate feeling that exchange is hurtful and corrupting. I have a dear friend, a brilliant Marxist economist, who says, "I *hate* the market!" I reply, "Jack, you don't hate the market. Your home is filled with lovely antique furniture you have acquired through the elementary form of market pricing." "I don't care: I *hate* the market" (Amariglio and McCloskey 2008). Like the Wizard of Oz, a pre-analytic vision runs the show from behind the curtain.

At age 16 I agreed with Jack's feeling, and viewed myself as a socialist, admittedly unlike Jack a decidedly unlearned one. I happened in 1958 to devour in the Andrew-Carnegie-financed public library of Wakefield, Massachusetts the Russian prince Pyotr Kropotkin's *Mutual Aid: A Factor of Evolution* (1902) and the gullible American journalist John Reed's *Ten Days That Shook the World* (1919). If I had instead come across Rose Wilder Lane's *The Discovery of Freedom* (1943) or Ayn Rand's *Atlas Shrugged* (1957) I suppose I would have gotten a better grasp of market pricing, earlier. Many market-loving classical liberals came to liberalsim by that free-market path, and were never socialists. Yet the socialism-to-liberalism route is very common in 20th century political biographies, such as Leszek Kołakowski's or Robert Nozick's or, to descend a couple of notches, D. N. McCloskey's. (The contrary route from market liberal to state socialist is vanishingly rare.) The old joke is that if you are not a socialist by age 16, you have no heart. If you are still a socialist at 26, you have no brain. (I adjust the ages.)

Why has socialism kept its appeal since 1848 to every new generation of young people, including mine, in the face of repeated disasters in its application? ("Our socialism will be different," the kids declare, "because we are pure of heart.") Well, for one thing, we all grow up in families, which of course must be little socialist communities, from each according to her ability, to each according to his need, communal sharing. Friends are that way, too. Erasmus of Rotterdam started every edition of his compilation in the early 16th century of thousands of proverbs with "Among friends, all goods are common" (1500–1533). That's right. If you buy a large pizza for the party, but then declare, "I paid for it, so I get to eat it all," you won't be invited back. Market pricing be damned: among family and friends it's communal sharing or equality matching, and, in political cases backed by coercion, sometimes authority ranking, all the way down.

Therefore, when a naïve adolescent in a theoretically non-hierarchical society, such as I was, discovers that there are poor people, her generous impulse is to bring everyone into her family – a family of, say, 330 million souls, a social democracy. She would not have such an impulse if she had been raised in a hierarchical society,

whether aristocratic or authoritarian, in which hierarchy is naturalized. Aristotle, the tutor of aristocrats in a slave society, declared that some people are slaves by nature. And the fictional pig/commissar in Orwell's *Animal Farm* declared that all animals are equal, but some animals are more equal than others. Under "capitalism," man exploits man; under socialism, it's the other way around. The literary critic Tzvetan Todorov reports that Margarete Buber-Neumann (Martin Buber's daughter-in-law), "a sharp-eyed observer of Soviet realities in the 1930s, was astonished to discover that the holiday resorts for ministry employees were divided into no less than five different levels of 'luxury' for the different ranks of the communist hierarchy. A few years later she found the same stratification in her prison camp" (Todorov [2000] 2003, 83).

The other reason socialism is attractive to 16-year olds in a modern economy, and another reason we will have to keep refuting it with each new generation, forever, is (as the economist Laurence Iannaccone (2018) argues) that the more complex an economy becomes, and the further people are from work with some obvious fruit, being further and further up the astonishingly long supply chains of a modern economy, the less obvious becomes the link between effort and reward, and especially their social good. Close up, the economy looks to a modern teenager more and more like arbitrary manna from heaven, to be "distributed," as the unhappy choice of words has it, among members of a family. In the 1950s it was my condition, a teenage child of a Harvard professor, whose money dropped on the family like manna. To a person embedded in a large company, and still more in a governmental office, or a monastery, or a graduate school, and most of all in a loving middle-class family insulated it seems from the market, it's back to communal sharing.

By contrast, a person, even a 16-year-old person, who works on a family farm or in the family's small shop has no trouble seeing the connection between effort and reward in the society as a whole, and its advantages. My students like that can grasp economics with ease. They grasp the basics of scarcity and specialization and the win-win of exchange. I have friends who grew up on dairy farms, twice a day milking cows to sell the milk, 365 days a year, no breaks. They know effort and reward and the market, and make good economists and very good workers.[5] St. Paul of Tarsus had no trouble seeing it in the little economy of Thessalonian Christians: "If any would not work, neither should he eat" (2 Thess. 3:10). Such a rule, so contrary to the recent construal of Christianity as a sweetly distributive socialism, is the only way in anything but a highly disciplined or greatly loving small group can get a large pizza made in the first place "for distribution." But the modern middle-class child doesn't realize it, and if he spends his life in a large organization up along the supply chain it is no wonder that he arrives at age 26, and then at age 76, not understanding economics, and favoring socialism. Senator Bernie Sanders and the British Labour Party leader Jeremy Corbyn were, like me, born in 1942. In 1958 we had the same opinion about

[5] I instance a colleague at the University of Chicago in the 1970s, D. Gale Johnson (1916–2003), and my friend Bart Wilson (1969–) at Chapman University, and outside of academic life my friends Chris Vinyard of Virginia and George Dunsmore of Vermont.

"capitalism:" overthrow it. Getting beyond such callow if emotionally satisfying opinions is difficult. The American novelist Saul Bellow said of his early Trotskyism, "like everyone else who invests in doctrines at a young age, I couldn't give them up" (1994, 308).

In 1960 socialist leanings went along with folk music, to be amplified shortly by the worldwide upheavals in the late 1960s, and especially the Vietnam War. I came from what was known to us bourgeois youths as a "good" draft board – a declining New England mill town with plenty of young working-class youths to be sent to die in Vietnam, while the son of a professor was never going to go. It added guilt to my opposition to the War, and my singing against it. (Some of us said eventually to our Trotskyist selves, "Hmm: government is incompetent even at its most particular task, war. Interesting. Any implications for economic policy?") I was never a good musician, but learned to strum a guitar and sing with passion the leftish songs of struggle. Arise, you prisoners of starvation. I know more labor and protest songs than most of my leftish friends. If the economy is about distribution, not about scarcity or effort or allocation or creativity, and if the bosses have piles of gold in the back room, then anyone with a heart will view the virtuous task as a struggle to extract more gold from the bosses, to give it to the workers. The bosses have enough. This's the economic theory that underlies policies for improving wages or working conditions by magical statute. Get more of that endless gold, by law: make employers pay for safety, long holidays, health insurance, higher wages, anything, without limit. Zero sum.

Work in my family was praised, though. My family and my experience as a teenager gave dignity to physical work, as suits a democracy, and as suited a socialist folk singer, and which most fortunately kept the later economist from ignorant snobbishness about laborers as slaves by nature. When the family in 1959 went to England for my father's sabbatical, I worked for a time as a laborer making hay on a farm in Wiltshire, drinking pints and pints of Ludlow beer in the pub with my English army mates, playing tennis on the manor-house lawn weekends with the daughters of Lord Tryon, Treasurer of the Queen's Household: in brief, I learned the British class system, and despite it began a lifetime love affair with Britain. For the short run such knowledge of class confirmed my socialist leanings.

Back home, my mother, an actress and opera singer and then a frustrated housewife, did all the labor in the house, from sewing and cooking to mowing the lawn to demolishing non-bearing walls. Dad read the paper. In that respect I took after my mother, as a good physical worker. But of course work is work is work, as the economist's metaphor of human capital avers, and so the lessons of physical work apply to intellectual work, too. I once heard a paper called "Writing on the Bias" by a professor of writing saying that she learned to write expertly by watching her mother sew expertly, cutting cloth "on the bias" to give the skirt graceful movement (Brodkey 1994). Measure twice, cut once, check and tighten, and get the job done.

My mother's father was an electrical contractor. I am the only person in most rooms who can become an apprentice electrician in the state of Michigan, because my

grandfather Fridtjof, my uncle Joe, and my cousin Phil were all members of the union there, which is the only way you get into it. (And yet Democrats are indignant when an economist notes that trade unions are cartels; the economist gently replies that maybe they are a good thing, sometimes, – I myself joined a union of professors in 2014, in protest at the nitwitted stubbornness of the University administrators – but anyway, she continues, relentlessly, they *are* cartels, raising the condition of one part of the working class at the expense of the rest.) I joined briefly at 17 the National Maritime Union, dreaming of going to sea in summers during college, until I learned that even under the protectionist Jones Act the only jobs for American able seamen (and I could barely tie a square knot) were garbage scows in Boston harbor. My first paid job after the English hay making was washing pots in a restaurant. I was so earnestly energetic that they offered me an apprenticeship as a cook. Sorry, thanks, no, I have to go to Harvard College. Teenagers are often prevented from working nowadays by the statutes inspired by trade unions and the theory of the boss' gold. But for the teenagers to work at menial jobs is good, especially if they are middle class and are going to be economists. Donald Boudreaux, the distinguished trade and legal economist, whose father was working class, notes wittily that at 18 his comparative advantage was indeed … bussing tables.

My regular summer job in four summers during college was as a laborer on the highway department of my home town north of Boston. I learned that working men are not idiots, something the bourgeoisie needs to learn. People who take their daily showers in the morning before going to the office need, as earnest democrats, to admire also people who take their showers in the evening, after getting dirty on the road crew. I especially admired my foreman, Glenn from Missouri. The Massachusetts men made fun of his Southern accent in the way of open ethnic nastiness in the 1960s, but he knew his craft, and taught what he could to the bourgeois summer boys. When I stop now and watch a man on a road crew raking asphalt, he probably thinks that the old lady is admiring his physique. No, she's examining his technique.

> [Meanwhile I had a happy childhood, though like Churchill, Marilyn Monroe, and Joseph Biden I stuttered, quite badly. As the decades wore on in an occupation requiring a lot of talking I got better, until by now in my eighth decade I am nerveless about interviews and speeches – though I still stutter on the word. . . "stutter." I had all sorts of cultural advantages, so it was morally healthy for me to have a serious handicap.
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> My father was distant but genial, absorbed in his own intellectual life, and playing billiards at a high level. I was never any good at billiards, but he taught me poetry, of which he had at his command hours and hours of recitation, from Keats down to amusing schlock like "The Cremation of Sam McGee." My mother was deeply loving of all her three children, which gave each of us an irrational self-confidence. Her own mother, though a very good grandmother, was not loving to her daughters, imparting an irrational lack of confidence to an extremely gifted singer, actress, and at length poet.]

I came to Harvard College in 1960 intending to concentrate in history, because as an avid childhood reader I had come to love the glorious dead, the more dead the better. Still do. But I learned that the study of history required reading many long and

tiresome books (of the sort I now write: irony upon irony). In those days what Peter Novick (1988) later chronicled as That Noble Dream of objectivity ruled in American historical scholarship. (Now of course, especially in U.S. history, it decidedly does not.) Objectivity, whether or not it was actually achieved in the age of the Cold War, did not at all suit a young social democrat – my first vote was in 1964 for Lyndon Johnson against the "conservative" Barry Goldwater – and so I looked around for another concentration. My father was a professor of political science at the same institution, and I could not therefore shift to what would have been natural for implementing my lifetime passion for raising up the working class (by bossing them around as I then thought; or by leaving them to get on with it, without the war on drugs or occupational licensure, and with an effective and dignified hand up, as I now think). Then I came across Robert Heilbroner's *The Worldly Philosophers: The Lives, Times, and Ideas of the Great Economic Thinkers* (1953) and was deeply charmed, as many budding American economists have been. Here was a way to raise up the poor directly, by fixing the economy. In my second year I became an ardent student of economics, and have been ever since. I realized that I needed mathematics to pursue it, and so took two years of calculus, though never going quite far enough. For what later became important in economics, I should have soldiered on through real analysis, the course (though pointless for actual science) that concentrators in mathematics take in their second year.[6] Yet I had liked books *about* the history of mathematics such as Bell's popular book *Men of Mathematics* (more of those dead people), later studying the history and philosophy of mathematics more seriously, and acquired at least a conception from the outside about its higher studies.

Another source of news from higher mathematics was one of my two college roommates, David, who was a brilliant electrical engineer. David introduced me and my other roommate, Derek (who was better than I was at mathematical thinking, and at music) to items like Gödel's incompleteness theorems and Norbert Wiener's cybernetics and Claude Shannon's information theory. David also learned the banjo, and he and Derek of many instruments and I would leave off studying late of an evening to belt out labor songs. It all fit with the economics that Derek and I were being taught – Keynes and social engineering to help the poor, by running their lives. Derek and I believed Keynes's lofty assurance in 1936 that an economist is "in a position to calculate the marginal efficiency of capital-goods on long views and on the basis of the general social advantage" (1936, chapter 12, section viii). Joan Robinson (1903–1983) was a young colleague and the greatest follower of Keynes. It was Robinson's judgment that "when large scale adaptions have to be made, central control is much

[6] In an early chapter of his notoriously difficult freshman physics course at Cal Tech, the Nobel physicist Richard Feynman told the kids that they needed to learn some matrix algebra, and might as well see the simple proofs involved. Then he wrote, defensively, "What is [proof-oriented] mathematics doing in a physics lecture?" (Feynman 1963, vol. I, p. 22–1.) His rhetorical question – why proof? (he said "how various mathematical facts are demonstrated") – would startle an economist who has learned her mathematics outside the departments of physical science.

more flexible" ([1946] 1978, 27) than private enterprise. It "takes twenty years for an industry under private enterprise to readjust itself to a fall in demand," quite unlike the gratifyingly flexible nationalized industries then forming under Labour. To an old liberal nowadays their brisk statism seems, as the British would put it, daft, loony, bollocks, bonkers, stark raving mad – and very much *not* helpful to the poor. But it was highly flattering to young American economists just learning such magical ideas as the multiplier and the creation of supplies of goods and services out of demanding them. Derek and I, the official students of economics in our room, scorned David's reading during breaks from second order differential equations of Ludwig von Mises's *Human Action* (1949), the so-called "Austrian" economics focusing on creativity – which decades later I came to admire. Had I picked up the book and read, instead of sneering at it from a lofty statism of long views able so easily to discern the general social advantage, it would have saved a lot of time.

When in 1964 I shifted from Harvard College to Harvard graduate school in economics I was still supposing that my classmates and I would go down to Washington and, as the contemporary if daft phrase had it, "fine tune" the U.S. economy, or with still greater ease the Indian economy. The radio dial for tuning would be input-output analysis, which I had used to write my college fourth–year thesis. The thesis, which I have patented as a sleep aid, was not a good piece of work. I learned how to do economic research correctly by doing it at first incorrectly, and then having at least the minimally good taste to correct it. Input-output analysis is the businessperson's metaphor of a "supply chain" generalized to the whole economy. That a supply chain is called a "chain," as though there are no substitutes for making iron or ice cream *this* way, is what is wrong with it, and therefore with the input-output analysis that I applied for deciding between trucking vs. railroads in far India. (About India I knew very little except beloved stories by Kipling – "Do you like Kipling?" "No, I don't kipple" – and the sweet myths of planning that the Brahmins were articulating during those days, five year plans and all.) Why an undergraduate at Harvard would be good at "deciding" between Indian transport systems was part of the master myth of fine tuning. A snap: just consult your largely imaginary input-output table, with no substitutes, no human creativity, just top-down planning by Brahmins and Keynesians. It is a high-handed attitude towards economic development that I would some years later turn decidedly against, on scientific and ethical grounds. I was by 1970 listening to Theodore Schultz at the University of Chicago, and applying his vision of sensible peasants to the English Middle Ages. To honor the poor of India, or the glorious dead of 1300 CE, you have first to stop thinking of them as gormless idiots.

Input-output analysis was a proper subset of a movement in economics from the 1950s to the 1970s known as "activity analysis." It was linear algebra applied to the economy, laboriously translating the smooth-function analysis of production by Knut Wicksell in Sweden and John Bates Clark in the U.S. into straight lines and fixed coefficients. So, "a marginal product of zero" was translated into "a slack variable." In Cambridge, Massachusetts it came to its adolescence in a 1958 book co-authored by a teacher of mine, Robert Dorfman of Harvard, and two economists at MIT whom I

should have been a student of, Paul Samuelsson and Robert Solow – though metaphorically all modern economists have been their students. In Cambridge, England the linear approach was older, dating back to the Marxist contemporary of Keynes, Piero Sraffa, finally delivering in 1960 his little book, *Production of Commodities by Means of Commodities.* Linear models had become characteristic of the brilliant Marxist group at Cambridge led by that same Joan Robinson, who by the 1950s was a Maoist and went about in Chinese outfits.

Anyway, I was taught activity analysis. Many of my classmates never recovered. Yet shortly after I learned it, it became obsolete, providing a vivid example, an early one among many I have watched and a few I have been invested in, of what the philosopher Imre Lakatos (1970) called a "degenerating research programme," a piece of economic science fashionable in its brief day and then dead. Science necessarily has lots of these, or else scientific research would be improving at explosive rates we do not in fact witness (note the similarity of such logic to economic entry/exit and Darwin's use of it). Some economic dead ends were, in sequence from the 1950s to the 1980s: monopolistic competition (I was among the last undergraduate students of its inventor, Edward Chamberlain), original Keynesianism (which I learned, though always vaguely puzzled by its magical quality), abstract general equilibrium (which I learned, though always vaguely puzzled by how proof could lead to fact), monetarism (which I leaned at Chicago, though finally realizing that it was true only for the world as a whole), rational expectations (ditto as to where I learned it, admiring its microeconomic character, if finally seeing at Iowa that its econometric implementation was hopeless), and then later many others, down to the on-going degenerating research programmes of non-cooperative game theory (when humans are cooperators, not Trumps) and behavioral economics (when humans live in wide markets, not alone in a psychological experiment).[7]

I was also at Harvard taught econometrics, in three terms, one by John R. Meyer, later president of the National Bureau of Economic Research, and the other two by another econometrician, Guy Orcutt, visiting from the University of Wisconsin [my father's alma mater, incidentally: though raised in Boston, I have Midwestern roots, and have always worked there]. Orcutt was a pioneer of large-scale economic and social simulations, simulation being the engineer's chief tool – coming into its own nowadays all over the scientific and engineering world, after the astounding Moore's-Law decline in the cost of computation. Architects now use it, and so don't need to be good at drawing. Though I was well trained by the standard of the time in another quantitative method, regression analysis, which has steadily taken over the minds of economists as the very meaning of "empirical" inquiry, I hardly ever used it for science.

When in 1987 the British Economic History Society invited me to write a pamphlet describing the "new" economic history, which the older British scholars viewed as a

[7] See McCloskey (2018).

distasteful import, they would not let me call it *Historical Economics.* They insisted on entitling it *Econometric History*, to make sure its alien character was signaled. No wonder, actually: two of the founding volumes in 1964 on what we later took to calling "cliometrics" (from a joke: Clio, the muse of history, plus measurement) claimed to be "econometric." The book by Alfred Conrad with that same Meyer (I was the proofreader) was subtitled *Studies in Econometric History*; and the best book of the great economic historian Robert Fogel was subtitled *Essays in Econometric History.* Neither of the books actually used regression analysis importantly. Both actually used accounting and simulation. But it would have been less impressive for Conrad and Meyer to subtitle their volume, *Studies in Accounting History.* Econometrics understood as more and more elaborate regression analysis is the scientific problem in economics, not the solution, another of those degenerating research programmes. Its use of tests of statistical significance without substantive loss functions has never been defensible, at Newman's front and back ends. The better method is All Hands on Deck – fact-based simulation above all; a little statistics, mainly with charts; a lot of out-of-sample inquiry into other situations, other countries, other periods; a great deal of focused scepticism about the "data" (Latin for "givens," which pretty much reveals the problem); a lot of introspection ("What would I do?"), because after all we are the molecules; a lot of reading of and listening to the human experience: theologies, histories, novels, plays, movies, gossip, country music. Maybe one could use also a couple of multiple regressions on a sample of convenience, to shed light on the parameters for the simulation that gives the order of magnitude of the effect that actually matters for the science. But it's useful only in this ancillary role, the way astronomers use it.

In my last year in the College I had taken a year-long graduate course on the economics of transportation from Meyer, which was my first inkling that economics was anything but an aesthetic criticism of the economy (criticized for structural monopoly, monopolistic competition, capital market imperfections, twenty year adjustments, irrationalities, entrepreneurial failures: all of them degenerating research programs because never relevantly measured). Economics was taught like literary criticism or art history, and mostly still is, like studying poems and paintings as *objets d'art* as against writing them or painting them. Meyer wrote economic poems. In college he had been a student of the economic historian Douglass North at the University of Washington, and led me, as North had led him, into thinking of economic history as serious economic science. Maybe the *only* serious economic science. Meyer "believed in economics," which is the economist's admiring description of someone who thinks that people are not idiots, that markets are pretty good, and that cost and benefit calculations using their outcomes will actually matter (all of which, by the way, is denied nowadays by behavioral economics). Being Meyer's (incompetent) research assistant early in graduate school taught me how to be an applied economist, and a tough-guy academic, too. During summers in the first two years of graduate school, instead of raking asphalt I worked with Meyer and a couple of professors of civil engineering from MIT, Brian and Marty, on a transportation proposal for Co-

lombia. It was more top-down-ism, but confirmed my deep belief in quantification, which I retain. Engineers like Brian and Marty have to be urgently interested in how big things are, or else the highway bridge falls down.

In those days, furthermore, we did multiplication and the like not on electronic calculators giving 16 digits of alleged accuracy but on slide rules giving mere approximations. A slide rule requires you to remember where the decimal point is to be placed in each of the multiple steps, or else your result will be off by a factor of 100 or 1/1000 or whatever. It forces you to think in orders of magnitude, which became my quantitative habit. I asked, and keep asking, because I am still an engineer at heart, how big, roughly speaking, could be the impact of ironmaking technology or of economy-wide entrepreneurial failure or of the enclosure movement or the move to free trade or of the old poor law or of the gold standard or of liberalism or a dozen other topics in British and then world economic history that I later examined. The engineering attitude has the disadvantage of top-down arrogance. But How Big humbles the arrogance. The engineering and the approximations reinforced an appreciation of error bounds. I had learned to appreciate them in finding input-output tables less than perfect even as accounting. I read Oskar Morgenstern's book, *On the Accuracy of Economic Observations* (1950), and never again thought of a number as a number, but always, as economists taught by engineers do – and economists taught by mathematicians never do – as N plus-or-minus ε, where the error ε could be in the actual world disastrously different from negligible.

Right from the beginning, and still, I found myself as an economic historian defending people in olden times from the charge of idiocy. The other tough-guy academic on whom I modeled myself was like Meyer just such a defender (and like Meyer and my engineers and my roommate David, a quantifier), the immensely cultivated economic historian Alexander Gerschenkron, whose graduate seminar I took for a couple of years. (He was a friend of another immensely cultivated man I also admired, my father, though Gerschenkron was both literary and mathematical, as I aspired to be, too. My father had a mathematics phobia, and was impressed when his child was judged age 14 or so to have both verbal and mathematical abilities.) At an evening dinner seminar in 1966 Gerschenkron assigned me to go after (in person) the historian David Landes, with whom a decade earlier Gerschenkron had sparred about entrepreneurship. Landes treated British businesspeople as idiots, especially in iron and steel. I said they were not, and later showed so with Solow-inspired productivity calculations in an engineering spirit in my PhD thesis, written under Gerschenkron's non-directive direction. (We had a single brief conversation about my thesis, and he read no draft until the final one.)

I fell in with Gerschenkron's hostility towards easy sociological explanations for difference. Yet teaching a dozen bright Harvard undergraduates for a year the texts of Marx, Tocqueville, Durkheim, Weber, and the British pioneers of cultural anthropology saved me at least from sneering ignorance about other ways of understanding the economy and its past. I look now with disfavor on the tendency after *Freakonomics*

(2005) in economics and in economic history to abandon the study of the economy itself and use instead crude arguments from Prudence Only and ever more sophisticated (if disastrously loss-function-less) regression analysis on locational data to do *ersatz* sociology, but in ignorance of the sociological tradition. "Persistence studies" in economic history, which assert repeatedly that things are hopeless because bad habits persist, are examples. The old model for such work is the prejudice against Southern Italy evinced in Edward Banfield's *Moral Basis of a Backward Society* (my father hired him away from Chicago, with James Q. Wilson, for whom I also was a research assistant, though even more incompetently than for Meyer) and in 1993 Robert D. Putnam's performance in *Making Democracy Work: Civic Traditions in Modern Italy.*

Taking economics seriously as a way of understanding the history or the economy was reinforced in 1965 by traveling two subway stops from Harvard Square to MIT – a much better department of economics at the time – to take a course on American economic history from Peter Temin the first time he taught it. Temin made elegant but qualitative use of economics, never in his career actually measuring anything much. But one of my classmates in Temin's course was Richard Sutch, from whom as nearly a *Doktorbruder* I learned a lot. Like Meyer, Richard when an undergraduate at the University of Washington had been a student in a course on economic history taught by Douglass North, which makes me an adoptive intellectual child and grandchild of North, from two directions. Sutch came also to Gerschenkron's seminar at Harvard. (In the first meeting in 1965 of Gerschenkron's seminar Richard was the only non-Harvardian. Decades later he told how startled he was at the meeting, and depressed afterwards. Gerschenkron, the learned Continental, mentioned as a point of method Occam's Razor, and asked if anyone knew what it was. One of the Harvard smart alecks replied, *Essentia non sunt multiplicanda praeter necessitatem*, that is, "Essences are not to be multiplied more than necessary." Richard, the rough-hewn son of the frontier West and then a student at the mere engineering school up Massachusetts Avenue, was appalled to find in his first encounter with East-Coast Harvard people that they spoke to each other *in Latin.* He learned subsequently that the sentence was the entire fund of Latin possessed by the Harvard smart aleck, who at the time was a notably naïve positivist.) Richard was a star in the Temin class and in the Harvard seminar – which contained as well Robert Hall, Thomas Sargent, Richard Sylla, and other luminaries-to-be of economics and economic history. It was in the Harvard seminar, and from Sutch especially (Gerschenkron merely watched and smoked his pipe, letting the junior devils torture whomever was presenting), that I learned how to run a productive seminar in applied economics. The question was always, "Is the cause large enough to have the effect claimed? How Big? How do you know?"

I then decamped in 1967 for a year on a Harvard traveling fellowship to England to do research for my thesis on whether British ironmasters had failed in the late 19th century, as Landes and many others claimed. Iron and steel was supposed to be one of the big late-Victorian failures in an economy that had once led the world, producing in the 1840s one third of the world's pig iron, for example, and in the 1850s inventing cheap steel. Two quite different ideologies ran such research from behind the curtain.

Socialists, such as the brilliant business and economic historian William Lazonick, wanted to show that "capitalism" didn't work even in what they supposed was its country of origin. The other, conservative line was the one Landes worked, also claiming that British businessmen were idiots – but this time not on the basis of class struggle but on the basis of national struggle. Both were zero-sum theories, and I opposed both of them, from behind my own curtain.

What neither Lazonick or Landes believed was a metaphor at the center of my economic thinking for decades (though recently abandoned as casual, and retained only for accounting purposes), the production function, in which capital and labor and materials are more or less good substitutes for each other. The big influence here was Robert Solow's famous paper of 1957 using marginal productivity theory to portion out how much of rising national income was from greater capital accumulation per worker and how much from "technological change," as he called it. A similar calculation was done about the same time by Moses Abramowitz, a colleague at Stanford in the early 1970s when I was for a term a visiting assistant professor there (Paul David was courting me, but soon gave up); Moe called it "the residual," or more pointedly "a measure of our ignorance."

The year in England 1967 – 68 in Swinging London, confirmed my anglophilia, to the bizarre extent of becoming a passionate if incompetent player and to this day a fan of English cricket, which was taught to me by my friend the economic historian S. R. H Jones. I met the British economic historians, who were less clever than us Americans in economics but more serious than us about the primary sources, people such as Barry Supple (who had in fact substituted one term for Gerschenkron back at Harvard) and Roderick Floud. At length Roderick and I brought the two tendencies together for Britain, as they now largely have become in European economic history, editing a two- and then three-volume textbook (1981; 1994), written by many hands.

[Meanwhile I found in 1962 the love of my life, Joanne, and married in 1965. Joanne's career flourished as a professor of nursing administration along with mine as a professor of economics and of history. My son was born in 1969 in the same month my father died, and about the exchange I wrote my last poem – in high school I had fancied myself a poet. My daughter was born in London during our second year-long trip there, my tenure year at Chicago, 1975.]

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And so in 1968, shorn of some of the prejudices and mis-educations of my youth, I began a scientific career. Though I had not yet finished the thesis, I was styled an assistant professor at the Department of Economics at the University of Chicago. The word "professor" startled non-academic people in Italy when I was introduced thus by my mother there, because at age 27 no European was *un professore.* Well, maybe some mathematical genius.

The amiable ex-Communist and by then rightish liberal Robert Fogel had hired me. It meant that Chicago's Department of a couple of dozen had fully three economic historians, Fogel on American economic history, the Russian specialist Arcadius Kahan, and me, with the retired Earl Hamilton, the maker of price indexes for the Spanish price inflation of the 16th century, in attendance at the weekly luncheons of the Department. Talk among us Chicago economists was never about politics or baseball, as it always was at Harvard. It was about economics, making Chicago the best place to learn it.

Chicago was during the 1970s admitted even by those who hated its market liberalism to be among the three most creative departments in the world – MIT, the University of Cambridge, and Chicago. The Department itself was only half of the economists at the University engaged in creating new economics. In the School of Business, for example, modern finance was being invented. For a decade I would eat lunch daily in the downstairs bar of the Quadrangle Club with its inventors, such as Merton Miller and Fischer Black. I didn't understand their higher mathematics, but did understand portfolio diversification, which I soon applied to explaining the scattering of peasant plots in medieval English open fields. Across the Midway in the Law School a modern law and economics was being invented, by Milton Friedman's brother-in-law Aaron Director and by Ronald Coase, the mild-mannered expositor of transaction costs and property rights. My office mate during 1968–69, the Chinese economist Steven N. S. Cheung (the prize student of the property-rights economist Armen Alchian at UCLA), was my tutor in property rights, with which I then approached the 18th-century enclosure of English open fields, after failing to find much effect of changes in contract law. How Big, I asked, engineering style. I was doing the program of law-causing-growth of Douglass North (and actually measuring, as on this matter he never did) a decade or so before the Good Douglass spoke out. He too was heavily influenced by Cheung, when Steve moved to Washington. The difference was that I measured the influence of legal change, and much later measured Doug's master idea of institutions, and found it small in actual history. It was if anything an intermediate, not an ultimate, cause. The ultimate cause was liberty and its theory, liberalism.

My colleague as an assistant professor, J. R. Zecher, a student of Karl Bruner at Ohio State and later my dean at the University of Iowa, explained to me what Robber Mundell and Harry Johnson in the Department were talking about in advancing the monetary approach to the balance of payments. It showed the services of money to be a matter of supply and demand (a piece of microeconomics I understood, unlike the ever-shifting sands of macroeconomics) – but in the entire world, not as Milton Friedman and Anna Schwartz persisted in thinking, country-by-country. Zecher and I wrote two papers on how the gold standard actually worked. No one believed us. But we were right and Friedman and most other students of the history were wrong.

Zvi Grilcihes, who later moved to Harvard, gave me over lunch, this time upstairs at the Quadrangle Club, a deeper understanding of the promise and the paradox of

measuring productivity change à la Solow-Abramowitz. On the one hand, any new productivity not explained by ordinary marginal productivity theory seemed to be a free lunch. On the other, Griliches, an Israeli student of Schultz at Chicago, didn't believe in free lunches, and traced the better methods in American agriculture, for example, back to the costly investment in land-grant university research. Four decades later the Griliches Paradox became the central theme in my proposed explanation of the nature and causes of the wealth of nations. As put by the Austrian economist Israel Kirzner – whom I knew about because his books were on sale from the University of Chicago Press, but whom I did not understand at all, because, puzzlingly, he did not believe in equilibrium – the spring of the economic watch is "the incentive is to try to get something for nothing, if only one can see what it is that can be done" (1976, 124). Modern economic growth is exactly a free lunch, and must be if the lack of it before modernity is to make historical sense – routine investment is historically routine. Being free of opportunity cost does not mean that (as I started calling it in the 2010s) the Great Enrichment since 1800 was inexplicable. A new rhetorical environment beginning to advocate liberalism in the 18th century encouraged (literally: "gave courage" to the hope of) entrepreneurs large and small. As a result over the next two centuries the production possibility curve leapt out by a factor of 30. Investment and legal change and rainfall and the existence of the universe were necessary but ancillary.

Aside from the thesis, which was well received and became my first book, my first important article rejecting the pessimistic view of Victorian entrepreneurs (after a hiatus caused by the emotional shock of having my second attempt at journal publication rejected, something that every young academic has to deal with) had been in 1970 for the British *Economic History Review*, "Did Victorian Britain Fail?" Answer: no. The article was supply-side economics before the letter (by which I mean an economics taking scarcity seriously, not the use of the term to describe the particular argument of my friend and colleague at the time at the School of Business, Art Laffer). It marked a definite break with Keynes, even breaking with his strangely mercantilist views on overseas investment. I never again believed in pump priming or governmental stimulus or the economist being in a position to calculate the marginal efficiency of capital-goods on long views and on the basis of the general social advantage – though in fact a crucial calculation in the article was just that, against the easy assumption that private investors were idiots.

I realized with a jolt that Keynes, my hero when I was an undergraduate, was not skilled at microeconomics, which 1970–1980 I now taught to students in Chicago's massive graduate program. My course, Economics 300, was taken by any student who did not have, say, a master's degree in economics from Hebrew University, and so I saw most of the students I later mentored as director of the graduate program. In my course they were made to understand economics through working weekly the numerous hard problems I devised, taken mostly from the real world, just as engineers do in their educations. It was also the way all the other instructors of microeconomics at Chicago taught, sharply different from the theorem-and-proof methods in which I had

been taught and which came to be embodied in textbooks by Hal Varian and then by Mas-Collel, Whinston, and Green, now the standard worldwide. In the hands of Friedman, Harberger, Becker, and McCloskey the microeconomics was applied to the world, not viewed through "proofs" as *objets d'art.* By teaching it for ten years, setting and grading problem sets by up to one hundred students every week, I got utterly clear about scarcity, budget lines, supply and demand, entry and exit, production functions, marginal productivity, index numbers, the labor market, and so forth, My textbook published in 1982 and 1985, available as a pdf at my website, was translated into Spanish, Chinese, and of all things Czech. But like other Chicago books (from elementary to advanced: Paul Heyne, Alchian, Peter Boettke, Stephen Landsburg, McCloskey, Stigler, Friedman, Becker, and recently Kevin Murphy) it failed to stem the tide of axiom and proof. The failure is why many economists substitute existence theorems or regression equations for economic thinking. They don't really know how to think like an economist, as naturals like Becker could easily, or as non-naturals like me could if they sweated at it.[8]

Many of the so-called Chicago Boys who came later to influence policy in Chile and Brazil learned price theory in Economics 300. The beliefs on the left about such carriers of an enriching liberalism to Latin America are mostly fairy tales. People confidently assert for example that Milton Friedman advised Pinochet in Chile. He did not.[9] In fact I taught more Chicago Boys than Milton did, and was gratified in the late 2010s on a visit to Chile to be honored at a large luncheon with former students. I was startled to find that they were all at least middle aged. Older. How did *that* happen?

At Chicago I also taught American economic history to undergraduates and British economic history to graduate students. I did not teach, or for a long time do research, outside of the Anglosphere, unlike say Joel Mokyr at Northwestern, a great teacher of economic history among his other accomplishments. Unlike polyglot Joel, or my supervisor Gerschenkron who knew even more languages than Joel, and read novels in all of them, writing with his wife for example a literary criticism of German translations of Shakespeare, I am terrible at languages. It is of course the way many native speakers of English are, more and more as everyone else learns English. But no excuses: my monolinguality is one of the chief shames of my scholarly life. I did acquire in my 30s, in Ben Jonson's tag about Shakespeare, "small Latin and less Greek," and worked away at Italian, too, *ma con pochi insuitati,* eventually at age 63 Dutch, too – but all of them when it was *te laat, te laat.*

Chicago required all graduate students to take a course in economic history, just as Harvard did until Gerschenkron retired, which gave the students a chance to do real economic science early, if on dead people. The month after I left Chicago, in 1980, the Department killed the requirement. The economist George Stigler (great and Nobel, though voluntarily barbarous) had taught the other required course, in the history of

[8] You doubt me? Here's the test: can you solve ten randomly chosen of the one thousand or so worked problems in my book? Oh dear, I thought so.

[9] See Montes forthcoming and 2016; Edwards and Montes 2020.

economic thought, until he himself killed it. I objected, to no avail. The combined result of killing the requirements in economic history and history of thought is that PhDs from Chicago, as from most American programs in economics, think that property rights did not exist in the Middle Ages and that Keynes is pronounced "Keens." (I report what I witness, no exaggerations.) Harvard had already, in one of its many contributions to the barbarization of economics, dropped the requirement in the history of thought the year before I started graduate school there. The personal result was that, foolishly, I did not read Smith or Mill among others with any seriousness until decades later. In the 1990s *The Wealth of Nations* and especially *The Theory of Moral Sentiments* came as revelations: this to a 50-year old alleged economist. For shame.

But in those 1970 s at the University of Chicago I got a good deal done in economic history:

1970s: British History

British iron and steel did not fail, 1870 – 1913.

The British economy as a whole did not fail.

Keynesian models cannot be used to explain economic growth.

Foreign trade was not an engine of British growth.

The effects of the Poor Law depended on income effects vs. wage effects.

Enclosure did not much enrich England.

Open fields were rational.

The gold standard worked through direct arbitrage in commodities, not through the price-specie flow theory, or Threadneedle Street.

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Then in 1980 I left the University of Chicago, for the University of Iowa. My scientific direction changed.

People are surprised, because Chicago is world famous (and very aware of it), while Iowa is a middle-of-the-road public university. The main reason I left is that the Department of Economics had pegged me as one of The Help. Long afterwards, I was pleased to hear that Gary Becker had said, "We made a mistake with McCloskey." At the Department there were two classes, The Barons and The Help. The Barons had Nobel prizes or were expected to get them. The Barons did nothing to keep the place running, with its two hundred graduate students in residence at any one time, and quite a large number also of undergraduates (Chicago did not have an undergraduate program in business, so they came to us). Such tasks, and administration in the Department and in the University, was left to The Help, which nonetheless included immensely distinguished economists, such as Theodore Schultz, A. C. Harberger,

Harry Johnson, D. Gale Johnson, H. Gregg Lewis, and others. You can count on it that anyone who has been the chairperson at Chicago in Economics is one of The Help.

In the spring of 1980, when I asked to be promoted from tenured associate professor to full professor, after they had promoted a student of mine (who later did little of scientific importance: they made a mistake on that one, too), Economics refused. Robert Lucas told me that my work wasn't "hot," the way the work of his friends the mechanics was. The Department of History was very willing (I was already an Associate there, too), but I was too much of an economist at that stage to view moving to History with equanimity.

I have always wished for love from institutions – an irrational wish, as I have slowly learned, though without entirely abandoning the wish. To my chagrin my love for Chicago was not returned quite as warmly as I hoped. So in that spring of 1980 I put myself abruptly, as we economists say, on the spot market. To my surprise, Stanford and Northwestern and other likely places expressed no interest. But Zecher was by then dean of the Business School at Iowa, and Iowa City looked like a better place for my 10-year old son (who was acquiring dubious friends in the urban jungle of Chicago), and my wife got a good job in the nursing school, and our startlingly large capital gain on our little townhouse in Chicago, purchased when I had gotten tenure in 1975, would allow us to buy a small mansion in Iowa City.

So I left. About the same time a number of other people left, and Chicago stopped being the creative place in economics it had been in the 1970s. It became under Lucas' influence a haven for mechanics, as many other departments already were. Chicago lost its distinction as a place where people took facts and basic economic thinking seriously. Gary Becker, again, told me long afterwards that he agreed, "and we are trying to do something about it." He failed. The current self-joke on a T-shirt printed for Economics is, "That's alright in practice, but does it work in theory?" I would have been unhappy at such a place, toiling away as The Help.

I was happy at Iowa, and productive. But it was a wrenching move in my otherwise charmed life. True, it's a counterfactual, the sole personal or professional one I worry much about (not for example hypothetical not-stuttering, not-marrying-Joanne, not-changing-gender; not being an economic historian, not turning to the humanities: about these and others I have never had a regret or a doubt; it exhibits a basic, even loony, optimism). Long afterwards I realized that going down the other path, staying at Chicago, might, after all, have ended badly by random event – being, say, hit by a taxi on 57th street. As Fats Waller said, "One never knows, do one?" What if Antoni Gaudì hat not been hit by a tram in Barcelona in 1926, though age 73, or Frank Lloyd Wright had not run off with a client's wife in 1909, only age 42? Would the Bauhaus have failed in the face of their humanistic architecture, and would the cities of the world have come to look like human habitations instead of glass warehouses? Can't say, can one?

Anyway I was always embarrassed to get distinction from a university rather than to give it. I remember for example going to Southeast Missouri State University to give

a talk and being treated like royalty merely because I was at Chicago. Ugh. It felt phony and undemocratic. Likewise in the other direction: the instant I moved to Iowa, an editor at the Harvard University Press dropped like a hotcake a project that he and I had agreed on. Double ugh. At Iowa I gave honor, as a reasonably big fish in a smallish pond, which gave me in turn permission to start on a new tack.

Having learned by age 38 how to be a conventional economist, at Iowa I started to take seriously the humanities. A crux was an invitation in my last year of twelve at Chicago in the fall of 1979 by the literary man Wayne Booth – I suppose on a reputation for being slightly less barbarous than my colleagues in Economics – to speak to his undergraduate Politics, Philosophy, and Economics course on "the rhetoric of economics." I said, "Sure, Wayne. But what's that?" Oh. He gave me a short reading list, which I crammed during a visit to my father-in-law's house in Vermont over Christmas of 1979, and gave the talk early in 1980. I was already made uneasy by the symmetrical sneering about the other place by the Harvard economists of my youth and the Chicago economists of my employment. They both claimed to be persuaded scientifically. Uh huh. How, then, do they actually become persuaded? I was for example uneasy about the evident fakery of statistical tests said to be "consistent with" some assertion that the economist wanted to be admired. I later learned to call it "ignoring power." It was a sub-routine in the fakery that Karl Popper made respectable by his claim that scientists actually sought disconfirmations, heroically. It mightily flatters the scientists, who therefore love Popper and become angry if you question him. I had started to read again the philosophy of science, which I had read in graduate school only up to justifications for an antique positivism, Popper being the end point (though he claimed to have killed positivism; uh huh).

I educated myself in various ways. For one thing I studied and read, starting for example to take Latin courses, being ashamed that an alleged scholar had forgotten all high-school Latin (excepting *Essentia non sunt* . . . .), and learning along the way, as I got into graduate courses in Latin, about another discipline with high intellectual standards, but different ones. In this respect being a full member of the Department of History at Iowa as well as Economics was an education. I taught for a couple of years the big 430-student course, Western Civilization (after 1648), getting the kids to sing Irish revolutionary songs for the lecture on Revolution and Romance, and to watch the movie "Night and Fog" on the Holocaust for the lecture on The Plot of European History. In History the long faculty meeting – we joked that it was the World's Most Deliberative Body – was a big part of my socialization in the values of historians. The Aydelotte Rule was that you could not speak or vote on a candidate unless you had read the book she had written, and were able to discourse intelligently on its merits. If American deans did their job and enforced the rule generally, American academic life would be raised to another level entirely, beyond the citation counting and political orthodoxies that now corrupt it.

For another thing, I helped start with colleagues in the humanities and the other social sciences at Iowa a Project on Rhetoric of Inquiry, which became my real in-

tellectual home (you can see how crucial that invitation from Booth was: with such threads our lives are woven). The acronym was, and is, Poroi, which I coined as a fruit of my study of first-year Greek: it means "fords" of a river, or more generally "ways and means," as in the House Ways and Means Committee. We gathered weekly to "workshop" a colleague's pre-distributed paper with the purpose of improving it. (The verb "to workshop" and the purpose was invented in Economics at Chicago.) We were not experts of course in the colleague's field, but we could always give her help with the organization, the characteristic tropes, the tone, and other matters that had for two-and-a-half millennia been studied and taught under the rubric of "rhetoric." Some three score colleague from Iowa and surrounding colleges attended, and still do, long after I have left Iowa, in subjects ranging from English to hydrology. In parallel with Poroi, I and others gave joint courses. I taught with physicists, communication scholars, English professors. The deans and chairpeople hated it, and made us do it as overload if at all, because it didn't fit the plumbing diagram. But it better educated me, and the students.

And for another I commenced a series of academic journeys, usually with my family in tow, which radically expanded my sensibilities – some would say blew them sky high. I was invited by Albert Hirschman for a year to the Institute for Advanced Study, which dazzled me. I remember the last week I was there being invited to lunch over at Princeton by an economic historian of the Islamic world, who to explain the lateness of the invitation blurted out that he hadn't realized earlier in the year that I was important (I was by then editor of the main journal in the field, which is what he meant, but after all my home university was Iowa, which is also what he meant. Ugh.) I had better luncheons with the great anthropologist Clifford Geertz, one of the many stunning anthropologists my life has been punctuated with (Robert Paul, Renato Rosaldo, Marshall Sahlins, Ralph Cintron); it gives an economist pause. At nearby Princeton (the Institute is independent) I met the sociologist of science Harry Collins, who informed me about the Strong Programme in the Sociology of Science, and in particular about the significance-test controversy raging then in sociology and psychology. (It became, in the 1990s and 2000s and after, a major project of criticizing econometrics, led mainly by my student at Iowa and then co-author Stephen Ziliak.) And in a bookstore at Princeton I stumbled on a copy of Richard Rorty's *Philosophy and the Mirror of Nature* (1979), which freed me as so many others from the squirrel-cage of epistemological dogma. Then I spent a northern summer at Australian National University, where I drank tea with central-state realist philosophers, and met Rorty in the flesh (he was there for birdwatching, mainly). Then in the mid-1980s at the University of York I taught economics and economic history and helped with the field exams for two summer terms, and played cricket with the sociologists of science there. (I started to read the poet Philip Larkin, eerily, in the week before he died.) Then, continuing my studies of the humanities, I spent six weeks in Hanover, New Hampshire [where I had in 1962 first met my wife, when she was in nursing school close to Dartmouth College] as a student at an annual jamboree for graduate students of literature and an occasional professor of economics and history, the Summer School

of Criticism and Theory (at which later I taught a week's course when it moved to Cornell). Then I was a fellow (I now object to the word) at the Department of History at the University of Manchester, meeting more historians, and spending a good deal of time watching cricket.

All this rushing about physically and intellectually provided the materials for the main event. The talk in Wayne's course back in 1980 had been recorded, and when I got to Iowa I told a research assistant to write it out. It became the core of a laboriously assembled article in the *Journal of Economic Literature* (1983), and then a book (1985), and then numerous other articles extending and defending through the 1980s the idea that economists use metaphors. The book was reviewed fifty times, rare in academic life. My economist colleagues were on the whole outraged to be called poets. My humanist colleagues were amazed that anyone would think otherwise.

Three conferences came out of all this kerfuffle. The first was organized by a Dutch assistant professor at Wellesley College, Arjo ("AR-yoh") Klamer, who when my 1983 article came out was depressed to find that someone else was thinking along the same lines as his PhD thesis at Duke University – what Arjo called the conversation of economics. Worthies like Solow and the young Jeffrey Sachs gathered to discuss the rhetoric of economics, among them the astounding Stanley Fish, then the chair of English at Duke, and a friend of Arjo's at pick-up basketball. He taught at the Dartmouth Summer School the same year I was there (we knew Stanley had arrived from North Carolina when his red Jaguar showed up in the faculty parking lot), and then he was my dean and friend at the University of Illinois at Chicago. The second conference we organized at Iowa, attended by Thomas Kuhn and Clifford Geertz and Wayne Booth, was on the wider question of whether science and scholarship should be seen as rhetorical. We said yes; we could never get Kuhn to accept the word, though all his work argued for the substance. We experienced similar irrational resistance from the Strong-Programme sociologists of science we invited to a follow on. But it was all a wonder and an education, with such findings as:

1980s: The Rhetoric in Economics

Productivity change in the Industrials Revolution was widespread, not confined to cotton.

Microeconomics is best learned through doing hundreds of realistic problems in applied price theory.

For example, monopolistic competition is an internally inconsistent theory.

For example, consumer surplus is equivalent to rises in national income.

The Coase Theorem in law and economics is routinely misunderstood as Smith's or Arrow's Theorem.

Better writing in economics, and elsewhere, can be learned by rule.

Economics has a rhetoric, as does all persuasive speech.

Tests of statistical significance are bankrupt in the absence of a substantive loss function.

§

During the 1990s I stumbled onto a discovery that my colleagues, such as Arjo Klamer in Economics at Iowa for a couple of years and John Nelson in Political Science there already knew, and thought obvious – namely, that the economy itself, and not merely its academic study, is a matter of speech and of ethics:

1990s: Rhetoric in the Economy

If you're so smart, why aren't you rich?

Conventional methodology of science is bankrupt as for example in economics.

A feminist economics makes good scientific sense.

Persuasion bulks large in the economy.

The bourgeoisie is not evil.

I had met Don Lavoie, an Austrian economist among many at George Mason University in Virginia, who taught me to look at the listening as much as the speaking side, hermeneutics as much as rhetoric. The very word "hermeneutics" outraged the more conventional Austrian economists, such as Murray Rothbard and Hans-Hermann Hoppe. Outrage, sneering, ignorant rejection, you will have noticed, is one of two reactions a scientist can adopt in facing an unfamiliar idea. The other is to listen, really listen. I had by then adopted as my motto a remark by Amélie Oksenberg Rorty (I owe a lot to the Rorty family), that what is crucial is "our ability to engage in continuous conversation, testing one another, discovering our hidden presuppositions, changing our minds because we have listened to the voices of our fellows. Lunatics also change their minds, but their minds change with the tides of the moon and not because they have listened, really listened, to their friends' questions and objections" (1983, 562). It is a noble ambition, being in economic terms the exchange that makes specialization, which is otherwise pointless, worth doing. I wish I always followed it, but, in gradually grasping what the Austrian economists were on about, I tried. Lavoie and I hung out with postmodern Marxists, too, such as Amariglio. They ask the right questions – though, hating the market, they give the wrong answers.

[Meanwhile, in 1995, for reasons I detail in *Crossing: A Transgender Memoir* (1999; 2019), I changed gender. "Reasons," though, are not exactly how such a decision is made. You do not change gender, or nation, or even political party, as though you are buying apples. It was extremely hard on my wife, as you can imagine, and on my two grown children. They have not spoken to me since then. I have three grandchildren I have never seen. Oddly, I love them, as less oddly I still love my former wife and my children. As the proverb goes, blood is thicker than water. *Crossing* tells of the terrors and the absurdities in my transition. Especially the absurdities. When at Iowa in the fall of 1995 I told my dean, like me a free-market economist, he launched (after picking his jaw up from the floor) into a comedy routine: "Thank God! I was afraid you were going to confess to converting to *socialism!* And wait: this is *great* for our affirmative action program, one less man, one more woman! And I pay you a lot: I can cut

your salary 70 cents on the dollar!" I then knew he would be my friend and protector, as he proved to be. My friend Klamer, who had returned to a professorship in art and economics in Holland, and who was the first person I told outside of my family, suggested I visit Erasmus University for a year, to relieve the pressure on my marriage family. When he told the president of the University that I was coming as Deirdre, the president, in the way of the Dutch civic religion of tolerance, said, "*En nu?* Why are you telling me this?" It didn't matter, which in fact was also the more surprising reaction of the conservative governor of Iowa. Is her CV the same? Can she teach?

A month or so into my visit I was standing talking with a group of economists, the only woman in the group, though of course they all knew my history. I made an economic point. The men ignored me, seeming not to notice what they had done. A couple of minutes later a man made the identical point, and the guys all got excited and said, "George, that's a *great* point." I was perversely pleased, *this one time only*, by the treatment – which every woman has experienced again and again and again, and which I experienced myself again and again and again: "*Yes*, they're treating me like a woman!" The second, third, … time was not gratifying. Oh, well dear: that's the deal.

After getting back, to Iowa and teaching – the students didn't care and the public scandal was finished, too – I was looking for something more, and found it in the Episcopal church.]

The three books on rhetoric in economics, started the 1980s and finishing in the 1990s, constitute, I suppose, a trilogy, though I had planned nothing of the sort before starting. *The Rhetoric of Economics* (1985; 2nd ed. 1998) said that economists use metaphors, being scientific poets, like physicists. *If You're So Smart* (1990) said that they used stories, being scientific novelists, like geologists. The two are the two ways humans think, along a scale from metaphor to metonymy, as has been confirmed by brain science. The third volume of the trilogy, purposely entitled in a boring way berceuse I was tired of economists being outraged by my books, having read only the titles, was *Knowledge and Persuasion in Economics* (1994). It was a full response to the attacks by economists and some philosophers. Its conclusion was that the only basis for epistemology, as philosophers like Hilary Putnam or indeed both Rortys argued, is ethics – exactly as Cardinal Newman as exposited by Wayne Booth had said: "discovering good reasons, finding what really warrants assent because any reasonable person ought to be persuaded by what has been said." That's all. No tricks.

Klamer and I wrote a paper for an economics conference in 1995 in which we showed that a quarter of the earnings from wage work came from persuasion, sweet talk, rhetoric – not threats or orders or physical coercion, or the routine work done alone, but the manager persuading her employees or her banker or her customers; the secretary sweet talking a document though a resistant bureaucracy; the rhetoric the police use short of violence (McCloskey and Klamer 1995). Hmm, I thought. That's surprising. Klamer was not surprised, because he was far ahead of me in understanding that humans work with language beyond orders and information. The sacred, for example.

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And so I moved in 2000 to my final academic employer, the University of Illinois at Chicago. Again it was the wearing out of love by the institution that pushed me. By this time my colleagues in Economics at Iowa were unhappy with me, as again one can understand: what is this weird stuff about literature and sweet talk? Where is your conventionally acceptable model of Prudent-Only Man expressed in mathematics and tested with the *t* distribution? And now gender change?!

The pull was another of those threads of our lives, this time being stuck in 1999 for a couple of hours on a delayed flight in Minneapolis and by God's accident being seated next to an old friend, Betsy Hoffman, who was now the highly entrepreneurial provost at UIC, who had hired a highly entrepreneurial dean of the Liberal Arts, that same Stanley Fish. At least the economists this time knew what they were getting, as those at Iowa had not back in 1980, when I looked like a standard-issue economic historian. At UIC I was also in History, but soon also in English and in Communication, and charmingly also adjunct in Philosophy and Classics. For me this multiple intellectual commitment running off in all directions was, as we used to say in Iowa, hog heaven.

I worked on *The Bourgeois Virtues: Ethics for an Age of Commerce* (2006), thinking it would be one and done. The book developed into a long ethical defense of the ill-named "capitalism," which fit of course with my long-standing classical liberalism. But I made the argument go far beyond the virtue of prudence. It was intertwined with my studies in theology, reading Aquinas, for example, with growing admiration. The book used so-called virtue ethics, a liberal approach (and that of Aquinas and Cicero and Aristotle), which the ancient Mediterranean philosophers used without question – and of which, suggestively, Adam Smith was the last modern exponent before its revival by female British analytic philosophers in the 1950s. I taught ethics occasionally back at Erasmus University in the Department of Philosophy on short trips, and one other full year, 2005–06.

But then I started to see that ethics might explain modern economic growth, the central scientific issue in economics. I had always, since the days of Solow and engineering and reading John Stuart Mill's *System of Logic* (1843) as a graduate student, seen calculating the oomph of alternatives as one way to do science. I thought of it in terms of the Solow/Abramowitz residual: after accounting for what marginal productivity, which we know about, can explain, the residual is the mysterious technical change we are left to explain. Or as Sherlock Holmes said in *The Adventure of the Blanched Soldier*, "Once you eliminate the impossible, whatever remains, no matter how improbable, must be the truth."[10] So in *Bourgeois Dignity* (2010) I embarked on

[10] That this principle, a commonplace of scientific or any other kind of thinking, is labeled a "fallacy" by logicians shows the limits of logic in the strict sense as a guide to science, and the importance of what the Greeks called *enthymemes.* The standard texts of first-order predicate logic for college students will have a section at the back called Fallacies, containing all the ways

running through all the explanations of modern growth. I found them to be impossible, even when combined with each other, to explain the startling 3,000 percent rise of income per person in real terms since 1800 – the Great Enrichment (the term "Industrial Revolution" is confined to 1750–1850, a mere doubling, 100 percent, and that only in a very few places; not until something more dramatic changed in the world was it inevitably going to be followed by 3,000 percent: 2 is a long way below 30 on the relevant scale of human flourishing.)

There were I argued two big sets of existing, and mistaken, oomph-lacking, explanations. On the right of politics the explanation is accumulation of capital, whether physical or human or legal, claimed by Adam Smith (in a rare but understandable false step) through Theodore Schultz to Douglass North. Its historical problem is that such accumulations are commonplace in history but did not result in anything like 3,000 percent. Its economic problem is that accumulation, without new ideas (and where do *they* come from?), experiences sharply diminishing returns.

On the left of politics the explanation is exploitation, and then accumulation (Marx after all was a follower of Smith), such as exploitation by slavery or in the surplus value extracted from the English working class. Its historical problem is that exploitation, again, is ancient and ubiquitous, but without 3,000-percent fruit. Africa, for example, had many slave societies. The economic problem is that stealing, except in a zero sum society, and then only for the thuggish side, is not the way to wealth, and the modern world has been strikingly positive sum. Redistribution *within* a base of 100 for an average person in 1800 CE does not get the average of the descendants of the 100-level earners up to an end point of 3,000. Three thousand percent can only come from a dramatic explosion of new ideas – steam and steel, universities and container ships, sewerage and radio, and on and on. Thus the subtitle of the second volume – it was now looking like I needed a multi-volume work to make the scientific point persuasively – *Why Economics Can't Explain the Modern World.* It can't, or at least the usual Samuelsonian or Marxist economics can't. And so:

2000s: The Ethics of the Economy

"Capitalism" is ethical.

Christianity and economics are consistent.

Political philosophy must rest on ethics (McCloskey 2006b).

Economics of the left or the right does not account for growth.

Neo-institutionalism for example does not.

Nor does investment or exploitation.

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that humans actually arrive at Newman's "ends," front and back, and much of the argument in between.

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But then the scientific question was where the explosion of enriching ideas came from. The proximate cause was not science or the Enlightenment.[11] These were elite preoccupations, Europe-wide when the imitative came from a tiny northwestern corner of Europe, first in Holland and then in the Anglosphere. Science until much later – say, after 1900, by which time the Enrichment was well under way – was anyway largely unconnected with practical matters such as moving to the frontier or opening a new kind of shop or inventing the band saw. What made people first around 1776 in the tentatively liberating countries around the North Sea and their colonial offshoots was something much more democratic and widespread: *the idea of liberalism.* Liberalism is the overturning of ancient hierarchy (a hierarchy which was enforced rather than reduced by the Enlightenment and high science: look at their patrons). It affirmed that, as the slave-owner of Virginia put it, all men (and women, dear) are created equal. The hierarchy of agricultural societies was to be flattened. It was extremely radical, an absurdity at the time in the minds of most people from Kyoto to Copenhagen. Its tentative implementation began to make the common people bold. They were permitted for the first time, in the British phrase, to have a go. And go they did, inventing the enriching panoply of the modern world.

But where then did liberalism come from? What was its own *ultimate* cause? I gradually realized that the causes were a series of lucky accidents of European history since 1517, the successful Reformation, the Revolts, Revolutions, and Reading that could easily have gone the other way. If Luther had not had a Elector of the Empire on his side, if the Spanish Armada had landed, if Charles I had been as sensible as was his eldest son, if George III had been inclined to compromise, if all the fortunate threads of European history in the three centuries before 1789 had not wound into a rope of the liberal idea by 1776 (the year, you know, of the publication of *The Wealth od Nations*), the modern world would have awaited another liberalism, elsewhere, Osakan merchants in Japan luckily seizing power, or the liberal tendencies anyway of Islam luckily asserting themselves with sudden success in the Ottoman Empire. Unlikely, you say. But so was the case of the miserably backward and horribly illiberal northwest of Europe in 1492. Liberalism was not some virtue deep in European history – not Western Christianity or the WEIRD hypothesis, for example, neither of which stands up to serious historical inquiry.[12] I came to lean hard against the presumption, difficult even for Europeans of good will to get over, that melanin-challenged people are better than others. Here I join hands with the comrades of my leftwing youth, but in a liberal cause, that all people *are* equal and should be left alone by the hierarchies – as the spectacular economic and often democratic successes recently of very non-European people has suggested.

[11] This contrary to my *geliefde* and *geleerd* friend, and ally in our little group of ideational students of the economic past, Joel Mokyr, as in Mokyr (2011) and (2016).

[12] I confront the Christianity hypothesis very briefly in McCloskey (2020). I am trying to get up enthusiasm for confronting the similarly Eurocentric WEIRD hypothesis.

I claim it as a killer app of a humanomics, which I slowly realized I had long been drifting towards. I was bobbing in the wake, unaware, of the pioneering ships of Smith, Mill, Boulding, Lachmann, Coase, Hirschman, Kirzner, Lavoie, Klamer, Vernon Smith, Bart Wilson. And so:

2010s: Liberty and the Great Enrichment

Liberalism explains the modern world.

Humanomics is the way forward for economics and economic history.

§

What are we to conclude from so strange a story?

For one thing, that stories matter to our lives – the story we tell ourselves as scientists no less than the ones we tell ourselves as wives or sons or Germans or whatever. From beginning to end we are narrating ourselves, doing memory. One thing I learned from my transgender memoir, which I knew theoretically but did not feel on my pulse until then, is that there's no such thing a narrative that is God's Truth – except, I reflected as a new Christian, God's Truth to be revealed at the Second Coming, until then imperfectly seen by fallible humans as in a glass darkly. We can't tell literally all the world, or all even a single human life. We can only set down stepping stones here and there through the gardens of our lives. The novelist John Grisham is supposed to have said, "The difference between reality and fiction is that fiction has to make sense." We are sense-making animals, setting down those stepping stones. It is after all a central point in the humanities and in humanomics.

How then does it fit together? What stepping stones? Three recent collections of shorter pieces published by the American Institute for Economic Research (which as a young socialist, without the slightest knowledge of what it said, I specifically disdained: paradox piled upon paradox) offer a blessedly short way of getting the data to draw your own conclusions. The collections contain replies, book reviews, and journalistic pieces on economics, history, and liberalism, and are a way into the scientific and methodological claims. Of course I would prefer that you turn to JSTOR and read every one of my several hundred articles, and buy and read every one of my two dozen books. (Especially buy. Ha, ha. Note that what makes the joke is the unease we have with Fiske's market pricing.)

All my work shows that I am above all a quantifier. The claim will surprise people who do not find lots of charts giving marginally relevant statistics or massive tables of numbers with asterisks attached. My friend the historical sociologist Jack Goldstone said of *Bourgeois Dignity* (2010), without thinking the matter through very carefully, that it "was not quantitative." But he was being fooled by the absence of very many

tables in the book, or any *t* tests at all – not a standard error in sight. Yet on every page, and often several times per page, the book is asking How Big, and suggesting ways to answer it, and sometimes coming up with relevant orders of magnitude, engineer-style. When I pointed this out to him, Jack agreed. He was a graduate of Feynman's Cal Tech, after all. If you can find a page of my writing, even my most literary or theological writing, such as "Voodoo Economics" in *Poetics Today* (1991) or "Humility and Truth" in the *Anglican Theological Review* (2006a), that does not contain implicitly a quantitative standard, I shall be surprised.

I do not write any sentence without asking – often confessedly at the N^{th} draft, when I notice how stupid or ill-argued something is that I've allowed myself to write in the N-M^{th} draft – the terrifying but scientifically necessary question Milton Friedman always asked in seminars: How do you know? The first week as an assistant professor at Chicago in 1968 I was at a cocktail party at Robert Gordon's house, and we were gathered around Milton. I ventured to assert, *on the basis of a column in* Newsweek *that Milton had just published,* that U.S. professional sport was a "monopoly." He looked up at me (he was very short) and asked the overwhelming question. I could hardly admit that I knew because he had told me so, and I retreated in confusion. He was undoubtedly seeking innocently for more evidence or arguments to test the proposition, not disciplining a smart aleck. But the smart aleck learned the lesson.

Because I write reasonably well (after those N revisions) people are liable to assume I am similar to, say, Walt Whitman Rostow (1916–2003), a great economic historian, contributing in the 1940s to the early uses of Keynesian reasoning in economic history and, with Anna Schwartz, to the quantitative economic history of business cycles, who wrote so well and above all so easily that he did not ask the terrifying question sentence by lovely sentence, and was led into scientific error. (Advising President Johnson on Vietnam, he was perhaps led into military error by the same disability.) John Kenneth Galbraith was another case in point. I once did a retrospective review of Galbraith's *The New Industrial State* (first ed. 1967), re-issued in a commemorative edition in 2007 by his economist son, James, and said to Jamie, whom I knew a bit and liked, "I'm sorry I was so tough on your father, but he never tested anything, never wrote as though he were asking himself 'Really, how do I know?'" Jamie, who is the soul of scientific seriousness (though a fierce Democrat), replied, "Yes, I agree, Deirdre, and I've devoted my own scientific life to not making the same mistake." Good. Something like the same curse of fluency is going on with the numerous economists who are too fluent at axiom-and-proof, Max-U modeling on slight revisions of other models, or the search for more magic dust of instrumental variables. The fault is to let one's facility with a machine, whether regression analysis or English prose, overwhelm sober truth, which is better found with All Hands on Deck than running the machine until it burns out.

I want, as Newman said, to do forthrightly the before-and-after, the judgments at the "ends." I do not want them to run the show form behind the curtain. But I want also to do correctly the middle bits, the counting and classifying and comparing that we

usually think is all there is to science. Some of the incorrectness is the shocking misuse of statistical significance, which Steve Ziliak and I have been gratified to see at last the American Statistical Association (2016) has roundly condemned the culture of "statistical significance" (and so has the US Supreme Court, nine votes to zero [Wasserstein, Schirm, and Lazar 2019; Ziliak and McCloskey 2008]) – though economists have yet to realize it. I wax indignant at weak middle-range scientific method: the belief in the precision of extremely roughly estimated numbers; the failure to think through a relevant scale on which a number or comparison can be meaningfully placed; the failure to give or even imply a relevant quantitative test (thus the unstoppable literature that I have written against for fifty years of war-talk of economic "failure"); not knowing how one's "data" are given; the mistaking of necessary and intermediate for sufficient and ultimate causes of economic growth. They all arouse my scientific indignation, and should arouse yours, too. (Comparing is the humanistic version of counting, by the way, and not comparing – Does it work in Chinese history? – should arouse your indignation as well.) I realize, as Adam Smith pointed out early in *The Theory of Moral Sentiments*, that indignation against Ms. X or Mr. Y is liable merely to evoke a protective response in the reader. But, darn it, we have to stop doing these wretchedly unscientific things.

In scientific method and substance, you see, I admit to a habit of opposition. I promise you it is not because I am seeking attention, or merely find it amusing to irritate people. It is rather because most of what passes for scientific truth in economic history, and in much of economics, is quite wrong, and often absurdly so, and I feel urgently a duty to correct it, for the good of the order. Every competent economist knows that at least half, maybe a good deal more, of what passes for economic reasoning in politics is scientific rubbish – that the balance of payments or trade is relevant for anything, that nations "compete" in foreign trade, that jobs are "created" and that such creation is the point of economic policy, and on and on. But so it is in economic science, too. Let's do it right. All Hands on Deck. Humanomics.

And besides being a quantifier I am a Christian liberal, and was one even when I thought I was a socialist agnostic. The liberal David Boaz at the Cato Instate puts it at the outset of *The Libertarian Mind,* "In a sense, there have always been but two political philosophies: liberty and power" (2015, 1). I have never sought power. I have always respected people, at any rate theoretically (though how many times I have failed, and snapped at subordinates or insulted bosses, I do not want to recount). As a boy I did not fight. I intensely dislike pushing people around.

But I do, passionately, want to persuade you. For your own good, you understand, as an economic or historical scientist, and for the good we can do with a liberal humanomics for the wretched of the earth. You come, too.

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