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What's Still Right with the Austrian School of Economics: A Comment on Boettke

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I can be classified as a late convert to the Austrian School, a fellow traveler. (Not, please, as Lenin is supposed to have put it, a “useful idiot,” but perhaps, as Mises certainly did put it, a “useful innocent”!) A turn to Austrianism was encouraged by intellectual and personal engagement with Karen Vaughn, Don Boudreaux, Emily Chamlee-Wright, Virgil Storr, Pete Boettke himself, and above all the sainted Don Lavoie. But I’ve been almost everything available in economics—a Marxist, a Keynesian, a social engineer, a Chicagoan, a property-rights enthusiast. Such undisciplined thinking at least yielded a somewhat broad perspective—which Pete himself achieves by his admirable catholicity of reading and his willingness to engage. Let me take advantage of his deep intellectual courtesy.

Pete quotes Lucas in Arlo Klamer’s *Conversations with Economists* (1983) about how very quickly Lucas’s suggestions became research projects among graduate students, as though the students were sitting by the telephone waiting for marching orders. Pete drew the moral that what Austrian economics needs for a “progressive research program” is just such “ongoing projects, what in many instances might be dubbed derogatorily as ‘shovel ready’ projects.”

I don’t think that’s what anyone needs. The problem is the same as with a Keynesian stimulus, to which Pete is referring in using the phrase. Are the shovel-ready projects actually progressive, or merely fashionable for the moment? Is the scientific marginal product above their social opportunity costs? Are they, so to speak, sugar highs rather than serious scientific proteins? I think Pete will agree on reflection that we have had many, many such examples of sugar highs in the history of economics. In recent decades we have been getting them at a faster and faster pace. Behavioral economics these days provides scores of examples (McCloskey 2018b).

We need, surely, to be empirical about the economy, which is the burden of Pete’s argument. But we also need to be empirical about the history of economic thought. I have been a member since the 1960s of enough movements in economics to

recognize a degenerative research program when I see it, usually a few years after I joined it. As an undergraduate at Harvard College, class of 1964, studying economics (while a roommate, in electrical engineering, was reading *Human Action*, which I and my other roommate scorned as “conservative”) I was an enthusiast for monopolistic competition, devised by my teacher Edward Chamberlain. A few years later I discovered by trying to explain it to graduate students at Chicago and especially in my price-theory book that it is logically self-contradictory. It says that a local dry cleaner knows he faces a downward sloping demand curve, because his competitor down the street is close. But somehow he doesn’t also know that the close fellow will react to the pricing policy inspired by the downward slope.

As a college senior and young graduate student at Harvard I then fell for what was known as “activity analysis.” Another one of my teachers, Robert Dorfman, had co-authored with Paul Samuelson (my mother’s long-time mixed doubles tennis partner, in case you care) and Robert Solow (the beneficiary of my mother’s work at the Veterans Administration right after the War) to write the pioneering work, *Linear Programming and Economic Analysis* (1958). It was supposed to put economics on a new basis of matrices— linear programming (can *you* solve a linear programming problem by the simplex method? I can), input output analysis (my own specialty until I realized that it made no economic sense beyond its uses for accounting), abstract general equilibrium expressed as separating hyperplanes, and other shovel-ready projects. Five years after I had learned it, linear analysis was dead, to be replaced by game theory, with its own rich array of shovel-ready projects. Some near contemporaries of mine at Harvard, such as the economic historian Robert Allen at Oxford, never got over linear analysis—which is to say that they never learned, first that “slack variables” is another name for neoclassical zero marginal products; and second, that in any case the economy is a matter of Austrian opportunity costs guided by prices.

Merely two examples, you say. But without much effort I can list in the history of economics since 1848 fully 108 (McCloskey 2018a), many for example in the form of proffered “imperfections” in the system of commercially tested betterments that meanwhile yielded a 3,000 percent increase in the real income of the poor. (To employ Yiddish syntax, some imperfections!) The economists were of course to be hired by government to repair the numerous imperfections. Thus monopoly (1890s), increasing returns (1920s), inevitable mass unemployment (1930s), demand created by advertising (1950s), the prisoners’ dilemma and the fisheries problem (1960s), informational asymmetries (1970s), and on and on. Here are the last 10 of my 108 (these are merely the “imperfections,” and do not include the affirmative programs such as linear analysis; you are invited to suggest the doubtless scores of others overlooked out of ignorance or lack of imagination): that prices are influenced by an unjust distribution of income, and therefore are irrelevant for policy in a just society; that profit is against people and social well-being; that high payment of CEOs is unjustified; that without artificially high wages we will not get labor-saving innovation (Kaldor; Habakkuk;

Robert Allen; Robert Reich); that the government has massively innovated (Mazzucato); that any imperfection—orphans drugs, for example—shows that capitalism is bad on balance and needs to be repaired by government, even if the imperfection is caused by government; that neo-liberalism has impoverished people worldwide; that we face neo-stagnation (Tyler Cowen 2011, 2014; Robert Gordon 2016); that inequality will rise, soon (Thomas Piketty). Each of these has inspired dozens of shovel-ready projects, scores of scientific articles, hundreds of editorials.

No, we should *not* demand more degenerative fashions with the half-lives of fruit flies. What we should demand are believable answers to serious questions, answers that stay answered. As Pete summarizes Michael Polanyi (Karl Polanyi's smarter brother), "previously unanswered questions get answered, but . . . previously unasked questions now can be asked" in an "unended quest." The humanist's version of the same idea was formulated in 1983 by the philosopher and anthropologist Amelie Oksenberg Rorty. I carry it around in my purse. What is crucial, she wrote, 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" (Rorty 1983, p. 562). Her first husband, Richard Rorty, quoted to similar effect Michael Oakeshott on the "conversation of mankind" ("humankind," dear): "As civilized human beings, we are the inheritors, neither of an inquiry about ourselves and the world, nor of an accumulating body of information, but of a conversation begun in the primeval forest and extended and made more articulate in the course of centuries. . . . Education, properly speaking, is an initiation . . . in which we acquire the intellectual and moral habits appropriate to conversation" (Oakeshott 1933, 198–99). Or as Pete says, "We must return to the republic of science and reaffirm the norms of free inquiry," by listening, really listening.

But it's not the unendedness that is good about the quest, or else we descend into what Richard Feynman, another brilliant scientist-philosopher, called "cargo-cult science." He was referring to the coconut-marked faux-airports that the New Guinea highlanders built after the War to try to get the big airplanes to come back with their interesting and profitable wartime cargoes. We seek truth, not faux-truth. We're not merely trying to find employment for economics PhD's, a dean-like, mercantilist rhetoric that Pete frequently falls into. (I do hope Pete regards "dean-like" as an amiable insult— and, better, as a word to the wise—and will stop using such locutions as "R1" ["research 1"] universities, as though Swarthmore or the University of Pacific had no scholars worth listening, really listening to.)

We seek not citations or impact factors and other corrupt decanal fancies but scientific progress. We want to create scientific value, which has to be judged by serious scientific standards, namely, by reading and assessing the work of the scientist, not by its mere popularity for the nonce among deans, soon dying out, like

monopolistic competition and linear analysis, increasing returns and inevitable mass unemployment. To avoid cargo cults will require attention to serious rhetoric. That means thinking about what we are doing instead of counting articles. The Blessed Adam Smith, after all, wrote only two books. No tenure for him at George Mason University.

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One important example in economics is the econometric movement, with its large number of shovel-ready articles and its lack of attention to serious doubts, in cargo-cult fashion. We have not, as Ed Leamer put it decades ago, got the “con” out of econometrics. Or the “tric[k]s out. Or the solipsistic “me” We’re left with a cry of “eo.” In Italian, “io, io, io,” or me, me, me. Not progressive. No surprises. No learning. No high scientific marginal product.

One of several reasons that Bob Lucas’s rational expectations became a cargo cult in macroeconomics—mostly, economic theories go to macroeconomics to degenerate—is that it claimed to depend on “observable implications” *expressed econometrically*. I remember well in that glittering dawn my excellent new colleague at the University of Iowa in 1980, young Charles Whiteman, a brilliant fresh-water Minnesota PhD, presenting to our seminars proposed “tests” by econometrics. Eventually Chuck gave it up and became a dean.

The problem was, and remains, that econometrics is itself a degenerative research program, worshipfully constructing faux landing strips out of *t*-test coconuts. Econometrics was established in the research practices of economists by the Concordat of 1957, Tjalling Koopmans’s *Three Essays on the State of Economic Science*. I remember how eagerly we graduate students read his bold if simpleminded program (“koopman” in Dutch, by the way, means “salesman”). It was a Concordat between theorists urged to make theorems without reference to facts and econometricians urged then to “test the implications” à la positivism c. 1920. Koopmans (Nobel 1975) was the great propagandist for the division of the empire of economics into provinces of factless theory and of inconclusive econometrics, both expressed in matrices. Matrices were hot in 1950s economics.

The nub of the problem with the econometric shovel-readies is statistical “testing” by sampling theory, without reference to scientific or social loss functions. Koopmans debated the Virginia economist Rutledge Vining in 1949, during the controversy over the introduction of Cowles-Commission methods of econometrics as against the older empiricism of the National Bureau of Economic Research (Vining 1949). Vining attacked Koopmans’s proposed “strait jacket on economic research”—which by 1957 the profession would be found putting on as quickly as it could manage the buttons—and then quoted George Udny Yule, one of the pioneers in England of statistical method, writing in the early 1940s against the fashion for Fisherian two-standard-deviation tests: “there has been a completely lopsided—almost a malignant—

growth of sampling theory [that is to say, t tests without attending to substantive oomph]. . . . Caution, common sense and patience . . . are quite likely to keep [the experimenter] more free from error. . . than the man of little caution and common sense who guides himself by a mechanical application of sampling rules. He will be more likely to remember that there are sources of error more important than fluctuations of sampling.” Wise words, since ignored.

As Pete affirms, “developments from 1930 to 1980 . . . [were] among the most pernicious intellectual developments of the 20th century. Scientism kills science.” All you need to persuade yourself of the truth of his diagnosis in economics 1930-1980 is to ask what major economic fact has been established by econometrics since its invention in the 1940s. But answer came there none— / And this was scarcely odd, because / They'd eaten every one.

By the standard of my PhD generation I was well trained in econometrics, which means that by now I am far, far behind the curve of the evolving, shovel-ready cargo cult. I am chiefly an empirical scientist, what you would call in biology a bench scientist, eager to establish facts that last. For example, that Victorian Britain did not fail economically, that foreign trade was not an engine of British growth, that enclosure was not important economically in England, that scattering of plots in medieval English agriculture was insurance, that the gold standard worked through arbitrage not gold flows, that interest rates in the Middle Ages were very high, and lately that ideas not capital or Pete's beloved institutions caused the modern world, stimulated chiefly by the political idea that Pete and I both love, liberalism.

Though pretty well trained in econometrics, I've hardly ever used it, except as a conceptual framework for thinking about economic observations, much as abstract general equilibrium is inapplicable to anything empirical, except as a conceptual framework for thinking about the economy (Diamond 1988). In graduate school in the mid-1960s from my mentor John Meyer and two professors of civil engineering at MIT with whom John was working I learned the uses of simulation, which I later employed to study portfolio diversification in medieval open fields. From Peter Temin at MIT, going over there in the first year he taught economic history, I learned the use of simple theory-driven narratives of the sort Boettke here praises—in Temin's case for example the use of supply-and-demand logic to narrate the history of the iron trade in the U.S., which I then applied to iron and steel in Britain. And from my supervisor Alexander Gerschenkron I learned the crucial role of comparison—which is the humanist's version of regression analysis, as for example the comparative method of the French historian Marc Bloch.

Such methods are never taught explicitly in graduate programs now—nor is philosophical introspection (Pete quotes Machlup's brilliant question: “Suppose matter could talk”) or graphical methods (see Tufte's *The Visual Display of Quantitative Information*, one of the great books of the past fifty years) or opinion surveys or archival

research or experimental methods or national income accounting—in favor instead of three terms of regression analysis, driving its scientific marginal product well below zero. By a happy accident my own three terms of it came from that same Meyer, and two from the pioneer of socio-economic simulation, Guy Orcutt, visiting from Wisconsin. So I learned that regression analysis wasn't the only way to make a serious empirical argument in economics. Try telling that to a fresh PhD nowadays from most graduate programs, except George Mason's.

A few regressions in a 1976 paper on the gold standard with a colleague at Chicago, Joseph. Richard Zecher, and then in a 1984 paper on the cost of medieval grain storage with a student at Chicago, John Nash, revealed that t tests and R -squares were almost always irrelevant to scientific questions. This despite their grotesque prominence among such econometricians as David Hendry, who advises everyone to “test, test, test.” No, no, no, David, not if the suggestion is to test econometrically in the usual ways expressed in standard errors, following the “completely lopsided—almost a malignant—growth of sampling theory.” But yes I said yes I will Yes, by all means *actually* test, quantitatively, comparatively, qualitatively, by theory-based narrative, theory-based simulation, intelligent introspection, listening to the conversation of humankind, and the rest of our ways of getting a purchase on the world. Sure, occasionally you might look at the size of coefficients in a regression, when relevant. Usually in such looking you should ignore the standard error. In short, do not decide scientific questions by t tests based on the notion that numbers contain their own meaning, independent of the human conversation of a republic of science.

Thus cargo cults. We don't need 'em.

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I wholly sympathize, though, with the appeal in Lachmann's letter to Pete and in the comment to him by Mancur Olson, that it's time in Austrian economics to pursue “ordinary science,” that is, believable answers to serious questions. And I enthusiastically approve of GMU economics's “research program in applied comparative historical political economy.” Go Patriots!

But what seems lacking even in the empirical Austrian program, and also in the overlapping program of neo-institutionalism which Pete advocates, is serious attention to *quantitative* simulation, especially in the matter of “the institutional analysis of development.” You will think me inconsistent: I criticize econometrics as a scientific failure and yet call for quantification. But econometrics is on the whole a con game, a cargo cult. I call for real quantitative work, in the numerous ways that orders of magnitude matter. If you want to see how physicists do it, read Feynman's *The Character of Physical Laws* (1967) and *Feynman Lectures on Computation* (2000). You will be stunned at his steady attention to magnitudes. No standard errors without a judgment in the substantive scientific conversation of how big is big.

An example of the problem in an institutionalist/Austrian program is a book I admire very much indeed, written by a neo-institutionalist/Austrian economist I also admire, Douglas Allen's brilliant *The Institutional Revolution: Measurement and the Emergence of the Modern Economic World* (2012). Doug claims that an improvement in the measurement of Nature made for lower transaction costs and the Industrial Revolution. His argument is a typical example of neo-institutionalism in the style of Douglass North, which Pete Boettke here wants to marry to Austrianism. A fall in a wedge of inefficiency is supposed to provide Good Incentives, and the modern world. We all favor Good Incentives. For example, we all favor private property-- which in fact has been enforced in every society since the caves, or else it was not a society. But the elimination of wedges, as nice as it is, leads merely to Harberger Triangles of improved efficiency. It does not lead to the factor of 30 or 100 in properly measured real income per head, which is the Great Enrichment 1800 defining the emergence of the modern world. Such an Enrichment can only be explained by a radical transformation in the conditions for commercially tested betterment, namely, the liberalism of the eighteenth century gradually implemented to give ordinary people a go. Let my people go. That's the novel ideal of Voltaire and Adam Smith and Wollstonecraft and Mill and Hayek and Boettke. The idea of liberalism and its astounding fruit for innovations such as steam turbines, window screens, restaurant franchising, containerization, the internet, and R1 universities made us rich and a little bit wise.

Allen does in his book excellent scientific work in explaining some of the peculiarities of British public administration, such as its reliance on aristocratic honor and on the prize system in naval warfare. But he attributes to such *public* administration an implausibly large quantitative effect on *private* incomes. The merging of power and plenty, though popular with many historians and some economists, is mistaken. Further, the alleged increase in a modern ability to measure marginal products is implausible. Large modern enterprises face greater, not smaller, problems of assessing the contribution of individuals. Thus CEO pay. Thus collective bargaining. Doug's book on measurement, that is, does not measure, and the probable order of magnitude of the items he focuses on is too small to explain any but the details of administration.

Doug uses the "analytic narrative" that Pete recommends most skillfully. But one does not have, in short, the sense of a killer app. In an early comment on my own thinking about the role of ideas in the Great Enrichment the neo-neoclassical Herb Gintis complained in the same way, using the very expression. I replied in the years following by supplying three volumes of evidence, all of it founded on the quantitative fact of the astonishing 3,000 percent increase in real incomes of the poorest among us. I think it kills. (Herb hasn't told me if he's satisfied yet.)

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So the purpose is not merely to sustain yammering. We pursue truth, small-t,

qualitatively about mental categories in humanistic style, if we have the sense, and quantitatively in measuring the categories in the style of physics, if we can dig out the evidence. Pete's sociological framing of his position makes it sound like the point is yammering for pay. He praises Randall Collins's astounding if in the end pointless book on "the success and spreading of a philosophical movement." Such success is surely not the correct criterion. We are not trying to achieve sociological success. After all, Ptolemaic astronomy had sociological success for about 1450 years, and is still used daily for navigation on the Earth. But for many uses, such as getting to the Mars, it is not true, small-t.

Summarizing Polanyi, Pete declares truly that "a scientist must make contributions that reflect and balance plausibility, intrinsic interest, and creativity." But remember that Michael Polanyi was a physical chemist, making actual, lasting scientific contributions just below the level of the Nobel (his son John got it in chemistry in 1986). Meanwhile brother Karl Polanyi was spinning historical cargo-cult projects of analytic narratives that have been repeatedly shown, in field after field, to be wrong, despite all their power to generate "research" and academic jobs and honors (Hejeebu and McCloskey 1999). But, says Pete, "learning what constitutes that intellectual bar that must be hurdled in science is perhaps the most important acculturation process of the next generation of scientists." No it's not. The most important bar is telling them to be good about seeking truth, acquiring "the intellectual and moral habits appropriate to conversation."

If you want a Good Career, of course, you can follow the script of James Watson (b. 1928) in *The Double Helix*: "A generation of graduate students," wrote Anne Sayre about Watson's teaching, "learned a lesson: the old morality is dead, and they had . . . been told about its demise by . . . an up-to-date hero who clearly know more about how science was acceptably 'done' than the old-fashioned types who prattled about 'ethics'." (Sayre 1975, p. 195). To the contrary, said Ronald Coase (b. 1910): "My mother taught me to be honest and truthful" (Breit and Hirsch 2004, p. 190). James Buchanan (b. 1919), speaks of a teacher in graduate school who "instilled in me the moral standards of the research process, . . . something that seems so often absent in the training of economists of the post-war decades" (Breit and Hirsch 2004, p. 139). As Feynman put it to the Cal Tech graduates in his commencement address on cargo cults, "After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that." It's about all the method we can handle.

We are trying to get hold of truth—small-t truth, provisional, always facing up to our friends' questions, doubtless not the final Truth, which will be revealed only, as some of us believe, at the Second Coming. "A regular periodical," says Pete who edits one, "would help individuals within the group express and clarify the central ideas, raise critical questions about such ideas, and ultimately cultivate a culture of criticism so that individuals within the movement can constructively express their skepticism and challenge in-movement ideas." That's right and good: back to Rorty and

Oakeshott and Feynman. What all the men miss, though, is the importance of love. If you love your colleagues in the pursuit of truth you can give and accept criticism advancing it, instead of merely getting angry. You can listen, really listen. Many conversations in economics, even in in Austrian economics if it is lacking love, are dialogues of the deaf.

“Perhaps the most important aspect of a thriving community,” Pete continues, “is the focus on and preoccupation with ideas instead of a fixation on any one individual.” He’s spot on there. I am struck by the smarmy hero-worship that goes on in economics, and undoubtedly in other fields (though not in literary and most historical studies, by the way: they commit other sins), an unattractive devotion to hierarchy. It’s craven. (To come to believe it, google Econ Job Market Rumors. A minute or two of reading will suffice.) Arjo Klammer and David Colander found it in their survey of grad students, notably provincial—only Harvard is True; only Chicago—to the point of self-parody, not listening, really listening. “What kills philosophical movements,” says Pete, “is cult of personality, insular isolation, and immunizing stratagems with respect to criticism.” We all know what school of Austrian economics he is talking about.

Yet Pete claims that “philosophical ideas that become cults of personality are doomed to have little success.” I am afraid not. Keynes. Newton. I certainly agree with the remark as a matter of pursuing truth, but as sociology, which Pete relies on, I doubt it. Aristotle was worshipped. Galen in medicine. As much as I admire St. Thomas Aquinas, who narrowly escaped prosecution for heresy when he was alive, when he became dogma in the Catholic church six centuries afterwards it created a “cult of personality, insular isolation, and immunizing stratagems,” which one can see still at work. Count up the cases in economics. Pete again descends to a “success”-oriented sociology of science in admiring “thick horizontal relationship throughout the globe” instead of truth.

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Austrian price theory, Peter notes, was “especially in the hands of Mises and Hayek, *institutional* in nature: they placed a priority on the framework within which economic life takes place.” But also on ethics. “An institutional framework of property, contract and consent, is a fundamental pre-requisite for the operation of prices and profit-and-loss. Prices guide, profits lure, and losses discipline within the competitive entrepreneurial market process.” True, but such a neo-institutional framework à la Douglass North leaves unanswered the central question of the *causes* of the wealth of nations. The cause was in fact an ethical change in the 18th century, that liberalism Pete and I admire.

Yet this, too, is available to an empirical Austrian economics, in the battle among “Smith, Schumpeter, and Stupidity” of which Pete elsewhere speaks (Kiesling 2011;

Boettke 2011). What economists need to understand from historians, but do not, is that “an institutional framework of property, contract and consent, [which] is a fundamental pre-requisite for the operation of prices and profit-and-loss” has *always* existed. What was new in the past two centuries, and caused the kink in the hockey stick after 1800, is not going to be discovered therefore by “price-theoretic and institutional analysis of the economic process,” as much as I love and practice it, no more than by the endless macroeconomic ruminations on the A -term in Solow’s $A F(K,L)$ on which I spent my youth. What explains the Great Enrichment is an entirely new birth of liberty, encouraging human creativity for the first time in history on a massive, and growing, scale.

The central historical error in the North-Weingast argument underlying neo-institutionalist explanation of the Great Enrichment (which Pete swallows whole) is to think that it started in 1689. The evidence is overwhelming that it did not, yet people influenced by North who do not look seriously into the history themselves, such as Daron Acemoglu, go on and on saying “property, rules of the game, presto!” The Northian story has passed into conventional thinking in economics, as for example in an alarming article titled “Growth and Institutions” for *The New Palgrave Dictionary of Economics* (2008) by Daron Acemoglu (Acemoglu 2008; compare Acemoglu, Johnson, and Robinson. 2005, citing R. H. Tawney, unaware it appears that such Fabian views have largely been overturned by historical science):

Consider the development of property rights in Europe during the Middle Ages. Lack of property rights for landowners, merchants and proto-industrialists. . .

No, as has been known by historians of medieval Europe for a hundred years. Property was very fully developed, especially in land and in personal possessions. For Italy, of course, the fact is obvious, and the evidence there of fully developed rights in all sorts of property including labor is overwhelming. But a market even in land even in remote England functioned in large and small parcels. Exchange on secure terms took place there in all commodities and factors of production at the latest from the Normans and their lawyers—or outside the king’s court in leet courts registering peasant deals in the thirteenth century, and in most respects hundreds of years earlier, as has been a commonplace among English medievalists since the 1950s at the latest. Edward Miller wrote in 1951 that “there was a very flourishing land market amongst the [southern English] peasantry. . . in the early thirteenth century” (Miller, p. 131). One of the leading recent students of medieval English agriculture, Bruce Campbell, notes that “tenants of all sorts were active participants in the market, trading in commodities, buying and selling labor and land, and exchanging credit,” citing some of the numerous medievalists who agree (2005, p. 8). That does not mean that everything worked smoothly. Campbell argues that the fourteenth century was characterized by “rural congestion engendered by the lax tenurial control exercised by most landlords” (p. 10). Overfishing. But anyway Campbell’s picture, based on the best scholarship

over many decades, is the opposite of the exploitation and the absence of markets posited by Acemoglu. The serfs owned the lords, not the other way around. Such a conclusion is found in most of the modern evidence-based literature on the peasantry in England, as for example in Raftis (1996, p. 4).

To continue with Acemoglu's stylized history:

was detrimental to economic growth during this epoch. . . .

No: lack of property rights had little to do with poor medieval productivity (McCloskey 1975a). And see Raftis 1996, p. 118: in the medieval historiography developing since the 1940s, "customary tenure [that is, serfdom] becomes no longer a block to [English] economic development but an instrument for such development Peasant progress occurred despite the limitations of the manorial system."

Consequently, economic institutions during the Middle Ages provided little incentive to invest in land, physical or human capital, or technology.

No: incentives of a strictly economic sort did not change between 1000 and 1800, not much. See Berman 2003; and again Raftis 1996, pp. 9–10, 7: "The major customary tenants [were] the most active economic agents" even in the "purest type of manor." A good, rough test of whether a student of the medieval economy actually knows the terrain is whether or not she is familiar with the work of Father Raftis (on this account see his Raftis's strictures on Robert Brenner [1996, p. 214n40]). Acemoglu and before him North, alas, fail the test.

and failed to foster economic growth.

Economic growth did not occur. But—outside of Russia—the absence was not because of a lack of property rights but because of a lack of massive innovation, and that in turn because of a lack of bourgeois dignity and liberty, and a lack of widespread elementary education.

These economic institutions also ensured that the monarchs controlled a large fraction of the economic resources in society, . . .

No. Even in early modern times the percentage "controlled" by monarchs was small by modern or some ancient standards: think 5 percent of national income. Rents from royal estates, until sold off, would make the figure higher—but the estates are rental income, which is an affirmation rather than a violation of the rights of private property that any taxation represents. The *aristocracy* did "control" a large share of the land, though freeholders owned a great deal, too, and the serfs that Acemoglu thinks were part of the economic resources "controlled" by the "monarchs" were in fact largely independent—certainly from 1348 on, and in their ability to sell their labor and buy their long-leased land, earlier. But again there was ordinary property and ordinary labor markets, contrary to the cargo cults initiated by Karl Polanyi and lately North and followers.

solidifying their political power and ensuring the continuation of the political regime. The seventeenth century, however, witnessed major changes in the economic institutions. . .

No. The *economic* institutions, if by that one means property rights, or even taxation, did not change much in the seventeenth century in England, by comparison with changes in other centuries. The great changes in property and especially contract law happened in the nineteenth century, not in 1689.

and political institutions. . .

Finally a partial truth, but only in England and Scotland and a few other places such as Poland: not in “Europe” as he claims.

that paved the way for the development of property rights. . .

No. Property rights, I repeat, were already developed, many centuries, or indeed millennia, earlier.

and limits on monarchs’ power.

A truth, but a Dutch and later a British and still later a Polish and Swedish truth, and having nothing to do with an allegedly novel security of property—for all the self-interested talk by the tax-paying gentry at the time against the modest taxation by the Stuarts and their heirs, from John Hampden to Thomas Jefferson. The share of British government taxes in national income did not fall in the eighteenth century: it strikingly rose (O’Brien 1993, p. 126, table 6.1).

Acemoglu in short has gotten the history embarrassingly wrong in every important detail, and his larger theme is wholly mistaken. It is not his fault, however. The few economic historians he has consulted, especially North, have told the history to him mistakenly, since they, especially North, had not consulted the work of historians using primary sources and had not sufficiently doubted the tales told by nineteenth-century German Romantic historians about the olde tymes of the Middle Ages and about the allegedly modern rise of rationality.

The problem is, to say it yet again, that much of Europe—or for that matter much of China or India, not to speak of the Iroquois or the Khoisan, when it mattered—had credible commitments to secure property rights in the thirteenth century CE, and indeed in the thirteenth century BCE (Clark 2007 is good on this, pp. 10, 212). China, for example, has had secure property in land and in commercial goods for millennia. And in the centuries in which the economists claim that Europe surged ahead in legal guarantees for property, the evidence is overwhelming that China and Japan had secure property. True, early in the short century of their rule the Mongols (Yuan dynasty, 1279–1368) were tempted to put in place such anti-economisms of bad property rights as prohibiting autumn planting—in order to give ample grazing for Mongol horses. But even the Mongols quickly realized that a prosperous and property-respecting China made a more profitable cash cow. And under the Ming and

Qing (1368–1911) property and contract laws were enforced upon high and low, as they had been during most of Chinese history.

Merchants, for example, appear to have been more, not less, secure on the roads of the Chinese Empire or the Tokugawa shogunate in recent centuries than they were in a Western Christendom plagued until the nineteenth century by pirates, or by highwaymen riding up to the old inn door. Chaucer's merchant in 1387 "wished the sea were kept [free of pirates] for anything / Betwixt Middleburg [in Zeeland] and Orwell [in Lincolnshire]," as the Chinese and the Japanese and the Ottomans had already long kept their seas, though with some difficulty (Chaucer, *Canterbury Tales*, "General Prologue," lines 276–277).

And behind the historical errors of the neo-institutionalism that Pete recommends stands a deeper problem of method, which Austrian economics—for example, that of Ludwig Lachmann—could resolve, and triumph scientifically by resolving. Consider the book by Pete's student and colleague Virgil Storr, *Understanding the Culture of Markets* (2012). Virgil is a free trader in ideas, and in particular imports back into economics *meaning*, long banished by behaviorist protectionism. But his elegant little book is far too kind to neo-institutionalists. From beginning to end Storr treats the neo-institutionalists such as Douglass North and Avner Greif gently. The neo-institutionalists, repeating without much thought over and over, "Institutions matter," mean to say that "Institutions are constraints like budget lines. They are not human conversations." Since the conversational character of markets is Storr's main point, he would do better to make common cause with Bart Wilson, Vernon Smith, and me in pursuing "humanomics," that is, an economics keeping its mathematics and statistics but entering, too, the human conversation since the Epic of Gilgamesh. Instead of the "patterns of meaning" that Clifford Geertz assigned to culture, the metaphors and stories, an "institution" is defined by the neo-institutionalists merely as the "rules of the game" in North's formulation, like the rules of chess. Even when Avner Greif tries to acknowledge the role of culture he sees it not as meaning but as constraint: it leads merely to "path dependence of institutional frameworks, . . . forestalling successful intersociety adoption of institutions" (quoted in Storr p. 1). As Storr says when gently summarizing North's unhappy late production, *Understanding the Process of Economic Change* (2005), "beliefs [that is, culture, including meanings] . . . influence the institutions [people] select to constrain the choices they make" (p. 3). "Beliefs" and "institutions" in the neo-institutional orthodoxy are constraining chains only, not a mobile army of metaphors, a dance ("How can we tell the dancer from the dance?"), webs of significance in which humans are suspended and which they themselves have spun (as Storr paraphrases Geertz), the poetry and stories of the culture. Storr puts well the relevant criticism of the neo-institutionalists when he remarks that the social-capital metaphor characterizing "beliefs" used repeatedly by North and others "exaggerates . . . the degree to which actors are slaves to their culture" (p. 9), automatons rather than poets or dancers.

§

Yet Pete says truly: “The property rights economics of Armen Alchian, the law-and-economics of Ronald Coase, the public choice economics of James Buchanan, and the entrepreneurial market process economics of Israel Kirzner. Each of these four challengers to the Samuelsonian mainstream can trace their roots to Mises and Hayek. As James Buchanan once argued, these four schools of thought in economics should be seen a source of consilience, rather than conflict, and that progress in the science of economics will come from the marrying of these different approaches into a new paradigm.”

Yes, and I enthusiastically join Pete in the project, adding only, as he would surely agree, Smith, both Adam and Vernon.

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