

## **The Humanities Are Scientific: A Reply to the Defenses of Economic Neo-Institutionalism**

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Abstract: I reply to amiable criticisms by Greif, Mokyr, Langlois, Lawson, and Tabellini of my own criticism of neo-institutionalism. They say that “culture” is included in neo-institutionalism – which is mistaken on any serious definition of culture, such as those involving ethics, rhetoric, ideology, and ideas. They also say that neo-institutionalism has advanced beyond Max U and Samuelsonian economics. That’s also mistaken. They do not attend to the humanities, which as “humanomics” can indeed acknowledge “culture” and non-Max U. Their particular historical examples show the opposite of what they think is shown. Ideas, rhetoric, ethics changed, and had to change, before institutions mattered.

What is crucial, wrote the philosopher and anthropologist Amélie Oksenberg Rorty, 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.<sup>1</sup>

An issue we can converse about, and perhaps change our minds about, is the range of the word “institutions”. Everyone here seems to agree that it is baggy. Sometimes in science bagginess is a good idea, leading eventually to the right amount of precision (“energy” and “evolution”, for example). Sometimes it’s not (“phlogiston” and “ether”).

Start with North’s “rules of the game”. As my friends Avner Greif and Joel Mokyr (2015) note, “the idea of institutions-as-rules, originally proposed by North (1981, 1990), was soon realized to be limited in scope. Yet, it was rhetorically powerful”. Yes, it certainly was, especially since the good Douglass kept repeating it, and most economists if asked would say that such a definition is the heart of neo-institutionalism.

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1 Rorty 1983, p. 562

Ask them. The expert practitioner of neo-institutionalism might reply, if he for some reason wanted to save the admittedly limited definition, that rules can be written or tacit (but possible to bring to consciousness) or even irrevocably subconscious. Yet (he would then say) all such constraints involve costs and rewards – which implies thinking of institutions as relative prices, budget lines facing given tastes.

Whew! Safely back to Samuelsonian economics.

The final step, though, is only justified even as price theory if you slip in an additional assumption that the rules or costs or constraints or whatever cannot be affected by (to use Albert Hirschman’s vocabulary) Voice or Loyalty, or by (to use my vocabulary) the virtues of Courage, Hope, Temperance, Love, Faith, or Justice, at any rate not by enough to matter greatly to the outcome.

I’m reminded of the move that Machiavelli, Hobbes, Mandeville, Bentham, Becker, North, and others take. They declare in effect that “dramatic economic betterment [or whatever the behavior is: getting married, having a child, following a contract, obeying the law, not obeying the law] has an element, however minor, of the undoubted virtue, Prudence, one of the seven primary virtues in the Western tradition. Therefore [non-sequitur alert] the behavior is *all* Prudence. *Forget about the other six virtues!* Maximize a utility function subject to constraints. And proceed to tenure”. In 1725 Bishop Samuel Butler complained about “the strange affection of many people of explaining away all particular affections and representing the whole of life as nothing but one continued exercise of self-love”.<sup>2</sup> “It is the great fallacy of Dr. Mandeville’s book”, wrote Adam Smith in 1759, “to represent every passion as wholly vicious which is so in any degree and any direction”.<sup>3</sup> By “vicious” he meant prudence-only, the sociopathy of Max U driven only by the costs and rewards of the rules of the game.

Smith, as a virtue ethicist in the precise sense of the Western tradition from Aristotle, Cicero, and Aquinas, with analogies in Chinese and South Asian traditions, disliked excessive reductions. “By running up all the different virtues . . . to this one species of propriety [namely, ‘the most real prudence’], Epicurus indulged a propensity”, he noted, “which philosophers . . . are apt to cultivate with a peculiar fondness, as the great means of displaying their ingenuity . . . to account for all appearances from as few principles as possible”.<sup>4</sup> It is Ockham’s Razor, with which so many male philosophers and economists have cut themselves shaving. Parsimony, after all, is not the *only* intellectual virtue. Smith therefore in substance avoided the utilitarian pitfall – into which Hume gazed fondly and into which Bentham eagerly leapt, and in which Samuelsonian economists such as my friend Douglass North and my friends assembled here wallow happily – of reducing all other virtues to prudence only, Max U.

I claim neo-institutionalism is Max U Redux. In fact, I venture to go further. I realize that it is going to make people angry at me (for which I apologize, as the politicians say, if I offend you). But I need to make the point about neo-institutionalism

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<sup>2</sup> Butler 1725, Preface, p. 349.

<sup>3</sup> Smith 1759 (1790) VII. ii. 4. 12., p. 312.

<sup>4</sup> Smith 1759 (1790), VII.ii.2.14, p. 299.

in the history of economic thought, because the point is true, and if we are going to understand what we are doing in economics we have to get it straight. It is that the North-Acemoglu program is deeply unoriginal, being, as I just suggested (and show at length in the trilogy of books on The Bourgeois Era [2006, 2010, 2016]), deeply Samuelsonian. Doug has persuaded most economists that on the contrary neo-institutionalism courageously rejects conventional Samuelsonian economics; that it is new, new, new; and that he made it up, with help from Steve Cheung; and that he (here it is not Doug – who knows better – but his followers who make such an assumption) got the Nobel prize for doing so.<sup>5</sup>

Doug has achieved such a spin on intellectual history by claiming persistently, in every paper or book he has written since about 1980, that “neoclassical” (better called “Samuelsonian”) economics misses institutions. But neither it nor most other approaches to economics do. (Come to think of it, some of Doug’s own earlier and path-breaking work in economic history was in fact unusually neglectful of institutions; but not the work by most of his colleagues in the field.) I’ve been complaining to Doug about his doctrinal error ever since 1980, pointing out to him again and again, without fruit, that institutional analysis is as old as economics. For example: Smith’s analysis of the political economy of mercantilism; Mill’s and Marshall’s [incorrect] analysis of sharecropping as a constraint and a social habit; Schumpeter’s and Israel Kirzner’s [correct] analysis of entrepreneurship embedded in social custom and psychology; Fogel’s and Engerman’s [probably incorrect] assignment of the gang system as an explanation of Southern productivity; Mokyr’s analysis (in Kelly, Ó Gráda, and Mokyr [2013]) of Britain’s skill-levels for betterment in the eighteenth century; my own analysis of open fields in the fourteenth century (McCloskey 1976); and on and on and on. Economic history in particular, whether or not it uses as I do “English economics” (as the German historical school derisively called it), has always been massively about institutions and their impact on the economy, for example on economic growth.

My friendly acquaintance Guido Tabellini (2015) says here that my characterization of what is mainly going on in neo-institutional economics – a tale of the elimination of (mere) Harberger triangles of inefficiency – is unfair. He says that *a contrario* it is about Good Institutions which “allow the economy to exploit dynamic gains from enterprise, investment and innovation”.

His story would be nice if it were correct (though, by the way, “investment” is not to the point, being derivative from enterprise and innovation). Most economists in fact understand neo-institutionalism, whether they read it or practice it, as being about static efficiencies redeemed. Again, ask them. Perhaps they adopt such an understanding because they teach their students or clients relentlessly about  $MC =$

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5 My prose invited in 1993 by the Nobel [Memorial] Committee for North and Robert Fogel as co-winners of the glittering Prize was the basis for the Committee’s citation, containing my judgment and many of my turns of phrase. The prize was for inventing historical economics, not neo-institutionalism. On the other hand, Fogel even at the time could have been given it for just that, as for example when he attributed economic behavior to religious awakenings.

*MB* – even though the life of trade-tested betterment since 1800 has been creativity, not Max U. Whatever its psychological origin, I claim that economists have a deep confusion about the connection between efficiency (Peter Boettke calls it “Smith”) and trade-tested betterment (“Schumpeter”).<sup>6</sup> The confusion is evident, for example, in the economist and economic historian Robert Allen mixing up – as most non-economists do also – movements along a given production function and movements of the whole function.<sup>7</sup> The one is governed by routine relative scarcities, à la Samuelson. The other, which explains the great bulk of economic growth, does not in logic or in fact depend on such static scarcities. Acemoglu and Robinson, again, whom Tabellini claims make the dynamic point, invariably stress efficiency gains out of static incentives, and do not explain how trade-tested *betterment* would come from property rights. Property rights in slaves? In government offices? In latifundia? In the image of Mickey Mouse after 1998? One can reduce betterment to a matter of mere efficiency (and “investment”) by claiming that the disgraceful Venetian inventions of patents and copyrights led to the optimal pursuit of knowledge. But Venice after the fifteenth century did not see an industrial revolution and did not share in the Great Enrichment until the late twentieth century.

In fact the program of neo-institutional economics in almost all its (in the words here of Greif and Mokyr) “strands that resemble one another much less than Professor McCloskey’s lumping of them would suggest” preserves Max U regardless. That’s why I lump them. As Greif and Mokyr further declare, neo-institutional economics is wonderful at “incorporating institutional features in neo-classical economics, even without violating the rationality and self-interest assumptions central to it”. That’s right. Greif’s focus in his work on a non-cooperative game theory that has repeatedly been shown in experiments to be nothing like the whole life of humans is a case in point. Mokyr is wiser in his historical work, which recently has allowed ample room for virtues other than Prudence. As I said in an early version of my paper (I am hoping that the editor will indulge me now), neo-institutional economics is Samuelsonian economics in drag.

Greif and Mokyr wax eloquent in denying that either of them is wearing a dress and high heels. They claim:

We’re not all Max U’s anymore. Her criticism [of Max U] is especially otiose, however, because the literature has long recognized that rules are, well, rules and that motivation is the linchpin of institutions. One work that expresses this view is Greif (2006). He noted that rules “are nothing more than instructions that can be ignored. If prescriptive rules of behavior are to have an impact, individuals must be motivated to follow them. . . . By ‘motivation’ I mean here incentives broadly defined to include expectations, beliefs, and internalized norms” (p. 7).

But such a construal of “rules” turns the economics into a tautology, which Greif on the evidence of the passage here does not grasp. If you define “incentives” so broadly that

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6 Boettke 2011.

7 Allen 2009; see McCloskey 2010, pp. 188-191.

they include “expectations, beliefs, and internalized norms”, then you can fit into them any behavioral evidence you wish, without scientific content, at any rate if you have no believable account of expectations, beliefs, and internalized norms, insisting on a rigid behaviorism ignorant of the humanities. If peasants in medieval open fields in fact had more plots than could be explained by the prudence of portfolio diversification, then the historian can *gesture* to expectations, beliefs, and internalized norms, and can go home early. He can leave a note on the door saying that regrettably the actual strictly behaviorist and mechanical and anti-humanistic study of expectations, beliefs, and internalized norms is in its infancy, and so he cannot, alas, be more specific. Stay tuned.

Yet the same is *not* true of serious scientific uses of the humanities, as I illustrated in the paper in the *Journal* with the long riff at the end on John Searle’s analysis of institutions (a riff that, sadly, no one here commented on; I am deeply hurt). Humanistic study is nothing like in its infancy. It is four millennia old. If we make use of it we *can* be highly specific in gathering evidence on “motivation”.

Still, Mokyr and Grief are vexed that I keep giving them reading lists in the humanities. I must say I am astonished by their vexation. I myself admit that I have not read all the works in neo-institutional economics that the critics gathered here cite. I am *ashamed* that I haven’t, and promise to try to do better. I thought this was the way we do things in science – giving out reading lists, testing one another, discovering our hidden presuppositions, many of which can in fact be discovered by serious listening to literature and its literature (called the humanities, *Geisteswissenschaften*, *sciences humaines*). Science is difficult. We’re not supposed to whine that it’s too much *work* to listen, really listen. A long time ago, in a group of admiring grad students and faculty at the University of Iowa’s narrow Department of Philosophy, I asked John Searle, whom I know a bit and whose books are on the reading lists I give out, whether he had read Hegel. John replied, “No, and I intend never to do so”, at which we all (even I, to my shame) laughed, signaling a [purposely ignorant] scorn for the whole of what is known in the trade as Continental philosophy.

I expected Mokyr and Greif to be better than Searle. But then I read the snarky complaint that my “accusing them of ignoring a wide literature that might be relevant somehow without specifying how, precisely, they should deploy the ‘exact and gigantic literature about ideas, rhetoric, ideology, ceremonies, metaphors, stories and the like’ seems to advance us by very little”. I have shown “how, precisely” such evidence advances us – for example, by offering a plausible and elaborately tested explanation of the Industrial Revolution and the Great Enrichment in the three big books I mentioned (2006, 2010, 2016), filled with quantitative and qualitative evidence. Earlier I had shown how the study of rhetoric could expose the ridiculous econometric routine of significance testing. Believe me (as Donald Trump says in every other sentence), the humanities can give great scientific insight and evidence – for example into the main scientific question in economics, how the Great Enrichment happened, 1800 to present. If we ignore evidence it is going to advance us by very little in our science.

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My amiable critics here all say that neo-institutionalism *does* acknowledge the humanities, through a thing they all label “culture”. Even another of my friends here, the highly sophisticated Richard Langlois (2015), uses the C-word. It is so baggy that, like “institutions”, little is conveyed. Still, if I were certain that my beloved colleagues had listened, really listened to the experts on “culture” from Homer and Hillel to Rabindranath Tagore and Tennessee Williams I would not be so resistant as I am to their use of the term.

Thus Langlois: “the principal culprits for actually stopping entrepreneurship are culture and institutions. McCloskey is now willing to admit, perhaps reluctantly, that both of these factors can be important. But the point here, and in *Bourgeois Dignity* (McCloskey 2010), seems to be that culture is what does the heavy lifting”. I demur, because of the vague “culture” word Langlois and all the others use, a baggy error term that can be brought in at any moment to save the hypothesis. By contrast, we have, if we will only listen, numerous precisely categorical and even ratio-scale-quantifiable studies in the humanities that can help us understand how ideas, ideology, rhetoric, ethics actually change. But we have to attend to them.

The word “ethics”, for example, is much more exact than “culture” (and so are “ideas”, “ideology”, and “rhetoric”, unless we stick with a sneering positivist method that claims without attending to the evidence that they are all “vague”). In 2006 I wrote a long book about ethics, but finally realized two books later that what I was getting at was not really Ms. Jones’ personal rectitude (which is what everybody thinks when they hear that McCloskey is talking about “bourgeois virtues”; even Mokyr, who should know better, falls into such a reading) but Jones’ attitudes formed in the human conversation *about* the rectitude of other people. I call it in *Bourgeois Equality* (2016), clumsily, “social ethics”, the indignation, for example, that is *not* expressed in Italy when *il furbo* sneaks ahead of everyone else at the ticket office in the Rome railway station.

Yes, I realize that economists since Robbins (as Langlois points out) have fled from the mere mention of ethics. They know nothing about it, believing it to be merely the preaching of stupid commandments, and don’t want to learn anything about it beyond Hicks-Kaldor compensation. Perhaps in this matter – I think Langlois would agree – we ought to go back to our master, the Professor of Moral Philosophy at Glasgow, 1752-1764.

I don’t think institutions work without a *great deal* of social ethics – think of the constitutions of the USSR or the Russian Federation; think of the laws on rape being the same in Uganda and in the United Kingdom, with very different results. Abraham Lincoln declared in the first of the Lincoln-Douglas debates of 1858, “With public sentiment, nothing can fail; without it nothing can succeed. Consequently he who molds public sentiment goes deeper than he who enacts statutes or pronounces decisions. He makes statutes and decisions possible or impossible to be executed”.<sup>8</sup>

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<sup>8</sup> Lincoln 1858 (1894), p. 298.

As in many cases here, I can use almost all of the examples offered by my honorable opponents to make my own, opposite, and Lincoln's, point. "Chinese people I have spoken with", writes Langlois, "do not see present-day Chinese culture as affirming or honoring of commercial activity. It is a culture of bald-faced pragmatism. We are all socialists (wink, wink), but it's okay to go about bettering our conditions". But the "honor" I speak of does not have to be treating entrepreneurs as superheroes, in the way we treated Steve Jobs (a nasty case, actually, of one sort of *lack* of social ethics in his treatment of people; though a good case of social ethics in his treatment of devices that people loved and would pay for). Such a modest "honor" sufficed in Britain in 1800 or China in 1978 to produce astounding results, considering the depth of earlier disdain. "Pragmatism" looks like an entirely new social ethic of, as Deng Xiaoping put it, "To be rich is glorious".

And so I do not understand what Langlois means by saying that "the Red sultans (mostly) stopped throwing improvers off the cliff, and the Chinese Empire is becoming rich. *That's an institutional change, not a cultural one*" (italics supplied). Again the word "culture" obscures the matter. Ethics *did* change—among the elite. Langlois seems to think that "cultural" change must be widespread to have effect. It shows how sloppy ones thinking gets if one sticks with "culture" without really taking on board what anthropologists, philosophers, and philologists have said about it. The spread through the society is of course relevant, and can be measured by the mind-scan on dead people the humanities offers. But if enough Ben Franklins or Count Bismarcks or Vladimir Lenins change from workers to entrepreneurs, or from liberals to imperialists, or from law students to revolutionaries, a lot can happen.

Langlois does, though, understand what I am saying: "McCloskey's thesis would seem to be that economic growth will take place if the system of convention (culture [yuk!], ethics) makes it legitimate to take advantage of commercial entrepreneurial opportunities". Langlois quotes the astonishing book by Young Back Choi in 1993: "Of the many factors relevant in determining entrepreneurial success most notable is the role of property rights in enabling entrepreneurs to overcome envy barriers, making possible the market process of social learning".<sup>9</sup> But then Langlois extends it: "Even if the culture is against me, I have a trump card in abstract and anonymous social institutions".

No you don't—not if ethics, ideology, rhetoric, the conversation of humankind is trumps against the particular anonymous social institution that would enable you to overcome envy barriers. The market, for example, doesn't *perfectly* eliminate racial prejudice. It helps, but not 100 percent. Langlois quotes Schumpeter in 1934 writing that "the only man [the entrepreneur] has to convince or impress is the banker who is to finance him—but by buying them or their services, and then using them as he sees fit". That is only true, as Choi's claim is true, *after* the Bourgeois Revaluation, part of the egalitarianism in economic rights and social standing that by a happy accident started to grow in northwestern Europe after 1517. Otherwise the property rights, the market

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9 Choi 1993, p. 153.

trump cards, and the writs of bankers would be dead letters, like the American constitution was for southern Blacks before the 1960s.

My friend Robert Lawson (2015) in the same connection here doesn't like my pairing of Italy and New Zealand, and urges me to do the two-variable regression on a "sample" of countries he exhibits. "But in my article (McCloskey 2015) I do not offer the Italian/N.Z. instance as the confirmation of a law, merely as an interesting *falsification* of such a law. Italy is notorious for public corruption, and yet does well enough in its private economy to offset the idiocy. Surely it requires explanation why such a country has about the same income as New Zealand, a paragon of honesty.

As to the econometrics, I need to remind Lawson that his two-variable regression is probably not a correct specification of the connections between income and corruption, a point he should have added to his brief concessions at the end. Doubtless a similar regression of the consumption of paper on national income would have similar results. But one would not want therefore to dump paper on poor countries to make them rich. A more profound example of the same point is William Easterly's attack on "capital fundamentalism": rich countries have lots of dammed-up rivers; therefore if Ghana makes such an artificial lake, it will become rich.<sup>10</sup> Surely high income itself independently reduces corruption, for various reasons that could be explored quantitatively. Chicago was very corrupt as recently as 1960, but with rising income (and education and other correlates of income) Chicagoans grew less tolerant of the Chicago Way (*Ubi est mea*: "Where's mine?"), and Fast Eddie Vrdolyak went to jail.

And in any case, as is true of all the commentators here, Lawson's point makes my own. Where would un-corruption come from? Not, as I have argued recently, from more laws, but from an ethical change – the sort that caused the Great Enrichment in the first place.<sup>11</sup>

The same reply can be made to all four comments, using their own examples to show that my argument is correct and theirs is mistaken. (I apologize for using such an aggressive trope as the *elenchus* on them, but urge them to set aside their anger and to listen, really listen.) Greif and Mokyr (2015), for example, give many interesting and important instances from British history in the eighteenth century – each of which, contrary to what they claim, shows that I am correct about the relative rigidity of institutions during the Industrial Revolution, and that ethics about other people's behavior, not institutions such as property law or whatever, is what changed.

They praise for example "North and Weingast's (1989) classic and influential paper on the topic and heavily used in empirical work on institutions", which characterization is certainly correct: it is a classic, and was influential, and is heavily and uncritically used by others in neo-institutionalism (as for example an alarming passage by Daron Acemoglu, which I dissect in *Bourgeois Dignity*, pp. 320-322). Yet the North and Weingast paper is mistaken as historical science, which I showed in pp. 310-354 of *Dignity*. Just one example among many of its mistakes: the taxation by which the

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10 Easterly 2001.

11 McCloskey 2015.



Stuarts are supposed to have terribly enslaved property owners, on the estimates in the North and Weingast paper itself, amounted to about 2 percent of English national income. Compared with the Dutch-imitating polity that was brought in with a Dutch king and the Glorious Revolution, and compared with almost any modern state, the Stuarts were pikers at extraction of income for economically pointless foreign wars. So much for property rights being overridden by the English state *before* 1688. (It is by the way strange of Greif and Mokyr to note that McCloskey “never refers to the North-Weingast paper”. It seems hard to attack me for neglecting this or that item in the literature in question but then to pointedly ignore a 45-page dissection of the very item in one of my books, which at least Mokyr claims to have read. But I should not whine.)

“By the late eighteenth century, however”, Greif and Mokyr write again, “contemporaries recognize the role of poor relief in fostering disruptive and labor-saving innovations”. It is a fine example (out of a half dozen in the later pages of their piece) of precisely the change in ideology that I have written the trilogy on. When “contemporaries recognize” it’s a case of public sentiment which makes statutes and decisions possible or impossible to be executed. It’s not a change in institutions---which is to say it is not something that can be ordered up by rules of the game. With such sentiment on his side, Lincoln realized, the rules of the slavery game could be changed.

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Langlois wants me to engage more with Schumpeter. I think he will be pleased by *Bourgeois Equality*, whose text (excluding, that is, citations in footnotes and bibliography) discusses substantively the insightful Joseph Alois more than thirty times. I have become a Schumpeterian, after a misspent youth assaulting his quantitatively challenged epigones such as David Landes and Peter Mathias and Derek Aldcroft. I only criticize the Master now for putting far, far too much weight on bankers (for a while Schumpeter was one himself, by the way) – not realizing that banking is ancient, not something peculiar to quattrocento Italy. And especially I still criticize Schumpeter for not having a serious sociological or historical account of how and why entrepreneurs were unleashed.

It is true, as Langlois says with characteristic precision, that “entrepreneurship is not a hothouse flower that blooms only in a culture supportive of commercial activity; it is more like kudzu, which grows invasively unless it is cut back by culture [gak!] and institutions. McCloskey needs to tell us more about the structure of the relationship among culture, institutions, and entrepreneurship, and thus to continue the grand project begun by Schumpeter”. I’ve followed Langlois’ advice and told people a good deal along such lines in *Bourgeois Equality*. The structure I have in mind, which I recommend to others as a testable scientific hypothesis, is that ethics and rhetoric, which can be given exact content and which can be seen in action in law and literature, were hostile to entrepreneurship in every large-scale society until the Netherlands in the sixteenth century and Britain in the eighteenth century.

Langlois is spot on when he notes that:

If you attend a meeting of the International Joseph A. Schumpeter Society, you can hear many papers on “entrepreneurship policy”, which

seeks an activist role for government in somehow fomenting entrepreneurship that would not otherwise take place. This is especially popular in Europe. . . . They don't want to stop doing any of the many policy things that discourage entrepreneurship.

Some years ago a few of us were at the end of an exhausting three weeks rushing around the capitals of the Balkans recommending liberty for entrepreneurs to have a go. The last meeting was in social-democratic Vienna, before a small audience of bored journalists and a few members of the public. We gave our talks. At the end a young man, about 20 years old, stood up and effused in excellent English, "I *loved* your talks, and *love* the idea of entrepreneurs having the liberty to have a go. *But* . . . in Austria you have to understand that we have a problem. There is *no government program for training entrepreneurs*". We didn't want to criticize such an enthusiastic young man, and so we merely smiled, and sank back in despair into our seats.

Tabellini praises, as North and company do, the modern state for "its ability to establish order and deter violence, to enforce contracts, to provide public goods" in aid of economic development," "entrepreneurship policy". It is surprising to see an Italian supposing that most states are in such a benevolent business. I recall the opinion of his countryman, Carlo Levi, who as an Italian and Jew and antifascist had little patience with the widespread modern notion of the State as savior, writing of the towns in Calabria to which he was banished in the late 1930s:

None of the pioneers of Western civilization brought here his sense of the passage of time, his deification of the State or that ceaseless activity which feeds upon itself [thus the Great Enrichment]. No one has come to this land except as an enemy, a conqueror, or a visitor devoid of understanding.<sup>12</sup>

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Mokyr wrote in earlier correspondence with me that "you are of course correct in that institutions must be understood in conjunction with beliefs, that is, culture". I have said why the rush to call the very precise findings of the humanities ranging from philology to anthropology by the baggy word "culture" is a scientific error. Now I want to draw attention to a widespread error in how economists think about "beliefs", especially evident in Greif/Mokyr and in Tabellini, namely the error of taking beliefs to be "information", and of a restricted sort, namely, probabilities about states of the world (though of course sometimes mistaken). Doug North thinks of "beliefs" as "information", again reducible to (1.) budget lines or, as he likes to say now (2.) "brain science". (Somewhat cruelly in the article I pointed out that he has not bothered to learn anything scientifically concrete about "brain science"; and like most social and behavioral scientists he ignores the 4,000 year old reflections on belief known as literature and the humanities).

Beliefs are *not* merely propositional, such as my own belief that natural selection explains the origin of species. Think about it. Beliefs are also dispositional and practical, that is, matters of identity, matters of rhetoric and ethics, matters of what sorts of

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12 Levi 1945.

propositions one is willing to entertain, and what ethical attitude one takes towards them. And therefore they are also “speech acts”. (I have been saying this to economists – that is, mainly to deaf ears – since 1983.) Thus, “belief” in Science, such as you and I possess, is not reducible to accepting propositions such as  $F = ma$ . It is a disposition, which in some degree changed during the seventeenth century. Here again the humanities comes to our scientific aid. The word “belief” – cognate with “love”, as “true” is cognate with personal “troth”, as in “betrothed” – means in religion before natural theology in the eighteenth century *not* propositional belief in, say, Snell’s Law of Refraction, as Karen Armstrong has pointed out, but rather a loyalty to a person or to a way of life, the following for example of what Jesus would do, or the 613 laws of Orthodox Judaism, or the scientific program of Francis Bacon. Luther had explicitly denied the propositional definition of faith: “Faith does not require information, knowledge, and certainty,” he wrote, “but a free surrender and joyful bet on His unfelt, untried, and unknown goodness.”<sup>13</sup> Commitment. I pledge my troth, as to the unreasonable effectiveness of mathematics in the physical sciences.<sup>14</sup> Mokyr and Greif are certainly correct to declare that “it would be useful to know how and why people believe what they believe and how they change their minds”. But that is what the history of civilization from the Torah and the Vedas and Cicero down to John Searle and Mary Midgley and Clifford Geertz is about.

Greif and Mokyr declare that “An institution as a system of rules, beliefs, expectations, and norms perpetuates only if it elicits behavior that is consistent with the rules, that reaffirms the associated beliefs and expectations, and that replicates its underpinning norms”. If this rotund expression is understood as anything other than a tautology (achieved by canny definitions *ex post facto* of “reaffirms” and “replicates”, for example), it is another materialist lemma. It looks lovely on the pages of the *Journal of Economic Theory* but looks odd coming from Mokyr – who has brilliantly shown the force of ideas, such as the mad ideas of Francis Bacon on scientific progress for prosperity, taking two centuries of failure to come close to eliciting behavior (namely, true enrichment) reaffirming the belief.

But in any case I would like to see the historical evidence that it is true. If you turn over books of history it looks, actually, absurd. Take belief in astrology, for example. Are we saying that casting horoscopes was “reaffirmed” by the outcomes of lives and battles? Or that the resistance for fifty years by American geologists to the plausible argument of 1916 by a German meteorologist that continents moved reaffirmed the underpinning norms of science? Or, to use an example I have been trying to get economists to understand since the 1980s, consider the institution of econometrics and in particular its reliance on null hypothesis significance testing in the absence of loss functions, such as what Greif and Mokyr call breathlessly “recent cutting-edge economic research”. The “underpinning norm” is surely that we need to get magnitudes when we assert an economic effect, and might get them from properly

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13 Luther, Sermon 25:7, quoted in Armstrong 2009 (2010), p. 171.

14 Wigner 1960.

identified observations. Good. But it has been shown by dozens of the leading theoretical and applied statisticians since Edgeworth that such testing does *not* replicate such a norm. Not at all.<sup>15</sup> Yet econometrics goes on and on and on, taught to graduate students in such a way that they are disabled from thinking quantitatively. That's "perpetuates" for you.

Citing some econometric studies of US states and elsewhere that define "religion" as ignorant fundamentalism, Tabellini asks, "How do we know that it is the diffusion of bourgeois ethics (and the associated appreciation of innovation with commercial value), rather than openness to innovation in general, that is responsible for rapid economic development?" How indeed. "Innovation in general" – in music, for example, in seventeenth-century Italy or in poetry in Shakespearian England – might be a substitute for trade-tested betterment in the economy. It probably was, as an alternative path for a creative person. Or it can be, as Tabellini is claiming, an indicator of a general ability, like IQ. That last one I doubt, and offer piles of evidence in *Bourgeois Equality* that it is not so. For instance, the success of overseas Chinese before 1978 while their countrymen languished suggests that no general "Chinese" ability was in play. What all my critics here miss is what I thought I made clear in *Bourgeois Dignity* (in *The Bourgeois Virtues* I was myself confused about it in the same way that they are): that "ethics" is to be taken as mainly *not* as individual character but as the opinion about character in the society at large. It is a sociological not a psychological matter.

On the other hand, I quite agree with Tabellini that "generalized" ethics (about other people's character) is what matters for economic success. The crux, as I have just said, is to realize that the ethics that matters is not so much how people are constituted as it is how they view other people. (In the work with Greif he cites, though, it does seem strange to attribute tribalism, correctly, to China but then not to acknowledge its gigantic role in Europe also. *All* pre-industrial societies are organized tribally. A long time ago Edward Banfield, for example, characterized the *mezzogiorno* as running on "amoral familism".) Anyway, such a generalized ethics is what in *Bourgeois Equality* I stress, and study, and explain. In a word, the crux was the rise of liberalism, the crazy theory that all men (and women, dear) are created equal.

Tabellini cannot be blamed for not reading a book by me that has not yet come out, *Bourgeois Equality*. On the other hand he criticizes me for not reading his own works, so I suppose turn about is fair play (*L'inversione è . . .*). It would not take much inquiry into what I have written elsewhere recently to find out that I have a massive answer to his "second obvious difficulty with an ideas-based explanation of economic and political development is that ideas are endogenous. Where do these ideas come from, and why do they spread so rapidly in some places or moments in time and not others?" The blithe supposition that "ideas are endogenous", as Tabellini declares with no sense of its scientifically erroneous character, is a materialist dogma, true, but not therefore obviously correct as science. One would like to know, for example, how Einstein arose as a merely super-structural result of the means of production at the base.

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15 Ziliak and McCloskey 2008, p. 2.

Or more to the point here, one would like to know what the history of our field would look like without, say, Smith, Schumpeter, Samuelson, and Arrow.

But in any case, where the ideas come from and how they spread (slowly, not as Tabellini says “rapidly”, though quicker than the institutions whose change is supposed to have caused the modern world), and why in some places and not in others, is the subject of *Bourgeois Equality*. Briefly put, the liberal idea arose from the accident in northwestern Europe from Amsterdam to New Amsterdam of the capacity to Read, the Reformation, the Dutch Revolt, the English, American, and French Revolutions – the Four R’s, if you please, not apparent in other places. The Reformation, for example, did not have its main influence through the anxiety about election that Max Weber stressed, but the dignifying of ordinary believers in the Radical Reformation (as against what historians call the Magisterial Reformation of Luther and Calvin and Henry VIII). There is more to be said, 750 pages more, actually, but I want you to buy and read the book. Especially buy.

In his strictures on my methodological points, Tabellini again retails positivist dogma. He says as though it were uncontroversial that “we ought to explain social phenomena starting from the decisions of individuals”. Who says? What about our friends in Post-Keynesian economics, for example, who deny it? I sometimes joke about my beloved graduate-school classmate Tom Sargent that Tom says, “We *must* base macroeconomics on micro-foundations”. And then in the joke I ask Tom, “But why?” To which Tom answers, “We *must* base macroeconomics on micro-foundations”. To which I ask again, “But why?” To which Tom answers . . . . Another example is Kant claiming on the second page of *Groundwork of the Metaphysics of Morals* that we *must* not use anthropology – that is, what we know about humans – in framing ethical principles. We *must*. But no reason we “must” is supplied, there or anywhere else in Kant.

Having repeated the usual and startlingly amateurish philosophical dogmas prevalent among economists (if Tabellini wants to get serious about the philosophy I highly recommend a book of 1994 called *Knowledge and Persuasion in Economics*), he outlines how the mind can be reduced to economic incentives – the materialist postulate again. He does not seem to be aware of Bart Wilson’s devastating criticism of locating a “taste for justice”, for example, in the utility function of individuals.<sup>16</sup> Tabellini, like my other dear friends here, wants us in economics to go on as before, ignoring the humanities, and refusing to learn anything from *le facoltà di lettere* – even though in Italian and every other language (if not in English since the late nineteenth century) they teach *scienza*, too.

## §

And that, my dears, is my point, fiercely denied in Samuelsonian neo-institutionalism.

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16 Wilson 2010.

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