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A Neo-Institutionalism of Measurement,

Without Measurement:

A Comment on Douglas Allen's *The Institutional Revolution*

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Abstract: In his elegant book Douglas Allen 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. A fall in wedge of inefficiency is supposed to provide Good Incentives, and the modern world. But the elimination of wedges lead merely to Harberger Triangles of improved efficiency—not to the factor of 100 in properly measured real income per head, which is the Great Enrichment 1800 to the present to be explained. Allen does yeoman work in explaining some of the peculiarities of British public administration, such as the reliance on aristocratic honor and on the prize system in naval warfare. But he attributes to *public* administration an implausible effect on *private* incomes. The merging of power and plenty 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.

Allen's book on measurement 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.

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JEL classifications: N00, O4, D8, Z1

In his deeply researched and elegantly written book Douglas Allen proposes explanations of many interesting puzzles about how the British government worked in olden times. It is a brilliant book, which I much admire, and from which I learned a great deal.

Yet I don't believe its main argument, not at all. I'm going to devote my space here to telling you why, and not praise much further a most praiseworthy project.

Allen's central argument goes like this. Before 1800 even the *British* government, that envied instrument of imperial aggression, could not measure excellence in its servants very well. Therefore, in the same way the criminal code of the time would hang people for stealing even a little because the courts and the non-existent police could not actually do much to increase the probability of apprehension, the government instead made foul-ups very, very expensive. The classic example for the mechanism of high penalties in an environment of low information (the fog of war, one might say) is the execution by firing squad of Admiral John Byng for failure to do his utmost in 1756 in the Battle of Minorca. Voltaire remarked famously that "in that country [he was a great admirer of Britain—though no one, including Voltaire, thought justice had actually been served in the case] it is good to kill an admiral from time to time, to encourage the others." Transportation to Australia for poaching even one of the landlord's rabbits served to *discourage* some other of the others. In parallel fashion, and for the same reasons of uncertainty, the British government, to get things done, had to rely on honor—"trust and hostage capital," Allen calls it—because, he claims, Britain lived in "a world dominated by the large role of nature [for example, the vagaries of getting and holding the weather gage in a sea battle], which in many contexts precluded the measurement of merit" (p. 218). It was hard to measure the influence of Nature. When measurement became better we got a modern world, which according to Allen does *not* depend on honor and trust and hostage capital.

My basic problem is that Allen, in offering measurement as the explanation, does not measure the measurement, either as a cause or in its effects. He does not so much as offer an adumbration of potential measurements. Instead he models possible explanations, in a style that has become fashionable as an "analytical narrative," in which typically, and also in Allen's case, the modeling far outweighs the empirical evidence that the parameters in the model are in fact of the magnitude required to have the effects claimed for them. I blame the admirable Paul Samuelson and his brother-in-law Kenneth Arrow for setting modern economics off in pursuit of "qualitative theorems"—though in truth from Ricardo onwards, with or without mathematics, economists have been inclined to rest easy with amusing games of modeling without measurement. The trouble with *just* modeling is that

an infinitude of models can explain any given effect, a trouble I have called the “A-Prime-C-Prime Theorem” (McCloskey 1991, 1994). Since modeling without measurement is what is scientifically disappointing about most of the work in the New Institutional Economics, from Avner Greif on down, it is worth taking Allen’s excellent work as a hard case in point. If there is a serious problem with such fine work in the New-Institutionalist style as his, perhaps we should worry about less fine work, such as Douglas North *Understanding the Process of Economic Change* (2005) or Daron Acemoglu and James A. Robinson, *Why Nations Fail* (2012).

I have come to advocate in my old age what Bart Wilson and I call “humanomics,” a science of the economy that takes seriously the numerous ways beyond meaningless *t* statistics and pointless existence theorems by which we economists Know. Novels. Linguistics. Philosophy. But a proper humanomics includes measurement, too (though well beyond the narrow writ of official econometrics, all the way to simulation and charting and mapping and experimenting and listening). When magnitudes are an important part of the scientific issue, as they are in explaining the miracle of the modern economy, they must be attended to. Allen doesn’t attend to magnitudes. I do not mean that he does not offer up irrelevant tables and worse-than-irrelevant *t* tests. Thankfully, he does not, showing in this as in many other matters his excellent scientific taste. I mean that there is no sense given anywhere in the book of How Big this or that cause or effect might be.

(Lest the evil thought is forming in your head, dear reader, that the very Literary Deirdre is here a pot calling the kettle black, consider the following anecdote [one way we learn, class]. An otherwise insightful friend said of my own last book, *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 regression in sight. Yet on every page, and usually many 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 it out to him, my friend agreed, and apologized. A most learned gentleman. Trained, actually, in an engineering school.)

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One problem created by the absence of How Big is that Allen’s ingenious examples of *governmental* systems of incentives have no scientifically plausible connection to *private* prosperity. The state’s systems were small in those days relative to private activity, and they bulked large only during the incessant French wars down 1815, in which the state was busy throwing away economic output to no gain. My city of Chicago was from 1870 to

1900 the fastest growing city in the world, in its large private sector a wonder of innovation (for example, steel-frame skyscrapers; for example, reinforced concrete skyscrapers; for example, mass processing of meat). But it was also in public matters fantastically corrupt (I can give you the relevant magnitudes), depending on a patronage system like the one Allen thinks was such a drag on modernization in Britain (for all its optimality in the conditions of the times, as he wisely insists). In Chicago as late as 1948, when an idealistic young law student, Abner Mikva, wanted to volunteer for the Democrats, the ward committeeman asked him, “Who sent you?” “Nobody sent me.” “We don’t want nobody that nobody sent.” Samuel Pepys and Robert Walpole couldn’t have said it better. Yet Chicago did just fine economically, thank you very much. It had for decades the largest sea port in the world, handling grain and especially lumber. “Hog butcher for the world,/ Tool maker, stacker of wheat/ Player with Railroads.” Such signs of economic growth didn’t depend on City Hall. And likewise Britain’s enrichment did not depend on Crown patronage. Its armies and navies did. But not its ironworks and cotton mills.

Allen’s argument, admirable as are many of its details, is the opposite of the one I would favor. He believes, with North and Liah Greenfeld and Patrick O’Brien and recently Prasannan Parthasarathi, that a powerful British *government* was a precondition for economic growth. I say that it was mostly an obstacle, in the usual ways, diverting activity into rent-seeking and military waste. “Trustworthy service to the Crown,” which is Allen’s touchstone, was no road to private economic growth. He says, “Britain, by becoming the most aristocratic of all societies, also became the wealthiest and the most powerful” (p. 79). “Most powerful,” yes: a Navy that practiced gunnery fanatically under the eyes of faux-and-actual aristocrats whose whole identity was tied up in warfare was for that purpose a Very Good Thing. The aristocracy ran the *public* sector in Britain for a long time. The last British cabinet still having a majority of literal aristocrats (a relevant measure, considering the tiny size of the British peerage) was surprisingly late, Gladstone’s of 1892. Thirty years later in Bonar Law’s there were still equal numbers of peers and commoners. Thatcher’s of 1979 still contained nearly a quarter from the “landed establishment” (though some quite recently recruited to it). But “wealthiest” had to do not with Britain’s aristocracy, nor even its much larger gentry, but with its bourgeoisie. The *economy* of the “polite and commercial people” was in the hands of the bourgeoisie and its non-aristocratic values from 1730 or so at the latest.

And Allen does not compare, and therefore does not get much beyond the English Channel. Historians or economists focused on one case, such as Britain, are liable to overlook similar conditions elsewhere that belie their celebration of, say, English (but not Scottish) common law or British (but also French) empire. In a recent book

modestly subtitled *A Conceptual Framework for Interpreting Recorded Human History*, Douglass North, John Wallis, and Barry Weingast overlook all recorded human history except England's, France's, and the United States', and treat even the Blessed Three partially, and often enough erroneously. (The reader is therefore startled when reading the book to find mention of *Spain* on fully 8 pages, with *Rome* on 7 pages and *South Korea* on 1, on a scale of attention given by France on at least 30. These are notable deviations from a "recorded human history" that consists otherwise of England, France, and the United States. Their index contains no entries for Africa, Arabia, China, Germany, Greece, Iran, Italy, Japan, Sweden, the Ottoman Empire, the Mughals, the Netherlands, Russia except the USSR.)

To be fair, Allen has enough on his hands getting straight the array of British historical facts he has in fact gotten straight, a good show. But to prove his point—quantitatively, so to speak—he needs to explain why other aristocracies and service classes, such as the Russian, Ottoman, Chinese, Prussian, and, especially, Tokugawa Japanese did not make their countries wealthy. "The purpose of the aristocracy was to provide a pool of trustworthy types," the better to do their utmost in naval or land warfare (p. 65). All right, suppose it was. Why not samurai riches, then? It won't do to reply that at the Meiji Restoration many of the samurai did in fact go into commerce and industry. They did so precisely by adopting bourgeois virtues, and ceasing dueling (Allen's chapter on dueling, by the way, is worth the price of the book).

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What are we actually trying to explain here? Answer: the Great Enrichment, the most important secular event in human history since the invention of agriculture. Nowadays our life in the most bourgeois countries of \$100 to \$200 a day is startlingly better than even the Old Prosperity before 1800 of \$6 or \$8 a day, and is enough for a life of adequate food and education even among the poor. About the lower end of the Great Enrichment, the economic historian Cormac Ó Gráda has recently documented the decline of famine (Ó Gráda 2009). Its higher end, enjoyed now by perhaps half a billion people, and each year by more and more, supports a flourishing life of loft apartments, art museums, higher education, adventure holidays, spiritual exercises, serious fiction, and all the ennobling and not-so-ennobling goods of a modern bourgeois town.

In other words, our message of hope in economic history when we lecture to undergraduates is that human welfare has shot up startlingly since 1800, giving human welfare in history a pattern like the handle *and blade* of an ice-hockey stick (many economic historians are Canadian or Swedish men, and delight in such talk). History

reached the business end of the hockey stick. In the next two centuries, despite an enormous rise in the world population, the world's average real income per head increased by a factor of at least *ten*, \$3 to \$32 a day. In places blessed by bourgeois virtues it increased much more. Nowadays a rich, bourgeois country like Japan or Switzerland produces and consumes \$100 a day or more, conservatively measured (in that it does not take adequate account of *better* lighting or medicine or, for that matter, economics). One hundred dollars a day on average in present-day U. S. purchasing power for every man, woman, and child had never happened before. Not even close.

How to explain it? Let me tell you another story about measurement. In the early 1970s I had Steve Cheung as an office mate at the University of Chicago, and Steve led me, as he later led Doug North, to reflect on property rights. Looking for the cause of the modern world, I therefore studied legal history for a while, Holmes, Pollock and Maitland, Holdsworth, Plunknett, Simpson, *et alii*, settling on the obvious legal change in the enclosure movement in English agriculture as a test case. (I tried contract law first, but found that it didn't change much until the nineteenth century, and so its improvement was ill-timed to cause the Industrial Revolution. And it was harder to measure than property law. I also found, by the way, that English contract and property law were well developed and enforced before the time of Edward the First, which is to say 1272. I wish people who listen entranced to North's fairy tales about English law and the Glorious Revolution would get this old point.)

My modeling in 1972, which was that of the New Institutional economics from North and Thomas in 1973 to Acemoglu and Robinson in 2012, and now Allen, was that in the bad old days a supply-and-demand equilibrium was obstructed by a transaction-cost wedge. I thought to myself, with my new-found grasp of price theory (which I followed Steve in teaching to the less well-prepared of the Chicago grad students), "Hmm. If the bad property law prevented land from being used efficiently, then an improvement in law, such as the enclosure movement, would allow marginal product to be equalized to marginal opportunity cost, would reduce the wedge, and, voilà, would result in higher income." It is the same sort of blackboard argument that North and others use when imagining that the introduction of patents in England in 1618 made invention into private property, and increased efficiency, and therefore (they believe) caused the Industrial Revolution (Joel Mokyr has recently devastated such a view in Mokyr 2009).

But what I found immediately, because I was so very simple-minded as to actually try to *measure* the magnitude of the effect (which is also Mokyr's naïve and unTheoretical approach to patents), is that the effect of so great an alteration as enclosure was nationally trivial, at any rate on the scale of what we really want to explain, the

Great Enrichment. You can see the influence here of my colleague at Chicago in the late 1960s and early 1970s, Robert Fogel, who had made such a case for the invention of the railways, and of my other colleague there, Arnold Harberger, who had given the general formula: (share of activity in national income) \times (percentage improvement of value-added, measured by efficiency gain) = national effect. It's what economists mean when they say that Harberger Triangles are small (and why Alito ought to get the Nobel Prize; he never will, alas, because of ignorant Swedish political prejudice about Chile).

So what? (Always the best question to ask in a seminar, class.) This: little Harberger Triangles of improvement are not going to explain a factor of 30 or (if quality is allowed for) 100. *And if they do, the model is unstable*, which is not a good way to model, since unstable models can prove anything, explosively, all over the place. That is, if you want to claim that (literally) marginally better ways of measuring inputs and outputs resulted in an industrial revolution, which is Allen's claim (and by the way, similar to *Robert Allen's*, too, no relation), then you are going to have to explain why small causes have grotesquely large effects, and then why they *didn't* have such large effects earlier or in other places, in Roman times or in China. You can't merely, in the style of New Growth Theory, introduce economies of scale when convenient, say in 1750, and where convenient, say Britain, to get a nonlinear, non-Harberger effect.

We economists have recently saved our models in the face of a new realization of how radical the Great Enrichment is, in other words, by going on with the same supply-and-demand models but adding in "nonlinearities" or "economies of scale" or "multiple equilibria." I am claiming that in the eighteenth and especially the nineteenth century the economy grew far beyond all previous expectations, and far beyond what static economics can explain, or even mechanically jazzed-up "dynamic" economics, because the forms of speech about enterprise and invention suddenly changed. Technically speaking, the new conversation caused the dimensions of the Edgeworth box to explode. Re-allocation by exchange within a fixed box, which is Allen's story, or reallocations by aggression along the contract curve, which is Doug North Mark II's story, was not what happened: instead, the production possibility curve leapt out. The habits of the lip changed in the seventeenth and especially the eighteenth century, for various good and interesting reasons—some in turn material, some rhetorical. Speech, not material changes in foreign trade or domestic investment or methods of measurement, caused the non-linearities or (in more conventional theorizing) the leaping out of the production possibility curve. We know this empirically in part because trade or investment were ancient routines, but the new dignity and liberty for ordinary people were unique to the age (if you don't

believe it, devote your days and nights to the reading of *Bourgeois Dignity: Why Economics Can't Explain the Modern World* [2010]).

The greatness of the Great Enrichment, in short, is the main intellectual puzzle in explaining the wealth of nations. Its greatness creates terrible problems of How Much for the usual allocative economics. Shuffling stuff around a little better is not the sort of stunning innovation that made the modern world. As Israel Kirzner expressed it, “for [the British economist flourishing in the 1930s [Lionel] Robbins [and the Samuelsonians], economizing simply means shuffling around available resources in order to secure the most efficient utilization of *known* inputs in terms of a *given* hierarchy of ends” (Kirzner 1976, p. 79). Yet the path to the modern was not through shuffling and reshuffling. It was not by the growth of foreign trade or of this or that industry, here or there, nor by shifting weights of one or another social class. Nor indeed was it by reshufflings of property rights, or their more exact measurement. Nor, to speak of another sort of reshuffling, was it by rich people piling up more riches. They had always done that. Nor was it by bosses being nasty to workers, or through strong countries being nasty to weak countries, and forcibly shuffling stuff towards the nasty and strong. They had always done that, too. Piling up bricks and money and colonies had always been routine.

The new path was not about accumulation or theft or commercialization or reallocation or any other reshuffling. It was instead about discovery, and a creativity supported by novel words. Allen notes that nowadays “we expect to have equal social status.” Precisely: a newly dignified bourgeoisie, invited to innovate radically. As Kirzner put it, such entrepreneurship is not about optimal shuffling—since a hired manager can carry out such a routine. “The incentive is to try to get something for nothing, if only one can see what it is that can be done” (Kirzner 1976, p. 84). A new rhetorical environment in the eighteenth century encouraged (literally: gave courage to the hope of) entrepreneurs. “Ours is a society,” Allen notes, “based on a concept [if not always a reality] of merit. . . . Not so long ago personal connections, conduct, and birth mattered much more” (p. 4). Bingo. As a result over the next two centuries the production possibility curve leapt out by a factor of ten or thirty or more.

Allen solves the problem of the Great Enrichment by stopping his analysis with the classic period of the (early) Industrial Revolution, 1750-1850. Robert Thomas and Douglass North had similarly declared in 1973 that “the industrial revolution was *not* the source of economic growth” (1973, p. 157). You *must*, they were claiming, start much earlier. Well, who says? Who says that all causes must be deep in history? Only if you stop the story of Europe in 1800 CE or even at a stretch in 1880 CE can you persuade yourself that the run-up to the Great

Enrichment is best viewed as being a thousand years, or five hundred, which saw a “sustained economic growth” of about a tenth of 1 percent per year. Bravo for the early-modern British. But if you carry on to the present you realize that the Greatest Enrichment is indeed the Industrial Revolution *and its amazingly enriching follow-on*—not a mere factor of 2 but, depending on exactly what you are measuring, of 10 or 30 or 100.

Some of my fellow economic historians, such as Stephen Broadberry and Gupta Bishnupriya and Robert Allen and Jan Luiten van Zanden, make much of that doubling of incomes in Europe before the Industrial Revolution. That way they don’t have to face the largest anomaly in world history of thousands of years at \$3 a day or so and then a leap in a couple of centuries to well over \$100 a day. They don’t have to face the problem that if little changes in law and “institutions” could have such astounding effects, surely the experiments in good property rights and budget-lines-all-in-order such as ancient Carthage or Kublai’s China would have had the same outcome.

Eric Jones attacks the view I favor that nothing much happened until 1800 (Jones 2010 pp. 27-29), instancing as he did in his *Growth Recurring* (1988) such “major growth phases” as early Song China and early Tokugawa Japan. Jones opines that “what kept growth episodes so few was mainly excessive rent-seeking on the part of the holders of political power” (p. 29). Probably. But the major growth phases were factors of 2 in rising income per head, not factors of 30 or 100. On a scale of human events, two is very, very far from one hundred, and Jones’ eloquence against the Nothing-New-Until-1800 folk (such as myself and Ken Pomeranz and Joel Mokyr and Jack Goldstone) is here misplaced. A rise by a factor of two in your ability to get food or shelter or education is nice. I recommend it, at any rate compared with zero rise per capita. But it is dwarfed by the factor of 10 or 30 or 100 that one discovers if the story continues after 1800 to the present. One-hundred percent of growth 1000-1750 CE stands in life-altering force far, far below the 900 percent of a factor of 10, not to speak of the 9,900 percent of a factor of 100 of 1750-2012 CE if the improved quality of medicine and messaging is taken fully into account.

We need to distinguish quite sharply, as Jones sometimes does not, in deference to gradualists like the demographic and economic historian Anthony Wrigley, the manufacturing-cum-regional-specialization that we call “industrialization”—which happened in other places, such as Japan and China, and leads to a factor of 2—from “economic growth,” which leads to a factor of 100. Jones himself put the point well:

Had the Enlightenment idea of progress not influenced practical affairs, England might have become a normal country, in the terms of the period, content with a quietly prosperous but not forcefully progressive economy—like the United Provinces or

Tokugawa Japan or Venice. Living standards would have been well ahead of Stone Age affluence but stalled on a plateau of bucolic prosperity, the potential for growth meandering away in a Venetian twilight.

Jones 2010, p. 245.

Precisely. The problem, as Mokyr noted, is to explain why the meandering did not occur, as it so often had in earlier “efflorescences,” as Goldstone put it. The Enlightenment conceived as French cannot be the explanation, since the French absent a British irritation would have gone on talking in their salons, and inventing military devices of doubtful practicality, just as the English, absent a Dutch irritation, would have stayed non-naval and non-financial and non-bourgeois.

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Allen provides, too, a test case for what I would call the Max-U Fallacy of Neo-institutionalism. Ever since North spoke out loud and bold, economists have itched to reduce human interactions to maximization within “rules of the game.” The trouble is that Samuelsonian economists cannot hear the word “rules” without thinking “budget constraints on Max U.” When listing “institutions that get the job done,” for example, Allen names every human interaction, from rule of law understood as Keep Off the Grass to families and customs, but then characterizes them, revealingly, as “economic property rights that . . . work together to make people behave a certain way” (p. 219), and later “an institution is essentially a system, or collection, of economic property rights” (p. 226). Allen quotes Avner Greif’s highly elastic definition in 2006 of institutions as “a system of social factors that conjointly generate a regularity of behavior” (p. 248n12).

Allen argues as an interesting parallel that the uncertainties of procreation implied, according to his model of institutions adjusting when nature comes more controllable, that marriage changed only in “the middle of the twentieth century, when technical innovations allowed some control over pregnancy and disease prevention” (p. 220). But the claim is doubtful. It has long been known that family limitation is ancient; and the ideology of the Feminine Mystique had as much impact as the pill (McCloskey 2001). On the same page he offers the changing master-servant relationship 1750 to 1850 as caused by the cheapening of time pieces, which seems even less plausible, and is, if you will, timed incorrectly by some five centuries: Europe had ringing church clocks from the thirteenth century on, and in any case other societies such as the Chinese had public clocks hundreds of years earlier, with no alteration of master and servant.

Allen wants to reduce society to incentives. I am an economist, too, and yield to no one in my admiration for well-aligned incentives. But the modern world works as much through professionalism as through incentives, as much through faithful identity as through profitable prudence. You can show it quantitatively. Let's, then, do the numbers. Allen explains very plausibly the role of prize capture and other strange business in the Royal Navy as designed, or at any rate evolved, "to encourage others to fight in an age in which measurement of performance at sea was so difficult" (p. 107). But aren't we *still* in such an age, exacerbated by the gigantic size of modern organizations? In a merchant's warehouse in 1700 London or *The Victory* at Trafalgar 1805 the head merchant or Lord Nelson could watch virtually everyone, in the commercial emergency or in the desperate engagement. How does a high-ranking executive for Macy's watch its 166,000 employees or the captain of the USS *George H. W. Bush* its 6,000 sailors, and why would one believe that measuring the output is *easier* now than then? Look at Major League Baseball, for example, and the shocking mismeasurement (we have known since *Percentage Baseball*) of the value of stolen bases or sacrifice flies, and therefore the mismeasurement of the marginal product of players skilled at such beloved idiocies. What is the marginal product of an extra ticket agent at Delta Airlines in the middle of a weather delay? Allen argues that "the major problem of the pre-modern world was the enormous role nature played in the ordinary business of life" (p. 21). Yes (and thunderstorms at O'Hare Airport are not about "nature"?). But in the major problem of the modern world is the enormous role that human spontaneous orders and directed organizations play. If it was now easy to monitor professors or doctors there would be no need for professionalism at all, since the customers or the bosses could reward and punish them to maximize wealth. It is the point that Coase made long ago, that a firm and its professionalism and management and solidarity is a substitute for a market.

Allen's central assumption in support of his flow chart is unbelievable, in short, and needs to be established by some quantitative evidence, which he does not offer. He says that it's easier to measure performance now than it was in 1700. I doubt it, and if he thinks so he needs to measure the rise of *efficacious* measurement. I have no doubt that moderns *like* measurement, and *honor* it. But that does not mean they do it correctly, or even use it for many practical purposes. Accounting, note, is about the past. Yet economic decisions are about the future. "Prediction is difficult," said Yogi Berra, "Especially about the future."

And most moderns, such as Allen and I, are employed in massive bureaucracies, in our case massive educational bureaucracies. For all the research assessment exercises and student evaluations of teachers and the other mindless attempts by our masters to reduce education to an assembly line, we cannot measure the contribution

of Allen or me to the output of our universities. It's hard enough on an assembly line. It is much more difficult in the steadily enlarging part of the economy—a quarter of national income right now—that consists of changing people's minds, "sweet talk," as I call it (Klamer and McCloskey 1995). You can make up many numbers, *savoir pour pouvoir*, to know to be enabled, as the early positivists put it, rather the way economists nowadays make up numbers about "happiness." But possessing a number does not mean you have the phenomenon by the tail, except for a scientific question that is naturally quantitative when you have in fact done the relevant measuring.

Management in a free society does not mainly spy on people ("monitor the workers to make sure they do not shirk," p. 19). Mainly it *persuades* them to do their duty, which they do out of a professional sense of self-worth as much as in fear of a stopped paycheck. I could show it to you in statistics of supervisory time spent (Klamer and McCloskey 1995). "The British had a large and successful navy. . . . How was that accomplished?" Allen replies, by "a clever indirect monitoring system that was only slowly copied by opponents" (for unexplained reasons, if it was so very clever; p. 110; he does not note the more usual explanation for the Royal Navy's success: "Rum, buggery, and the lash").

To which I would reply: yes, "incentives do matter," but so does identity and justice and other matters not reducible in a serious empirical study to Prudence Only. Lord Nelson, Allen notes (p. 116), was unusual for his personal courage and for the courage he evoked in others. "England expects that every man will do his duty" was his opening flag signal at Trafalgar, but the cheer that went up from the fleet in response to it, and the eagerness with which the tars and their officers followed his standing signal during the battle, "Engage the enemy more closely," were not achieved by offers of money.

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So if we're going to say that "institutions matter" we are going to have to measure, to show that this or that institutional arrangement had oomph. I would rather say that "language matters," because one can show the language of the economy changing, 1600-1848, in ways highly relevant to the functioning of markets and innovation (McCloskey, forthcoming). Maybe that, too, is wrong. But whatever explanation we give has to face up to the Great Enrichment, and devise economic arguments capable of explaining its scale and its uniqueness. Liberty and dignity for ordinary people seems a good bet. Improvements in a few techniques of measurement does not.

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