

At last, a remedy for chronic QWERTY-skepticism!

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At last, a remedy for chronic QWERTY-skepticism!

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Two complaints keep recurring in the skeptical responses which continue to greet economists and economic historians, myself included, who maintain that the phenomenon of path dependence and its connection with the likelihood of market failure deserve to be taken seriously.¹ One refrain is the dismissal of economic theorems (“blackboard economics”) as lacking relevance to the “real” issues. The other is the demand that a demonstration of major quantitative importance (“oomph”) should accompany any case study that claims to have identified a situation in which market processes failed to yield other than the best of the available alternative allocations of resources.

These two lines of criticism raise some substantively interesting methodological problems that merit closer scrutiny.² They turn out to be interrelated, rather than independent questions, and are connected to the important issues of the role of counterfactuals and the distribution of the burden of empirical proof in applied microeconomics. On more careful consideration, however, it will be seen that neither critique can sustain the dogged skepticism regarding *historical* economics that has expressed itself in attacks upon “path dependence” and “the economics of QWERTY.”

To the lighthouse? : on historical critiques as surrogates for theoretical arguments

Let’s begin with the disdain that Deirdre McCloskey expresses for the resort to theory -- “blackboard economics” -- in the rebuttal I offered to the arguments presented by “The Myth of QWERTY,” the feature story that Clive Crook, deputy editor of *The Economist* (02.04.1999), wrote about Stan Liebowitz and Stephen Margolis’s (then) forthcoming book.³ Surely a little bit of welfare theory about the economics of public goods hardly comes amiss in that context. Liebowitz and Margolis have drawn a parallel between their dismissal of QWERTY as “a myth,” and Ronald Coase’s critique of

¹ Both those motifs of dissatisfaction run through the text of Deirdre McCloskey’s E-mail exchange on the subject with Bradford DeLong (see, e.g., D.N.McCloskey’s ’s reply to DeLong, on EH.NET: 28 June 1999). These lines of criticism should be distinguished from the more sweeping, and more technically mis-informed dismissal of the idea of path dependence, such as that which recently has found its way into *The Economist* (“Economic Focus” for 3 April, 1999, and 18 September 1999).

² This cannot be said of the gratuitously argumentative mis-characterizations of the facts concerning the economic history of typewriter keyboards, video cassette recorder formats and other network technologies, which are repeated by the fringe of economists who have reacted vigorously against the concept of path dependence. The source of most of these distortions is the publications of Stan J. Liebowitz and Stephen E. Margolis, now conveniently collected in a book: *Winners, Losers & Microsoft: Competition and Antitrust in High Technology*, Oakland, CA.: The Independent Institute, 1999, Chs. 2, 3, 6 especially.

³ The full text was posted on <eh.res@eh.net>, the economic history bulletin board of the Cliometric Society, on 14 April 1999, and an abridged version appeared as a Letter in *The Economist*, 24 April 1999.

Samuelson's concept of pure public goods by questioning the historical validity of citing lighthouse services as the example of a pure public good. To critique a theoretical concept, one first has to understand it on its own terms.

Moreover, as a practical matter, to clarify the meaning of key analytical concepts such as "market failure" or "public goods," and to explain their relevance for understanding the evolution of technology in a market economy, surely are among the things that academic economists and economic historians ought to feel obliged to do. Isn't it an especially worthwhile undertaking where one sees such terms being distorted in the public media, largely for rhetorical effect in advancing a particular ideological position? If there are good arguments for free market regimes and capitalism, why let them be driven out by bad arguments?

Quite apart from the evident public purpose that may be served by "blackboard economics," I was quite amazed to find myself chided from this particular quarter for reviewing some points of basic theory in an exposition for a lay audience. Rushing to the blackboard for just such purposes (even at academic economics conferences) used to a famous "D. N. McCloskey specialty"; I wasn't aware that it has now become "incorrect." What a pity that it should be, I thought, because I really rather enjoy the occasional "chalk-up." Call it an occupational addiction, if you will, but it's really more than a personal indulgence. Language, as well as history (and most language *is* history) "matters". That's something that students of academic "rhetoric" -- especially McCloskey -- also spent a lot of time trying to teach the economics profession. So, linguistic precision matters: one ought not casually discard the meaning given to terms such as market failure," or the formal propositions associated with that concept.

McCloskey's main point, however, is that basic theory really can't take us very far because its conclusions generally are qualitative. This leads, in turn, to a call for quantitative demonstration of the "significance" of departures from optimality under "free market" resource allocation regimes. This seems quite reasonable, or at least it could be formulated in a reasonable way. In any case it is a refrain that often is repeated in response to unwelcome arguments: "Yes, but does that really matter?" Consider the following passage, from McCloskey's warm but persistently skeptical response (posted on EH.NET, 28 April 1999) to my essay "QWERTY and the Economics of the Millennium Bug":

I don't imagine that your case can be refuted with a snap of fingers. You were the pioneer of such arguments, which in theoretical work by Romer and so forth have become so important, and we are proud that it was an economic historian (of course). You were a pioneer against opposition, too, from people like me, who kept saying, "Forget about Allyn Young."

But the scientific question is one of oomph, isn't it? I think you agree that "existence" (that math-department word so foreign to an empirical science like physics or history) of economies of scale and the like is not sufficient for any scientific conclusion about how big such effects are. None.

So: On the reaper we sceptics are still waiting for a believable factual showing that the sharing of machines was expensive enough that unimproved acreage was a constraint. It's a matter of oomph. Likewise

on QWERTY we sceptics are still waiting for a showing of why, say, an insurance company with 3000 typists has not yet found it profitable to adopt a "better" keyboard.

Likewise on the 2000 bug (though for all my scepticism you won't find ME on an airplane January 31-Jan. 1!!) we request a showing that such economies-of-scale-and-irreversibilities are big factors in the world economy when set beside the constant-returns effects of adopting modern education, say, or dropping the jailing of intellectuals. You will say, "But look: \$300 billion." To which I say, always, what's the scale for significance? Worldwide that's a small effect. Not trivial: I'd gladly accept a tiny share of it as my personal income; it will be discernable I imagine in the annual figures of economic growth. But how does it compare with the gains from India, say, adopting American ways of doing things from education to electronics, which she will in the next 50 years? THAT difference is 100s of percentages of present Indian GNP per capita. Beside which 1, 2% for this or that dead-end? . . . Not much oomph?

I suppose this is what soon might catch on as "the new economics of oomph." Unfortunately, despite the many occasions on which the term is invoked, "oomph" itself still has yet to be clearly defined, much less subjected to measurement. (Although clearly the concept refers to some cardinal magnitude -- since it is something that we don't seem to have "enough" of -- even the appropriate unit of measurement remains unspecified. It is, perhaps, the "oompha"?) From the context, however, one may surmise that this innovation in quantitative rhetoric is a suggested calibration of the persuasive force of an argument. Who, then, is to say whether the level of oompha's are enough, the most skeptical member of the audience?

Thus, the \$300 billion that I cited as likely to be spent on fixing the millennium bug still is held by McCloskey to be "small" on the global scale of things, which is where we are told the significance of this particular "free market mistake" ought to be evaluated. Well, \$300 billion is about equal to the total value of foreign direct investment in emerging markets during the past decade. So, perhaps by the reflexive argument, the latter also is too small to "matter" much? As an object of policy, as a focus of economic analysis, as a factor that has altered the lives of the people in those economies, should we agree that those foreign direct investment flows are "not significant" enough for us to bother with?

Rather, it would better to pause to consider that there is nothing positive that we mere mortals can affect in the span of a generation, whether individually or collectively, that would be "significant" by the flexible standard of "persuasion" that McCloskey appears to have adopted? (I've put the qualifier "positive" into the preceding sentence in order to take care of the option of blowing up the planet. But then, in the scale of the galaxy, what's a planet --even if it does happened to be ours?) This suggests that the one thing that makes its worth having an *idée fixe* -- such as the belief that path dependence cannot be important in economics -- is the security of knowing that there is no fact or theorem that would ever be big enough (measured in oomphs or oompha's) to dent or dislodge it from your mind. I suppose that is the comfort of religious and other dogmas.

“Oompha-metrics” and economic theory: substitutes or complements?

A more subtle point worth bringing out is the clever asymmetry of McCloskey’s argument, from whence proceeds not only the view of the triviality of all the illustrative instances of the workings of the economics of QWERTY, but also the rhetorical force with which the QWERTY-skeptics attack such research as being “scientifically” inadequate, “urban myths” and “bogus anecdotes.” We first are asked to acknowledge that “theory” is not decisive. Economic measurements of benefits and costs are required, too. Fine. Who could disagree? But the claim, in effect, is that to be “scientific” we must be able to calculate the welfare magnitude of the losses entailed by deviations from the unique and globally stable efficient path, as if the latter was something existing objectively in the material world, something that you can see. So, although this is not explicitly acknowledged, it turns out here that measurement requires a good bit of theory. This truth holds for “oompha-metrics” as much as in econometrics.

How could you fix the reference point for the foregoing calculation, unless the theorems held? In most interesting instances of applied analysis, the Pareto efficiency locus -- and its dynamical counterpart -- remains a counterfactual idealization, at best. To establish such a benchmark empirically requires some kind of simulation, as McCloskey has recognized. More precisely, a *counterfactual* simulation is what is required. Yet, the problems of parameterizing a complete counterfactual model using data from the world of actuality are well known. Consequently, the methodological burden being pressed upon the applied economist by the QWERTY-skeptics is a pretty heavy one indeed.

It doesn’t seem unfair to point out that the economic analysis that is said, on the one hand, to be able only to take us “so far,” is the very same body of theory that the QWERTY-skeptics are using, with the other hand, to get as far as the presumption that “we” (or they at least) have a pretty good idea of what the economy’s optimum path looks like. How do they manage that trick? Largely by smoke and mirrors, alas: asserting that we should know that under the value system inculcated by capitalism, or “bourgeois culture” -- which Marxists used to associate with capitalism, and Hayekians associated with the workings of “free markets” -- we must end up in the near neighborhood of some sort of welfare *optimum optimorum*. There is a key double-elision in this claim, neither part of which is strictly legitimate.

The first elision is the association of the individual behavioral predispositions formed by “bourgeois culture” with the existence of the very stringent conditions of a competitive market economy conditions that would transmute private vice into public virtue -- as envisaged by the first and second theorems of welfare economics. The second slight-of-hand is the quiet shift from establishing conditions under which \$100 bills will not be left lying on the pavement (static welfare efficiency), to the assertion that these will guarantee the emergence of economic dynamism and long-run improvements of the lot of the masses in a society such as India’s.

Notice, further, that it simply will not do to try countering this criticism with the argument: “Well, if we were far below the optimum, there would be an economic surplus to be had by moving to the superior equilibrium, and that would be a sufficient spur to individual (private) remedial action.” The latter is the line of argumentation

which confuses what is privately profitable for economic agents acting in an historically given context, with the course of action for the social ensemble whose interactions will create the historical path of development. It is related to the circular logic of McCloskey's proposed "test" of QWERTY's alleged near optimality -- observe whether or not a firm that today is employing 3000 typists would or would not install Dvorak computer keyboards. But the latter is tantamount to saying that market failure cannot happen, because if it did happen, markets would work to correct it. McCloskey is not alone in this style of reasoning. One can read patently circular arguments of that kind elsewhere, notably in the critiques of QWERTY by Stan Liebowitz and Steve Margolis, and other contributors to the backlash against path dependence. But this doesn't require economists -- or anyone else -- to take such arguments seriously.

"Oompha-metrics" and the burden of proof

But let me press on further, by asking these QWERTY-skeptics by what logic the burden of empirical proof has to be distributed in the way they propose? Those who would contest the Panglossian presumption about the workings of markets under capitalism are being called upon to step up to the task of showing that departures from the theoretical optimum really matter "a lot." Why isn't it up to the skeptics to demonstrate empirically that they only matter "a little"? Where is it written that the burden of showing quantitative importance in this matter belongs only on the shoulders of those who keep finding grounds (in both reason and fact) for disputing the presumption of optimality or near optimality?

This issue is distinct from the problem that in gauging what was or was not an economically "important" or "significant" effect depends upon the kind of dynamic model one has in mind. In some dynamical systems differences at the margin remain "marginal." But in others, and especially in non-linear and complex systems having a multiplicity of attractors, what looks to be a "small" effects at the margin in a static evaluation readily can ramify and cumulate into far-reaching transformations of the system's emergent properties. In other words, the details of the simulation model, like the details of history, will matter to the outcomes. Here, however, I am putting all that aside in order to focus on the simple question of who it is that needs to describe the shape of the implicit counterfactual(s) optima, once we accept that there is no theoretical basis for the presumption that the present situation coincides with a unique *optimum optimorum* or is even a close, second-best optimum.

The position staked out by the QWERTY-skeptics seems to be that the gap between "what is" and "what ought to be" in terms of efficiency ought to be viewed to be negligibly small until it is proven otherwise. Were that argument sound, it would certainly support McCloskey's proposed assignment of the burden of empirical proof. But, it is not sound. Even supposing that "small" was well defined -- a matter of some doubt, as I have already suggested -- what is the basis for that pre-supposition? Have the skeptics managed to carry out the required counterfactual simulation? More to the point, isn't it methodologically incumbent upon those who hold to that side of the "conversation" to be making another trip to the black board?

The position they have staked out calls for them to have first identified the relevant conditions for both the *optimum optimorum* in the specific context, and some relevant "second best" optima besides. Only then could would it be relevant to produce

the evidence that the second best conditions were fulfilled. And, after having done all that, the skeptics would be obliged to show -- without any arbitrary linearizations -- that the actual situation corresponds to one among the "second best" outcomes that in some welfare sense is "close to" the first best. Obviously, this is not what the critics of path dependence and the dedicated QWERTY-skeptics have sought to do.

Little wonder, for it really is a formidable challenge. Pending some signs of such activity, I remain persuaded that the more fruitful course for micro-level historical case studies will not involve the construction of extended counterfactual models required for quantitative exercises of that sort. Rather, there is much to be learned from continuing to work on more modest, and correspondingly more feasible programs of research into how history matters. One way to do so is to seek to identify critical branch points in the stream of technological and institutional evolution. To make the case for path dependence in its canonical form, it is sufficient to show two things about such junctures. The first is that there existed a multiplicity of feasible paths forward, and that these were both distinctive and locally stable in some well-defined sense -- that the agents choosing one or another path would not have been likely (soon) to be led back to a single, globally stable attractor of the kind that characterizes an ergodic dynamical system.

The second requisite concerns the process of "selection" at the branch point(s): the actual path of development must be shown to be an emergent system property whose "selection" was an unintended consequence of the interactions among agents that were not engaged in any conscious collective choice. Alternatively, if it happens that one can establish a deliberate, purposive action to have been the decisive mechanism of selection, then the dominant actor's behavior should be seen to have been guided by considerations (economically rational or otherwise) that did not give significant weight to potential entailed consequences that happen to be of interest from our present vantage point. That is what should be understood by references in the literature on path dependence to the role of "historical accident"; it is not that such selecting events or actions were irrational, or inexplicable, or even unpredictable -- simply that they proceeded from causal factors that were orthogonal to the system level economic issues that we, as *ex post* analysts, can see were at stake.

To go further than that, and thus to enter into the specific domain of "QWERTY-nomics," calls for something more. One must show that the economic conditions of positive feedback which gave rise to the existence of that multiplicity of locally stable paths, were such as would deprive a decentralized, competitive price system of the ability automatically to guide the actors in the most socially efficient direction. In other words, where the source of non-ergodicity in the system happens also to be one of the large class of (convexity-destroying) conditions that undermine attainment of Pareto efficiency in a competitive market regime, market failure becomes a likely outcome.

A path dependent historical account of any richness and complexity will not consist of only one such branch point, but of a succession of critical junctures. This calls for careful examination of the conditions that over time acted to both generate and delimit the array of available choices, as well as the succession of selection mechanisms. The idealization of a path dependent historical process as one in which the economic system passes through only one critical juncture or branch point, from which

it emerges “locked in” to a particular dynamical equilibrium, is just that: an idealization. Its purpose is not the faithfully realistic historical explanation of most of the history of the world in which we live, but, instead, a idealized heuristic, meant to explain the approach to historical explanation.

The fact that the layout of typewriter keyboards, like the gauge of railways in many parts of the world, happened to have stabilized a long time ago, and that the technological systems in which these are embedded are still around, makes these illustrative cases heuristically useful rather than proto-typical. In reality, the structures defining locally stable attractors are subject to a higher frequency of disruptions. Such disturbing impulses can arise endogenously, albeit as low probability events; but others enter the economic sphere (narrowly conceived) from other, inter-penetrating of domains that have their own characteristic dynamics -- generating events in the cultural, political and physical environments.

For me the story of the evolution of the QWERTY keyboard format continues to be an instructive and empirically sound heuristic, exhibiting a constellation of generic features to which many episodes in technological and institutional history conform. Accordingly, I have tried on this occasion to articulate the generic elements of the story that bear upon an economic assessment of this technology’s evolutionary course of development, and to place the burden of empirical proof where it properly should lay. This has yielded a challenging and constructive program of research for QWERTY-skeptics to undertake -- supposing, of course, that they really wanted to give their implicitly theoretical presuppositions about the efficient workings of markets some empirical “oomph.”

Neoclassical skepticism and the QWERTY keyboard: is there a cure for repetitive thought injury?

Do I expect the doubters to quickly sign up to pursue the proposed course of inquiry? Alas, I must say that I do not. The evidence of past performance suggests that the deeper roots of QWERTY-skepticism lie in pre-commitments to *laissez-faire* economic ideology rather than to the advancement of economic knowledge. As for Professors Liebowitz and Margolis, and other truly dedicated QWERTY-skeptics, I expect they simply will go on insisting that nothing but “the best” technology -- and, for that matter, the best organizations, and the best non-governmental institutions -- could survive in market competition. Worse yet, for that (un-) reason, they most probably will go on dismissing the concept of path dependence as devoid of any useful insights into economically important phenomena or the formulation of micro-economic policies for a dynamic world..

Unfortunately, the amount of attention which has been devoted to the pros and cons of this one, particular illustrative case of path dependent technological evolution seems already to be excessive. Certainly, the haggling over QWERTY until now has not contributed notably to the formulation of the program of research into the economics of path dependence and its implications. Indeed, in my view, the obsessive character of QWERTY-skepticism itself threatens to distract attention from the more general class of theoretical questions and empirical phenomena for which QWERTY was intended to be only a readily comprehensible symbol.

To be sure, there is (by design) considerable rhetorical force in this illustration. That must bear some of the responsibility for fact that so many economists continue to be hung up on the question of whether or not QWERTY is the best keyboard available today; and, if it isn't, whether that entails a "big" economic inefficiency, or one that should be dismissed as inconsequentially small. For scholars seriously interested in the historical development of typewriting technology this could be a reasonable obsession. But, to suppose that it is substantively crucial to any of the interesting issues surrounding path dependence and its economic policy implications is just plain silly.

As this was not a direction in which I felt it would be particularly useful to encourage others to invest their time, it seemed best to decline invitations to become engaged in debates with the die-hard skeptics whose attacks on the concept of path dependence were formulated as disputations of the historical evidence regarding the story of QWERTY. My reading of those arguments was, and still is that their core reflects nothing so much as complete a failure to grasp the analytical points that the story of QWERTY was supposed to illustrate. Indeed, focusing so much attention on the efficiency aspect of this particular case, as if the relevance for economics of the whole subject of multiple equilibria in stochastic processes (and the mechanisms whereby "selection" occurs among them) somehow turned upon the answer to it, strikes me as wrong-headed on at least four separate counts. Perhaps, even at the risk of further stoking the fires of unproductive controversy, it will be best now to say why, as plainly as I can.

The first count on which QWERTY-skepticism has erred follows from the fact that you can have multiple equilibria that aren't uniquely Pareto-ranked, as well as path dependence in dynamical systems that attain one or another among the multiplicity of Pareto-efficient equilibria. That means that the question of what is and is not "inefficient" is quite separable from the question of the usefulness of the conceptual framework of path dependence, considered as an approach alternative to the neoclassical paradigm of *ahistorical* economics. For economists to accept the contention that path dependence only matters if it is accompanied by market failures would be to cast aside as uninteresting, and hence not worthy of explanatory attention, myriad features of the organization of economic activity, along with numerous issues of long-term economic growth, distribution, and inter-generational equity that lie beyond the narrow purview of well-defined issues concerning allocative efficiency.

Secondly, as I have argued in the foregoing, it is difficult to see any justification for accepting the burden of proving empirically that the outcome of a competitive market process has been other than efficient, given that you have established empirically that the case under examination is one in which the source of the positive feedback in the system is the presence of positive (network) externalities, or non-convexities such as learning effects and habituation. The theoretical presumption that the market would select the most efficient option among the available alternatives no longer exists under those conditions. This isn't news; it's old hat in welfare economics. So, then, the burden of proof plainly falls on those who say that everything has turned out for the best; that QWERTY is better—in terms of social efficiency criteria -- than anything that was and is available. They ought to substantiate that claim, and maybe explain whether that was just a stroke of good luck

or whether something far deeper, something economic theory hasn't recognized about the workings of markets, was going on.

Thirdly, QWERTY's story is hardly the only path dependent tale in which it has been suggested that some outcome -- other than the one that people in the past lived, or with which we are still living -- wasn't "best in the best of all possible worlds." Why obsess on this single, manifestly minor illustration? Why not look at the stories of light water nuclear reactors (a "sub"-optimal technology if there ever was one!); or pesticide- and herbicide-intensive agriculture; or at the whole bevy of information technologies that managed to become industry standards by displacing alternatives whose adoption certainly would not have been worse, and arguably would have been more advantageous for society?

Fourthly, empirical demonstrations in such cases, either way, aren't really so simple as has been suggested by those who focus on assessments of QWERTY today. Indeed, such assessments never will be easy to carry through properly when technologies and institutions have evolved along path dependent trajectories. The notion of identifying the question of efficiency with the evaluation of just the currently observed state can't do much sense in such circumstances. You also have to consider, in the case of the QWERTY keyboard, to take one good illustration, the question of the comparative ergonomic properties of the alternative keyboard layouts that were implemented on manual typewriters, and on machines of different vintages. Mitigation of the costs of market failures does not erase them if amelioration itself carries costs, and thus it will only be under special circumstances that one can hope to assess the resource costs of such "mistakes" by looking only at the most recent, or terminal part of the path.

Thus, for those who succumb to the sirens of cliometrics, and undertake quantitative research in the economics of QWERTY, it may be necessary to gauge inefficiencies in terms of the path-integral of the costs of what I've called "path-constrained melioration." That's a fancy term for the process through which modifications are made in a technology, or an institution, in order to mitigate the costs of its dysfunctional properties. One can see the resource cost dimensions of this process very plainly in the case of the millennium bug, because the nature of the coordination failure there was that the decentralized market incentives led firms to delay attending to the problem for as long as possible, so that the costs piled up at the end in a very visible fashion.

If you accept the dysfunctional characteristics of a technological system, or of an institution, as part of the status quo, then you look at the costs of remediation as an investment proposition. Either it is or is not worth making: more frequently than not it looks better to throw more money at the problem than it would be to start again from scratch, which is why tens of billions of dollars have been spent in "fixing" legacy system computer software. But why set up an accounting system that at each point accepts the status quo as having been unavoidable? Shouldn't one instead gauge the costs of the problems we have been handed to fix as a consequence of the poor selections made in the past?

When the economics profession eventually comes to take path dependence seriously and pursue research of that sort, so that more can be learned about the dynamics of path-constrained melioration costs, we will be better equipped to figure

out how to avoid, or mitigate future costly burdens that doubtless are being created now, and with which future generations will have to cope. It does not take great imagination to see that this path for research would lead the discipline towards a more serious engagement with the economics of environmental sustainability. The question is how much longer the world can afford to wait while economists get their collective intellectual act together: "If not now, when?"

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