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The Current State of the Debate Involving the Economics of QWERTY

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Introduction

Readers of this book will be aware that the economics of QWERTY is a controversial area. This chapter reports on some of details of the controversy as it has unfolded to date. In particular the nature of the incipient debate between Liebowitz and Margolis and Paul David may be of some interest.¹ This debate is complicated by the fact that neither side seems to fully realize or acknowledge the presumptions made by the other side and, as a result, they often end up talking past each other. This is a motivating theme of my account. The reader should be warned, however, that this is a partisan account; in seeking to interpret the debate I have inserted my own judgments on the issues.

Somewhat paradoxically both Liebowitz and Margolis and their critics (in varying degrees) are critical of mainstream neoclassical (textbook) economics and its standards of welfare. That is to say, they are both highly critical of the kind of neoclassical economics that assumes perfect knowledge, perfect foresight, many traders, etc., the kind that derives perfect competition as a Pareto optimal efficient standard against which to judge real world outcomes. Both focus (to a greater or lesser extent) on the importance of ignorance and uncertainty (and the importance of institutions) in rendering such a standard problematic. Where they differ decisively, however, is in the policy lessons that they take away from this.

The critics argue that the ideal of perfect competition is an ideal that, for one reason or another, the free market is incapable of attaining, and that, therefore, one should look to the government to obtain by collective action or regulation, what the market, with decentralized actors, cannot. Liebowitz and Margolis have explained

clearly why the endorsement of government intervention does not follow from a valid critique of neoclassical welfare economics (and, for that matter, why a defense of neoclassical welfare economics, in itself, is insufficient to establish an argument against intervention).² But their insights have not always been adequately appreciated.

David's Story and the Lack of Response

Cicero demands of historians, first that we tell true stories. I intend fully to perform my duty on this occasion (David 1985, 332)

With this sentence Paul David begins his story. This, as we have seen, is somewhat ironic, because, if Liebowitz and Margolis are to be believed, the account that he gives is not true. He has recently objected (David 1997a,b, 1999a,b) that the article was not intended as an accurate historical record, but merely as an illustrative hypothetical interpretation of history, a prelude to the exhortation to economists to do more economic history. If this is the case, then the quoted sentence seems out of place.³

Be that as it may, from the arguments made by Liebowitz and Margolis in their 1990 article, chapter 2 above, we can conclude that some significant questions relating to the history of the evolution of the typewriter keyboard have been raised that, *at the very least*, deserve to be considered. Surprisingly, no such consideration has been forthcoming.⁴ The absence of even an attempt to deal with these questions is troubling. On the matter of the history on which the economics of QWERTY is based, the case for QWERTY-nomics is under a cloud of doubt and suspicion.

It might have been expected that Paul David would have responded in one of two ways:

1. either he could have, in all candor, conceded that Liebowitz and Margolis have a case on the historical record and that he did indeed present an incomplete picture, making the QWERTY story a poor candidate for the illustration of path dependence, lock-in and the like; or,
2. if possible, he could have jumped at the opportunity to present a more complete history while differing with the interpretation that Liebowitz and Margolis draw, providing his own, one that is more supportive of the QWERTY story

as a candidate for the illustration of path dependence, lock-in and the like.

In a decade's time he has done neither. As we shall see, he has instead attempted to avoid the issue. In the process, in some recent unpublished contributions, he has insinuated (among other things) that Liebowitz and Margolis are wrong on the facts and that he will "soon" explain (See David 1997a,b and 1999a). In a direct reference to Liebowitz and Margolis he writes:

[I will] put to one side the specific factual allegations adduced in their article... and look instead at the logic of their analysis.... There will be time enough in the near future to put right the historical mis-allegations ... which being allowed to stand too long without the refutation it deserves, has encouraged some uncritical skeptics ... to dismiss the emblematic tale as 'the *founding myth* of the path dependence. (David 1997b, 4).

Similarly:

... there will be another, more suitable place in which to consider my detailed rejoinder to the dubious factual allegations that have circulated concerning the 'true story' of QWERTY (David 1997a, 7).

To the best of my knowledge, the response to "The Fable of the Keys" has yet to be made.⁵

On Some Theoretical Matters

"Correct" definitions of path dependence

While not responding on the history of the case, Paul David *has* chosen, in some of his recent, as yet unpublished, work to make an issue out of the definition of path dependence, suggesting that the technical definitions taken from the natural or statistical sciences have more validity than less technical ones and that Liebowitz and Margolis's taxonomy of path dependence is *unscientific*. I shall consider this below, but it is important to point out that David's own original definition of path dependence is not without potential problems. While logically sound it has no obvious empirical

relevance, something that Liebowitz and Margolis's taxonomy was to designed to remedy.

I begin with the definition David provides in the 1985 work:

A *path dependent* sequence of economic changes is one of which important influences upon the eventual outcome can be exerted by temporally remote events, including happenings dominated by chance elements rather than systematic forces.

The next sentence reads:

Stochastic processes like that do not converge automatically to a fixed-point distribution of outcomes, and are called *non-ergodic* (David 1985, 332).

Note that between the first and the second sentence a number of hidden presumptions creep in. It seems to be presumed that one can meaningfully talk about real world economic outcomes as a set of stochastic processes and that, in fact, historical processes are equilibrating processes. Otherwise what is the connection between the two sentences? A further assumption would appear to be that equilibrium in the real world is analogous to equilibrium in physical systems ("fixed point distributions") and that such equilibrium points are relevant to an assessment of the process, whether one gets there or not.

He continues:

In such circumstance "historical accidents" can neither be ignored, nor neatly quarantined for the purpose of economic analysis; the dynamic process itself takes on an *essentially historical* character (Ibid., 332).

Surely everyone would agree that *one cannot ignore history and do good economic analysis*. And it is no doubt true that neoclassical economics is hopelessly short on history. If this is all that David had wished to establish then there would be no interesting discussion. The real issue, in this particular aspect of the debate however, seems more to concern economic policy (rather than the neglect of history) and I will return to this in the next section.

In some of Paul David's recent unpublished work (1997a,b 1999a) on this subject focuses almost exclusively on Liebowitz and Margolis's theoretical contributions, or, even more narrowly, on their definitional framework. In effect he indirectly challenges Liebowitz

and Margolis's suspicions of the policy relevance of path dependence by criticizing at length their understanding of the concept. He does so brandishing the big stick of "Science."

The time has come for me to take explicit public notice of the numerous respects in which this critical representation of the concept of path dependence and its significance, and even its implications in the sphere of economic policy, is *a scientifically inappropriate distortion* (1997b, 3, italics added).

I aim first to expose the many things I believe to be wrong or misleading about Professors Liebowitz and Margolis's treatment of the analytical aspects of path dependence over the course of the past seven years (Ibid., 4).

He then embarks on a lengthy discussion of the definition and meaning of path dependence, which I now briefly examine. We may recall the just quoted 1985 definition:

A *path dependent* sequence of economic changes is one of which important influences upon the eventual outcome can be exerted by temporally remote events, including happenings dominated by chance elements rather than systematic forces (David 1985, 332).

This is a very general common-sense definition that is clearly consistent with a variety of variations of the idea that "history matters." It is true that he goes on to attempt to make this sound more "technical" by using language borrowed from mathematical statistics, but nothing in the article appears to depend on this more "rigorous" statement.

In his recent critique, by contrast, he makes it sound as if getting the "right" definition of path dependence and lock-in is crucial. It is not possible here to reproduce the entire argument, an attempt will be made to give the flavor.

Path dependence, as I wish to use the term, refers to a dynamic property of allocative processes. It may be defined either with regard to the relationship between process dynamics and the outcome(s) to which it converges, or the limiting probability distribution of the stochastic process under consideration....

Path independent processes may be said to include those whose dynamics guarantee convergence to a unique, globally stable equilibrium configuration or ... those for which there exists an invariant (stationary) asymptotic probability distribution that is continuous over all the states that are compatible with the energy of the system....

Negative definition: Processes that are non-ergodic and thus unable to shake free of their history, are said to yield path dependent outcomes.... (David 1997b, 5).⁶

He continues in similar vein to provide other variations. To what purpose? Two points emerge.

1. Path dependence may not imply inefficiency, and Liebowitz and Margolis are wrong to suggest that it is only if path dependence implies inefficiency that it is interesting (David 1997b, 9).

I would merely state here that Liebowitz and Margolis do not suggest that path dependence is only interesting if it implies inefficiency, but they do suggest that the more interesting policy implications arise when it does.

2. David seems to want to underline that, as he sees it, "path dependence is a property of *stochastic* sequential processes." He also emphasizes that it refers to "dynamic" processes.

The point here seems to be that Liebowitz and Margolis apply the idea of path-dependence incorrectly to deterministic processes (Ibid., 7). This again would appear to be an unwarranted conclusion. In fact, I would argue that David's notion of "dynamics" in an equilibrium probabilistic situation is inappropriately "static" by comparison to Liebowitz and Margolis's implicit vision.

Other than these points, it is hard to find a relevance for David's long, argumentative discourse on correct definitions (1997a,b). One need hardly add here that a particular concept which has one connotation in the natural sciences often develops important and subtly different connotations when applied to the social sciences. One need only point to concepts like "equilibrium" and "efficiency."

It is hard to see how Liebowitz and Margolis's attempt to expand path dependence in such a way as to make it more relevant to economics (by including the perceptions of economic agents in its construction) can be said to be "incorrect" or "unscientific."

Revisiting path dependence of the third degree

The reader will recall the taxonomy that Liebowitz and Margolis provide regarding the question of path dependence. First degree path dependence refers to the most basic observation that "history matters" – events have enduring consequences. Second degree path dependence refers to the fact that some of these consequences may occasion regret. Some events, the result of ill-informed choices, may have sent the economy along a path that is, from the vantage point of present knowledge, less than optimal, so that, had we to do it over again, we might have made wiser choices.⁷ Third degree path dependence narrows the focus still further to single out those cases of the second degree that provide an opportunity for remediation. Being on a sub-optimal path, in the sense that some better world could be imagined, has no policy implications in the absence of an economical way to move toward the optimum. David has attacked this taxonomy by characterizing it as a rhetorical trick designed to empty the concept of path dependence of any policy relevance.

Now it should be conceded that the existence of a third-degree-path-dependent inefficiency is something of a paradox. Why does entrepreneurial activity not remove it? Perhaps because it does not really exist? What appears to be an inefficiency is merely an erroneous judgment made by a third party who does not understand all of the costs that would be involved in choosing an apparently superior alternative. This line of thinking appears to lead us into the Panglossian conclusion that "whatever is, is necessarily efficient," an impasse that has been noted by many theorists, for example by E. J. Mishan (1975, 699) and again by David (1997a,b, 1999a) and Puffert (1999). The Panglossian impasse can be used to characterize the above type of taxonomy as a transparent attempt to foreclose any real policy discussion (as is done, for example, by David (1997b, 13-15). I will suggest that this is an unwarranted construction, one that rests on a particular presumption of what it is necessary to do to establish a case for remedial policy intervention.⁸

The identification of robust equilibria (the result of ergodic processes) that are not sensitive to variations in the values of key

variables, is fundamentally more threatening to the concept of "freedom of choice" than is the recognition that "history," including our personal choices, may make a difference. It is, however, one thing to claim to be able to understand in some way how (retrospectively) history has mattered and quite another to claim to be able to understand how it will do so in the future and to base our economic policy on this. There is, I argue, a fundamental asymmetry between understanding and prediction (much methodological discourse to the contrary notwithstanding).

Policy

What the "debate" really seems to be about is economic policy. The protagonists are on opposite sides of a fundamental policy divide and, are, in effect, talking past each other. Along these lines, I request the reader's indulgence to engage in some speculative mind-reading.

Paul David probably suspects that Liebowitz and Margolis's (and their sympathizers') work is a thinly disguised attempt to foreclose any type of anti-trust policy activism and even other less-activist policies, like any form of government sponsored activity to influence the adoption of appropriate standards. He probably suspects that they are, from the start, constitutionally predisposed against all kinds of government intervention. And he is probably largely correct.

For their part, Liebowitz and Margolis probably suspect that Paul David's work is a manifesto in support of the presumption that government *ought* to be involved in these matters and in favor of the proposition that the government can, if the circumstances are appropriate, be a force for good in the economy. They probably feel this way about most of the contributions in this area, some of which were cited above. And in this they would probably be right.

There is a difference of vision, a difference of presumptions. In this section I will illustrate how this plays out in the rhetoric of the debate. Curiously, one way to characterize my conclusion is to say that the policy recommendation that one arrives at is path dependent. Where you end up depends crucially on where you start.

Placing the Burden of Proof

This can be illustrated in very familiar terms (see Table 11.1 below). Consider the discussion about policy relevance to be analogous (it is very closely analogous) to the conducting of an experiment with (known or unknown) probabilities. As everyone knows, the outcome of the experiment will depend crucially on which errors one seeks to avoid, that is, on which errors one considers to be Type I or Type II. To be more specific, imagine that we are “testing” for the existence or absence of an inefficiency in an established network or standard (or the adoption of a product associated with it). Then two types of experimental design are possible depending on the choice of the null hypothesis, H_0 , as illustrated in the table below:

Table 11.1: Experimental design in searching for policy relevance

<i>Experimental design</i>	<i>$H_0 =$ the null hypothesis</i>	<i>$H_1 =$ the alternative hypothesis</i>
<i>Design A</i>	an inefficiency exists ⇒ (the status-quo is not efficient)	an inefficiency does not exist
<i>Design B</i>	an inefficiency does not exist ⇒ (the status quo is efficient)	an inefficiency exists

Assume that in order to establish policy relevance it is necessary to disprove the null hypothesis. The alternative designs reflect the presumptions of the experimenter. The essential difference between the two designs is *where it places the burden of proof*. Design B places it on those who lean in favor of policy interventions, while design A places it on those who presumptively oppose it. In this way Liebowitz and Margolis and David (and others who point to the theoretical “likelihood” of inefficiencies) are each trying to place the burden of proof on the other. This is why David can object to Liebowitz and Margolis’s taxonomy of path dependence by suggesting that it is a rhetorical trick designed to paralyze economic policy (in reference to the Panglossian impasse discussed above) (David 1997b, 11)⁹. From Liebowitz and Margolis’s perspective it

simply reflects where they consider the appropriate burden of proof to be placed. It is a principled position as “scientific” as any other alternative design.

It is always difficult to reject the null hypothesis, (it is sometimes not possible under any practical circumstances). The experiment is designed to make it difficult. Design B is designed to minimize government intervention. Design A is designed to facilitate it. The two designs reflect differences of opinion about the likely benefits of government intervention and, thus, differences in fundamental values. In truth, of course, all empirical (historical) observation is informed by some implicit or explicit theory. The facts never “speak for themselves.” One comes to every situation in life with prior presumptions. Differences in presumptions constitute the crux of the different approaches in this field.

In this way *no “scientific research” is completely value-free*. How is one then to choose between rival designs? Only by an appeal to common values.

In this context, Liebowitz and Margolis are, in effect, saying, “if you think you have identified a remediable inefficiency, prove it.” What justification do they have for doing so? The same justification that would presume an accused person innocent unless “proven” guilty (using a stringent probability level of significance to minimize Type I errors), namely that all governmental action is essentially coercive, and if we are to err we should do so on the side of minimizing coercion. They are seeking to avoid the costs of incorrectly identifying an inefficiency, while accepting the costs of failing to identify one. Thus David is surely wrong when he attributes to them the proposition, that, absent the identification of an inefficiency, one may presume to have proven that the outcome is efficient (David 1997b, 13, See also David 1992, 137). Clearly, there is a difference between proving the existence of an inefficiency and proving its absence.

Liebowitz and Margolis are perfectly clear on this. “There is neither convincing theory or even minimal empirical support for the lock-in proposition” (xx). “Although our theoretical discussion does not prove that markets must always choose the best technology, we do claim that there are good reasons to expect it to be very unusual for market participants knowingly to choose the wrong technology” (xx). And so they require a heavy burden to be met. “[P]roofs of existence of inefficiency can never rely on the mechanics of

production and consumption alone.... market failure ought to be a very specific and very worldly claim. Policy-makers shouldn't go about correcting markets until they have concrete proof that markets have failed" (xx).

David places the burden in a different place. In fact he explicitly addresses this in his most recent paper on this subject (David 1999c). He does so in the context of responding to Deirdre McCloskey's persistent challenge to indicate just "how much" it mattered that QWERTY (or anything else) was an inefficient outcome. In this McCloskey was pursuing a theme she has recently developed - drawing attention to the distinction between statistical and economic significance. It is the latter that is relevant for economic history and policy, the *magnitude* of the alleged effect, the "oomph." (Presumably, the two are not unrelated, since the larger the deviation from the value implied by the null hypothesis (where the null value is zero), the more statistically significant it will be - though statistical significance is not sufficient - or even necessary - to deliver oomph). David bristles at the challenge to demonstrate that an "economically significant" inefficiency exists. In the first instance, he points out that the very notion of "how much" implies the adoption of Pareto efficiency criteria; the QWERTY-skeptics must have some notion of "what the economy's optimum path looks like" if they are suggesting that one can measure deviations from it (Ibid., 4). And it is this line of reasoning that allows him to suggest that the problem with his critics is that they are inappropriately wedded to static welfare economics (Ibid., 5). But, secondly, "the burden of proof plainly falls on those who say that everything has turned out for the best" (Ibid., 8). "Why isn't it up to the skeptics to demonstrate empirically that [departures from some theoretical optimum] 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?" (Ibid., 5).

This is clearly directly relevant to Liebowitz and Margolis's work, whose approach is similar to that of McCloskey. The answer to David's question is surely, as explained above, that optimality is *not* assumed (at least not by Liebowitz and Margolis). It is not addressed. What is addressed is the likelihood of government policy being able to improve matters in a world of rapid change and innovation.

In clarifying the role of the (mostly implicit) burden of proof presumptions it becomes clear that apparently value free discussions almost always harbor hidden prejudices about the desirability or otherwise of state intervention. Bringing this to light forces a discussion of the appropriate location for the burden of proof. Should those who propose state intervention shoulder the burden to show that it would, on balance, be beneficial; or should those opposing it shoulder the burden of showing that it would, on balance, be harmful. Stated in this stark manner, and remembering that all state intervention implies the abridgement of individual autonomy in some way, most economists would have to agree that the former burden is the appropriate one. Juxtaposing this with the criticisms of Liebowitz and Margolis's taxonomic and policy discussions, lends the latter increased credibility.

Efficiency, Policy and Knowledge

Liebowitz and Margolis and David appear to agree on what it means for a technology to be inefficient - they all agree that the criteria must involve an appeal to individual consumer valuations. For example,

By [an inefficiency] we must mean that an alternative outcome would be preferred in some collective sense (perhaps by application of a compensation test) to the one [individuals] are now in, and that they also (collectively) be ready to incur some substantial costs to rectify the situation - assuming it was feasible to do so (David 1997b, 13).

How then are such situations identified and corrected? David is convinced that there are historical situations in the world in which individuals were "bounded by a parochial and myopic conception of the process in which they were engaging [and in which they] failed entirely to foresee the complementary innovations and investments that would be influenced by their initial commitment to one rather than another course of action" (David 1997b,15). This is clearly Liebowitz and Margolis's second degree path dependence, from which I said earlier no obvious policy implications emerge. What then would David propose? According to him, in a most revealing passage:

One thing that public policy could do is to try to delay the market from committing the future inextricably, before *enough* information has been obtained about the likely technical or organizational and legal implications of an early, precedent-setting decision ... [P]reserving open options for a longer period than impatient market agents would wish is a generic wisdom that history has to offer to public policy-makers, in all its application areas where positive feedback processes [like network-effects] are likely to be preponderant over negative feedbacks. Numerous dynamic strategies can and have been suggested as ways of implementing this approach in various specific contexts where public sector action is *readily feasible*. Still more sensible and practical approaches will be found if economists *cease their exclusive obsession with traditional questions of static welfare analysis* and instead of pronouncing on the issue of where state intervention would be justified in the economy, start to ask what kind of public policy actions would be most appropriate to take at different points in the evolution of a given market process (David 1997b, 16, italics added).

This is a remarkable passage worth analyzing in some detail. Liebowitz and Margolis emphasize the role of information (knowledge) in policy action, and establish a case sufficient to cause those who contemplate this type of policy, reason for apprehension. But, in addition, I would note, that if policy-makers have knowledge of superior alternatives they surely cannot be alone in this, and if they are why not just make the knowledge public? What David seems to be suggesting here is that *policy-makers have information about what future information (or type of information) will yet be revealed, and also that they can have knowledge of when enough information has been revealed to allow competition between standards to proceed unregulated*. Somehow the policy-makers know more (about what can and will be known) than economic agents do. David may object that even if the agents had the same knowledge about future knowledge as the policy-makers do, they are not organized to, or interested in, providing a collectively rational solution. But if such a solution is “efficient,” by common agreement (of what “efficient” means) it would be profitable to organize. Surely this is more than a rhetorical game. We are back to the issue of burden of proof, this time in a very literal and compelling way. One must also ask why we should be content to assume that policy-

makers have the right incentives in this regard (even if by some stretch we solved the “knowledge problem”)?

The “knowledge problem” is, however, the crux and it is implicit in Liebowitz and Margolis’s arguments. It is ironic, therefore, to find David in this passage and in numerous other places criticizing Liebowitz and Margolis for their preoccupation with static welfare criteria. Further, he seems to be suggesting that moving beyond such a framework would support the type of policy activism he is proposing here.¹⁰ The static welfare framework is indeed problematic and we may readily join David’s call for moving the education of economists beyond it. Static welfare criteria are inapplicable to dynamic processes and, it is for this reason that, David’s policy prescriptions are manifestly unworkable

Concluding Remarks

Both sides of this debate proceed by waiving the big stick of “Science,” but in different ways. David seems to think that credibility and respectability comes from displaying an understanding of technical theoretical frameworks borrowed from the “hard” sciences. In common with much of the writers in this area, he focuses on theoretical sophistication and consistency. Liebowitz and Margolis criticize this by, correctly, pointing out that model building is not a substitute for “empirical” (historical) investigation, to find out which model, if any, is applicable. For better or worse, however, there are no investigations that could provide “knock down” results. The way is always open for alternative counterfactual interpretations and speculations regarding future developments. The role of plausible counterfactuals is clearly relevant. Liebowitz and Margolis’s work echoes (perhaps not altogether consciously) some of the important epistemologically-based criticisms of standard microeconomics, and they do so in an area of applied economics that could hardly be more relevant.

Notes

1. I have made liberal use of material from Lewin 2000. In the Preface above I have acknowledged some people who helped in the preparation of this work.

2. In this they are, in many respects, close to the economists of the Austrian School. The Austrians argue not only that the perfectly competitive standard is unattainable, but that it is furthermore, “precisely irrelevant,” (Boettke 1996) and the efficiency standards associated with it are misleading. Austrians are thus critical of Keynesians *and* neoclassicals for different reasons. They are critical of neoclassicals for succumbing to a naïve “positivist”-inspired “physics-envy” in their theory construction, that has led them to create an easy target for the would-be planners of our world, including the Keynesians (Machovec 1995). They are critical of the Keynesians for succumbing to the “fatal conceit” of thinking that they can achieve, through policy intervention, what the free market cannot, and, in the process, threatening the very valuable achievements of the *real world* market process. In policy matters, Austrians do not see themselves as utopians, they see themselves as realists. On the other hand, they see both Keynesians and neoclassicals as utopians – albeit of different stripes. The protagonists in the debate discussed in the text unconsciously mirror the same two sides of this policy divide. Both are critical of neoclassical economics, one in muted and somewhat superficial terms, the other more fundamentally. And yet they are clearly opposed to each other.
3. In support of this reading we may note the following

Standing alone, my story will be simply illustrative and does not establish how much of the world works this way. That is an open empirical issue and I would be presumptuous to claim to have settled it, or to instruct you in what to do about it. Let us just hope the tale proves mildly diverting for those waiting to be told if and why the study of economic history is a necessity in the making of economists (David 1985: 332).

But then he proceeds to a series of quite provocative unsupported assertions that have, indeed, formed the basis of presumptuous historical judgments and related policy prescriptions, some offered by David himself (of which more below). Also, as we shall note, apart from the incongruence of

- this reading with the first sentence quoted above, David’s failure to concede Liebowitz and Margolis’s case, or even to respond on the issue of the veracity of the story he tells, leaves the distinct impression that he is more wedded to his particular interpretation than the above paragraph would suggest.
4. A notable recent exception is the online discussion on the EH.NET and also Puffert 1999.
 5. In his most recent contribution (1999b:7) David seems to have deflected the issue entirely. He writes: “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 path dependence were formulated as disputations of the historical evidence regarding the story of QWERTY.”
 6. Similarly for lock-in, “lock-in ... is simply a vivid way to describe the entry of a system into a trapping region When a dynamical (sic) economic system enters such a region, it cannot escape except through the intervention of some external force or shock ... [and] may thus become locked-in to attractors that are optimal, or just as good as any others [or not]” (David 1997b: 11).
 7. David opines: “notice that while incomplete information may be critical in blocking spontaneous escapes from dominated coordinated equilibria [read outcomes] it is not a necessary condition for decentralized market processes to select such states.” One wonders then what is? What other explanation is there for why voluntary economic agents would choose inferior situations other than ignorance (of the advantages or of the coordination costs)?
 8. This is relevant to the typewriter case discussed earlier. Paul David clearly implies that QWERTY is a case of third degree path dependence. Deirdre McCloskey has commented: “I am astonished that Paul [David] does not reply to the empirical, historical question: if QWERTY ... is such a costly constraint on typing industries, why have none of them, not a single typing division of any company, large or small, capable of internalizing the allegedly important externality in retraining its typists as you

could retrain someone to play a clarinet who knew the saxophone, ever changed?” (McCloskey 1999).

9. For example he refers to, “quite transparent resorts to the stratagem favored by Humpty-Dumpty, ‘It’s not what the words mean, but who shall be master!’” (Ibid., 11) and to “Strategic redefinitions, playing with words ... a form of rhetoric that is essentially obscurantist the purely semantic trick ... the taxonomic gambit deployment of taxonomic non-sequiturs ... rhetorical games....” (Ibid., 13), which leaves one wondering whether all this name-calling is itself some sort of “rhetorical game.”
10. At first I wondered how such a non sequitur could ever arise, but then I read David 1999b and realized that it was associated with the dialogue with McCloskey as explained above. Another remarkable thing about this passage is that he seems to be asserting that we ought to forget about discussing the justification for policy action in principle and simply talk about what *kinds* of policy action would be most appropriate.

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