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The Market Process and the Economics of QWERTY: Two Views

PETER LEWIN*,† plewin@utdallas.edu
School of Management, University of Texas at Dallas, P.O. Box 830688, Richardson, TX 75083-0688

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Abstract. Since its publication in 1985 Paul David's "Economics of QWERTY" has provided a paradigm case for the understanding and application of path-dependent processes in economics, some of which have been identified as yielding sub-optimal outcomes. The accuracy and relevance of this case, and this entire theoretical approach, has been subjected to critical scrutiny by Stan Liebowitz and Stephen Margolis in a series of articles and in a recent book. In this article I provide a wide ranging, and largely appreciative, review of the book and highlight, in some detail, the fundamental disagreements with which it deals.

Key Words: path-dependence, network effects, lock-in, QWERTY, burden if proof

JEL classification: B41, L40, N00.

1. Introduction and Overview

1.1. What is the Real Discussion About?

Allow me to simplify a bit.

What do Austrian economics (in some of its versions) and Keynesian economics (in some of its versions) have in common? Answer: They are both highly critical of neoclassical economics—the kind that assumes perfect knowledge, perfect foresight, many traders, etc., the kind that derives perfect competition as a Pareto optimal efficient standard against which

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†www.utdallas.edu/∼plewin

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 do these schools of thought differ? Answer: Mostly in the policy lessons that they take away from this.

The Keynesians 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, what the market, with decentralized actors, cannot. The Austrians, on the other hand, argue not only that the perfectly competitive standard is unattainable, 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 an interesting recent debate 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 also appear implacably at odds with each other. The debate to which I am referring is the one between Paul David and his critics, Stan Liebowitz and Stephen Margolis. David makes Keynesian-type arguments, while Liebowitz and Margolis feature as unwitting Austrians.[1] The debate is complicated by the fact that neither side fully realizes or acknowledges the characterizations drawn here and, as a result, they often end up talking past each other. This is the motivating theme of my account of this debate.

1.2. Some Quick Background

In 1985 David published an article (David, 1985) that subsequently has become very influential, not only among economists (theorists and economic historians) but also in the popular press. The article picks up from the fact that recently the imagination of economists has been spurred to discover evidence of new types of "market failure" associated with the special nature of information as a product This is, or course, related to the fact that many of the technological advances that have occurred are related to the generation and use of information in one form or another. This general literature is associated with parallel developments in the fields of mathematics and statistics having to do with topics like "chaos" and "complexity." In economics, the literature is known generally by association with the technical concepts of "network-effects," "path dependence" and "lock-in." These concepts suggest that the outcomes we observe (for example, in the generation of products to record and play video images, process data, or simply type journal articles)

may not be the best possible outcomes we could achieve. More seriously, random events may "lock us in" to a path and, therefore, an outcome that is socially inferior to an alternative one that is, or was, available. This is known generally as "the economics of QWERTY." It derives from the assertion, by Paul David (1985, 1986), Brian Arthur (1989) and others, that the inherited typewriter keyboard, with its layout of keys according to the `Q W E R T Y` configuration, represents an unfortunate accident of history and is an inferior outcome to more rational designs that might have been adopted, including one by August Dvorak. This particular historical example has served as a widely quoted and accepted paradigm case for what is seen as a general phenomenon, namely, the lock-in to inferior standards.

In an important new book Stan Liebowitz and Stephen Margolis (Liebowitz and Margolis 1999) have critically examined the various aspects of the economics of QWERTY and its implications. With eloquence and relevance they call into question the historical accuracy of the standard account of the QWERTY case and of similar "myths" of lock-in. They contend that no plausible case of sub-optimal lock-in has ever been satisfactorily documented. While the conventional wisdom remains that such inefficiency is widespread and much recent antitrust activity (including the recent Microsoft case) and legislative policy discussion is based on that assumption, Liebowitz and Margolis question the historical evidence, the theoretical basis, and the policy implications drawn from the economics of QWERTY.

Much of the book is derived from journal articles appearing from 1990 to 1998 (for example Liebowitz and Margolis 1990, 1994, 1995a, 1995b, 1996, 1998a, 1998b, 1998c) and reflects the historical progression of Liebowitz and Margolis's research. They began, in 1990, by examining the historical accuracy of the QWERTY story and, stimulated by what they found there, were led to examine other historical cases, like the Beta/VHS videocassette case and the Macintosh/Windows case. In each instance they found the evidence for sub-optimal ("inefficient") outcomes wanting. They then re-examined the theoretical basis underlying the conventional wisdom and provided an accessible but rigorous understanding of the concepts involved. Liebowitz and Margolis show also that the concepts of "network externalities" "path dependence" and "lock-in" are ill-defined and inconsistently used and they provide a definitive taxonomic clarification.[2]

In the process of considering both history and theory, the relationship between the two comes into question. Liebowitz and Margolis raise some important questions about the proper role of economic modeling. They show that almost all of the discussion in the literature is about alternative economic models and not about the real world. There are an infinite number of possible economic models, many of which exhibit lock-in. The important question is which models are relevant to economic reality and, even more important, to economic policy?

Concerning policy, the literature on network-effects underlies much of the new antitrust policy initiatives that have manifested in the ongoing case of the Justice Department against Microsoft. Liebowitz and Margolis have analyzed the Microsoft case and the implications for antitrust more generally. They find that these new initiatives make little sense if the objective is to benefit consumers, and are fraught with dangers to the competitive process and the dynamics of innovation.

1.3. Overview of What Follows

In this article, I provide an appreciative (but not completely uncritical) review of Liebowitz and Margolis's work as found in their new book—involving these various aspects of the economics of QWERTY, namely, the history, theory, and policy aspects involved. In the process, I highlight in some detail the incipient, and belated, debate that is now emerging between, most notably and explicitly, Paul David and our authors. This debate is provocatively illuminating of the, mostly implicit, methodological principles and presumptions that modern practitioners of economics bring to their work; and exposing and analyzing them in the context of this debate has important implications for the relationship between theory and policy that will be of interest to those familiar with Austrian economics and kindred "evolutionary," "institutionalist" or "dynamic" perspectives. As a less important, but interesting, by-product we will gain insight into some of the more unfortunate and frustrating practices that characterize the current economics establishment.

In section 2, I provide some detail on the history of QWERTY (the history of the typewriter keyboard and the history of thought about the typewriter keyboard). This is important for the subsequent discussion, but may be skipped or read lightly by those familiar with the issues. In Sections 3, I examine the theory involved, also in some detail but not comprehensively. (In Subsection 3.2 I offer a quick survey of some of the key concepts.) Finally, in Section 4, I turn to the question of policy, which is the focus and the climax of this article, since I maintain that the entire debate is, at one level or another, really about the policy implications. I close, in Section 5, with some concluding summary observations.

2. A History of the History

Chapter 2 of Liebowitz and Margolis's book is a reprint of the article that started it all (Liebowitz and Margolis 1990, provocatively titled "The Fable of the Keys"). It is a critical examination of the historical assertions made by Paul David. I begin then with a brief look at these assertions.

2.1. David's Story

> 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 shall see, 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, 1997b, 1999a, 1999b) 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, David tells us that the QWERTY story is a story of path dependence. Because, "history matters," sometimes in an irrational way, the historical path that a particular technological development takes can be decisive in locking in an alternative that is,

in some meaningful economic sense, inferior to another that is available. This was what happened with the adoption of the QWERTY keyboard. A rival design, by August Dvorak and W. L. Dealey was superior, but lost out to QWERTY. This was because of "*technical interrelatedness, economies of scale*, and *quasi-irreversibility of investment*. They constitute the basic ingredients of what may be called QWERTY-nomics" (David 1985:334, italics original).

We may leave aside for the moment the theoretical questions raised here and concentrate in this section on the historical evidence that David presents (or fails to present).

> Devotees of the keyboard arrangement patented in 1932 by August Dvorak and W. L. Dealey have long held most of the world's records for speed typing. Moreover, during the 1940's U.S. Navy experiments had shown that the increased efficiency obtained with DSK [the Dvorak Simplified Keyboard] would amortize the cost of retraining a group of typists within the first ten days of their subsequent full-time employment.... [Dvorak died in 1975 after bearing forty years of] the world's stubborn rejection of his contribution (David 1985:332).

This unfortunate outcome was not the result "of custom, conspiracy or state control." Rather it reflects the behavior of individuals "held fast in the grip of events long forgotten and shaped by circumstances in which neither they nor their interests figured." They were, to quote Tolstoy, held "in bondage to the whole course of previous history" (*Ibid*. 332). The whole thing was the result of an attempt to place the keys in such a configuration that would avoid the tendency for them to become jammed. When, with later technology, typewriters no longer used jamable keys, so that this was no longer a relevant consideration, it was "too late" to change. The installed base of software in the form of typist-human-capital was too great a barrier in the way of introducing the more rational DSK.[4]

> Although the initial lead acquired by QWERTY through its association with the Remington was quantitatively very slender, when magnified by expectations it *may* well have been quite sufficient to guarantee that the industry eventually would *lock-in* to a de facto QWERTY standard (David 1985:335, italics added).

David explicitly links his somewhat impressionistic overview with the theoretical contributions of Brian Arthur (1983) and Michael Katz and Carl Shapiro (1983). This literature features situations in which "essentially random transient factors are most likely to exert great leverage" and a "particular system *could* triumph over rivals merely because the purchasers of the software (and or hardware) expected that it would do so" (*Ibid*. 335, italics added).

> From the viewpoint of the formal theory of stochastic processes, what we are looking at now is *equivalent* to a generalized "Polya urn scheme'. ...[A]n urn containing balls of various colors is sampled with replacement, and every drawing of a ball of a specified color results in a second ball of the same being returned to the urn; the probabilities that balls of specified colors will be added are therefore increasing (linear) functions of the proportions in which the respective colors are represented within the urn. ...[W]hen a

> generalized form of such a process (characterized by unbounded increasing returns) is extended indefinitely, the proportional share of one of the colors will, with probability one, converge to unity (David 1985:335, italics added).

Which of the colors (or rival typewriter keyboards) will gain dominance, however, is "likely to be governed by 'historical accidents,' which is to say, by the particular sequencing choices made close to the beginning of the process" (*Ibid*. 335).

2.2. The Fable of the Keys

Two things are of note in this account of David's. One concerns the historical accuracy and completeness of the story he tells. The other concerns his characterization of it as *equivalent* to a particular stochastic dynamic process. The second will be considered later.

Concerning the history, Liebowitz and Margolis devote most of their 1990 article (Chapter 2 of the book) to a careful examination of the historical record (pages 23–37). They provide considerably more detail than David, with extensive citations (David has very few citations). They point out that the evidence from the many typewriter experiments is ambiguous at best and plausibly tainted by serious conflict of interest and methodological shortcomings. They document the many typewriter competitions that occurred with mixed results and the rivalrous competition among typewriter producers that David fails to report. In all, the assertion that QWERTY is an inferior standard cannot be sustained.

The case of the typewriter keyboard would appear to be especially suited to an assessment of "efficiency"—one that is not dependent solely on market results. This is because "what counts" for consumers of typewriter services can be boiled down mainly to two readily measurable dimensions, speed and accuracy in producing text. (Other dimensions, for example, the durability of the typewriter, can be standardized easily for comparison). So, if tests in these dimensions produced results that clearly contradicted the "market's choice," this would, at the very least, give us pause. One would have to wonder why obvious cost savings had been passed up. Other cases are not so readily reducible to clearly measurable dimensions. And when many dimensions are involved, it is hard to know what the relative importance of each is for the consumer. (For example, in videocassettes, consumers care about picture quality, playing time, cassette size and product durability in ways that are not immediately apparent without resort to observation of their market behavior.) In this respect, the typewriter case is, indeed, a sort of paradigm case.

An early work looking at typing speed was by Dvorak himself and some coauthors. In a book published in 1936 they compared the "typing speeds achieved in four different and completely separate experiments conducted by various researchers for various purposes"(26).[5] From one of these studies "the authors claimed that ... students learn Dvorak faster than they learn QWERTY" (26). Liebowitz and Margolis point out that in this and the other studies, no control group was used, and the samples were drawn from completely different populations (of different ages, different abilities, at different times), so that all that is really established is "that it is possible to find studies in which students learning to type on QWERTY keyboards appear to have progressed less rapidly in terms of calendar time, than Dvorak's students did on his keyboard." Also, what evidence there is "is mixed as to whether

students, as they progress, retain an advantage when using the Dvorak keyboard, since the differences seem to diminish as typing speed increases" (26). In general the Dvorak book lacks both sound experimental method and objectivity.

The Navy study that David referred to was obtained by Liebowitz and Margolis with difficulty. It was conducted in two parts in 1944. Both parts claim to show that retraining on the Dvorak is easier than on QWERTY. Again, various deficiencies in the experimental methodology are noted, including a serious truncation of the sample of QWERTY subjects (28). The tone of the report lacks objectivity. This impression is reinforced by a strong suspicion that Dvorak himself was involved in the administration of this study at the Navy (Lieutenant Commander August Dvorak was the Navy's top expert in the analysis of time and motion studies at the time). "We also know that Dvorak had a financial stake in this keyboard. He owned the patent on the keyboard and had received at least $130,000 from the Carnegie Commission for Education for the studies performed while he was at the University of Washington" (29). While these facts do not, in themselves, establish definitely a lack of objectivity, they are surely worthy of note.

Perhaps more important, is the ignoring by almost all writers on this subject, David included, of a 1956 General Services Administration study by Earle Strong. "Strong conducted what appears to be a carefully controlled experiment designed to examine the costs and benefits of switching to Dvorak. He concluded that retraining typists on Dvorak had no advantages over retraining on QWERTY" and "would never be able to amortize its costs" (29). Liebowitz and Margolis do not consider Strong's study to be without faults, but contend that it should be taken seriously instead of being ignored.

Even a current proponent of the Dvorak, like Yamada (1980, 1983), "as much as admits that experimental findings reported by Dvorak and his supporters cannot be assigned much credibility." And much of the evidence Yamada uses can actually be used to make a case against Dvorak (31).

Liebowitz and Margolis also consider evidence from the ergonomics literature, which is more current and arguably more "scientific." "The consistent finding in the ergonomic studies is that the results imply no clear advantage for Dvorak" (33). In fact, these studies suggest that there is a strong possibility that "the limitations of typing speed ... [may] have something to do with a mental or, at least, neurological skill and fairly little to do with limitations on the speeds at which fingers can complete their required motions" (34).

Competitions between expert typists provide another type of (limited) evidence. "[T]yping contests and demonstrations of speed were fairly common" at one time involving "many different machines, with various manufacturers claiming to hold the speed record" (36). In the 1880's Remington's champion Frank McGurrin won a number of victories for the QWERTY keyboard. "[O]n January 9, 1889, the *Times* reported a McGurrin victory under the headline 'Remington Still Leads the List'"(37). There were other types of machines besides the Dvorak but the evidence is complicated by the lack of standardization of the abilities and training of the various contenders. Touch typing was not common. Suffice it to say, that there is absolutely no presumption indicated that QWERTY was an intrinsically inferior design.

The final, and perhaps the most important, type of evidence pertaining to conclusions about the QWERTY standard relates to the details of the market process by which

QWERTY emerged. As Liebowitz and Margolis tell us it "was not invented from whole cloth." Quoting Yamada (1983:177): "Examination of these materials reveals that almost all ideas incorporated into Sholes' **[**QWERTY**]** machines, if not all, were at one time or another already used by his predecessors" (34). The emergence of QWERTY was in fact the result of a fairly long and complex rivalrous process between numerous competitors. Many of these manufacturers offered free training for typists, suggesting that conversion costs was not an issue. Also, from a manufacturing point of view, innovations like the IBM Selectric type ball, imply that switching to different configurations was not an expensive proposition. Liebowitz and Margolis conclude that "we cannot attribute our inheritance of the QWERTY keyboard to a lack of alternative keyboards or the chance association of this keyboard arrangement with the only mechanically adequate typewriter" (35–36).

In what follows it will be very important to be clear about *exactly* what it is that Liebowitz and Margolis are asserting in this chapter (the 1990 article) and what they are not asserting. They explicitly state that they are *not* asserting "that QWERTY is proven to be the best imaginable keyboard." Neither are they claiming "that Dvorak is proven to be inferior to QWERTY." Rather their claim is simply "*that there is no scientifically acceptable evidence that Dvorak offers any real advantage over QWERTY*." (44 n. 47, my italics). What constitutes "scientifically acceptable," and who bears the burden of showing this, will be something that occupies us below.

2.3. No Response?

In the meantime, we can conclude from the arguments made by Liebowitz and Margolis (1990), surveyed above, that some significant questions have been raised that, *at the very least*, deserve to be considered. The absence of even an attempt to deal with these questions, indeed the almost complete ignoring of them by those active in this field of economics,[6] is troubling in the extreme. On the matter of the history the case for QWERTY-nomics is under a cloud of doubt and suspicion.

In particular, 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, a more complete one casting doubt on the suitability of the QWERTY story as a 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.

After some nine years plus 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, 1997b 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 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).

And in a footnote to this:

> The historical arguments and evidence offered to support that critique are re-examined in my forthcoming paper 'Neoclassical economists at the keyboard: Is there a cure for 'repetitive thought injuries'? David 1997a:7 n.3)

To the best of my knowledge the paper referred to has not been forthcoming. One would hope that, in our profession, protestation and insinuation (as amusing as it might be for the author) would not substitute for real argument. The response to "The Fable of the Keys" has still to be made.[7]

3. The Theory

In "The Fable of the Keys" Liebowitz and Margolis introduce the subject with a critical survey of the relevant theory and discuss it again in the conclusion, and it forms a large part of the book. It is the subject also of a large and growing literature in the journals and advanced texts. 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 and it is my purpose to make this as plain as possible.

3.1. The Basics

The relevant theory can be broadly characterized as the theory of *network-effects*. (Liebowitz and Margolis introduced the term network-effects to substitute for the more common term network *externalities*, to account for the possibility, indeed the likelihood, that these effects are often internalized (68)). Network-effects, relating to the consumption of a particular good or service, occur whenever the benefits of consuming that good or service (most often service) depend positively on the number of individuals who do so. So an additional consumer adds benefits to the consumption of other participants. This phenomenon is not new and is extremely common. Indeed the social institution of "the market" itself is a

network. The benefits to all participants often, as Adam Smith realized, depend on its extent. Languages are networks. The value of learning a particular language often depends on how many speakers there already are. In fact, network-effects occur whenever benefits are related positively to the interaction of individuals within the network. Others examples are telephone networks, local area computer networks, clubs, trade associations, and of course the internet.[8] Network-effects are an example of economies of scale (increasing returns to scale) on the demand side as distinct from the more traditional economies of scale in production, with which they are sometimes, but should not be, confused.

Though common, network-effects are more important for some types of goods[9] than others. They have been given prominence recently because of the proliferation of so called "knowledge-goods," though, as Liebowitz and Margolis point out (and as readers of this journal do not need to be reminded) knowledge is a dimension of every good. The connection between knowledge-goods and network-effects, however, has been related to the fact that the usefulness of any knowledge possessed, often depends on how many others have similar knowledge (demand side economies of scale), *and* (not always correctly) to the fact that knowledge consumption is non-rivalrous, that is, it can be duplicated without cost (or almost without cost). One person's knowledge use does not preclude another's (implying supply side economies).[10] I discuss this further in a moment.

There is, therefore, a strong connection between networks and *standards*. A standard is a "shared way of doing things, of interacting." Standards serve to coordinate individual activity by reducing costs of interacting. A common language is a prime example. Common software would be another. Obviously the relative benefits of a particular common standard are related to the presence or absence of devices for cheaply converting from one standard into another-analogous to the presence of a competent language interpreter. Standards may be fixed or flexible to some greater or lesser degree. Many standards, like languages, legal systems, operating systems, etc. evolve over time. Their benefits are a complex function of the degree of stability and flexibility that they exhibit.[11]

Liebowitz and Margolis point out that networks likewise come in many shapes and sizes and vary along a few dimensions (Chapter 4). First, networks may be literal or notional. An example of a literal network is a telephone exchange. An example of a notional network is the network of Yale Law School graduates (68). Second, networks may be owned or unowned. This may be crucial in assessing the economic properties of the network. For example, an owned network does not exhibit any "externality problem," even though some of the benefits of consumption of the good involved are "external" to the individual. Even though each individual fails to take account of the benefit that he/she confers on others by being in the network, the owner of the network has an incentive to do so and will charge for "membership" accordingly. In contrast, we shall see, an unowned network presents properties that are more interesting and challenging from the standpoint of static allocational efficiency.

As mentioned above, network-effects are economies of scale in demand. As such their existence is an "empirical" matter. Actual networks may exhibit economies or diseconomies of scale. The same is true of supply side (or production economies). "The currently popular association of new technology with increasing returns may well be faulty, at least for some technologies" (81). In particular, as suggested above, network-effects should not be confused

with the decreasing costs of production that have characterized many new industries in the past (like refrigeration, automobiles, etc.) and that have been attributed to many of the new "information age" industries of today. Decreasing costs for the latter should also not be simply presumed. For example, it is sometimes argued that software production exhibits a high degree of increasing returns in production. Once the product has been developed, and once production is in place, the marginal cost of producing an extra copy is negligible so that it is possible to "spread" the fixed setup and development costs over a larger and larger volume of production forever reducing average costs. This, however, is only part of the story. Typically, increases in software consumption eventually imply increases in costs from other sources. As Liebowitz and Margolis illustrate:

> Assume ... that there is one technical-support specialist for each 25,000 users [of] ... Windows 95. If the hiring of additional technical-support personnel tended to bid up their wages, this diseconomy alone could overwhelm the decreasing average fixed cost. Suppose, for example, that hiring an additional technical-support specialist (for 25,000 additional users) increased the wages of technical-support specialists by $22 a year, or an hourly wage increase of a penny. This small change in wages would be sufficient to make overall average costs increase, not decrease with output (81–82, footnote omitted).

Generally, knowledge-goods like software, are produced and consumed together with a complex of other goods (sales, marketing, public relations, management, distribution, etc.) that may not be subject to increasing returns, and increases in software production may thus be associated in increasing costs from a variety of sources. Bottom line, "without investigation, it is unreasonable to accept that the law of diminishing returns somehow takes a vacation in new-technology industries" (82). Some of what is observed as economies of scale is no doubt explained instead by phenomenal improvements in the technology of production as a result of successful innovative activity, something that is much more difficult to characterize and analyze.

It is undeniably true that the production of software and similar contemporary goods exhibit a particular structure that is worthy of note, namely, instant scalability—the ability to expand production with little or no time lag (82). Replication of these goods is relatively easy. This may be important in considering firm and industry structures and the nature and types of competition one is likely to observe.

3.2. The Question of Efficiency

All this is interesting and relevant to an understanding of the modern economic landscape with its bewildering variety of new "information age" products. But its relevance has been substantially enhanced by recent discussions about economic efficiency and related policy implications. These discussions take the form of abstract theoretical speculations about the efficiency properties of various processes usually (although not necessarily always) associated with network-effects—processes that exhibit path-dependent lock-in. Crucial to an assessment of these discussions is clarity on the concepts of efficiency, equilibrium,

path-dependence, and lock-in. Some brief critical remarks follow (this is not intended as a comprehensive treatment of these terms).

Efficiency. Economists have searched long and hard for a concept of efficiency that is "objective" or value free. Economic outcomes consist of an array of goods and services and states-of-being of different individuals that are in themselves incommensurate. In order to pronounce one outcome more or less efficient than another one has to have a way of overcoming this "apples and oranges" problem. Commonly one resorts to attempting to appeal to the valuations placed on the outcomes by the affected individuals themselves. One appeals, that is, to individual preferences in deriving efficiency criteria. This obviously involves the decision that individual preferences ought to be what counts when deciding efficiency issues. In itself, however, this is merely a *definition* of efficiency. No value judgment is involved (beyond that of supporting a definition) unless one says something like, "efficiency is good" or "a more efficient outcome is a *preferred* outcome." When we do take this step, as we often do in policy discussions, what we are saying is that we believe that individual preferences ought to count in deciding what economic outcomes are preferable. This will perhaps strike readers as eminently reasonable. If what is "efficient" is defined as what the "people prefer," how could we not be for it? Is it not the quintessence of "economic democracy"?

As is well known, however, there are numerous practical difficulties in deciding what "people prefer" and, indeed, what this means. When changes are contemplated in which all of the individuals who are involved clearly gain from the change (that is, can be confidently said to prefer that the change be made), then there is little ambiguity and we have the well known Pareto improvement. (So, for example, in the typewriter case discussed above, one may be able to argue that, other things constant, a keyboard layout that is able to unambiguously deliver a faster typing speed would definitely be preferred by *everyone* concerned.) The most common difficulty comes from situations in which some individuals gain and others lose. In such "mixed" situations, we have to resort to so-called compensation tests, that is, somehow judging whether the gains outweigh the loses. If we take this leap, we are, in effect, saying that, in deciding matters of efficiency, the distribution of gains between individuals is not relevant. Of course, a standard defense is that it might be relevant, but that it is a separate issue—we ought to make the pie as large as possible before we consider how it ought to be divided up. This involves a new additional value judgment and clearly is a much less plausible and easily defensible position than one that simply says "people prefer the change." To be sure, it is still a kind of economic democracy—it says something like "more people prefer it" or "the intensity of the preferences far outweigh the intensity of the preferences against." (Again, in the keyboard case, we would not consider it reasonable for holdouts of "inferior" keyboard layouts to be able to block the adoption of a "superior" layout. This judgment may be couched in Pareto efficiency terms by noting that the truly "superior" keyboard would be able to deliver cost savings in excess of the losses suffered by those wedded to an "inferior" one.)

This efficiency standard is widely accepted in economic discussions and has penetrated deeply into the policy and legal environments. The situation is complicated because the word "efficiency" has a very strong colloquial connotation and in economic policy discussions

is often confused with what is meant by efficiency in the natural sciences where inputs and outputs are so much more easily identified and evaluated and no compensation criteria are necessary. It lends to economic policy discussions a spurious aura of being "scientific."

Economists encourage this impression in spite of the fact that they are well aware of the insurmountable obstacles to arriving at unambiguous decisions about which changes are efficient and which not. These involve the well known impossibility of discerning individuals preferences, having to use *hypothetical* market valuations instead, and of having to posit unknowable counterfactuals, often having to do with unknowable (even unimaginable) futures. The real drawback, however, of this traditional efficiency standard, I shall suggest, has not so much to do with its theoretical conception *per se*, as it does with the way in which it is traditionally used in economics, that is, in *the context of the static model of resource allocation*. It is in this context that it has encouraged the kind of attacks, in the name of efficiency, on the unfettered emergence and development of products and standards that we are witnessing in this literature.

In a static context, in which the value of all potential resource uses are known (either with certainty or "probabilistically"), in which technology is unchanging, the set of products is fixed and there are no external effects of any kind or any elements of monopoly, it is well known that a "competitive solution" is also Pareto optimal and, therefore, efficient in the sense discussed above. This idealized situation of neoclassical "perfect competition" has unrealistically and unreasonably served as a standard of comparison for actual real world situations. In particular, in the context of network-effects and standards, economists have thought it relevant and meaningful to argue that the presence of such effects suggests that private markets might provide an "inefficient" result. I shall argue that these attacks are ill-informed and based on an unwarranted presumption of knowledge as well as an irrational concept of efficiency.

Equilibrium. Equilibrium is closely connected to the concept of efficiency. It is, however, even more widely and less self-consciously used. I have elsewhere dealt at some length with the different possible meanings of equilibrium and their implications (Lewin 1997, 1999: Chapter 2). Of particular interest to us here is the tendency for writers to make connections between theoretical processes that end in some sort of equilibrium with actually existing and observed processes and outcomes in the real world.

As with the concept of efficiency, the concept of equilibrium has migrated from the natural sciences, where it connotes some type of stasis, a stable configuration of variables. In economics one cannot understand equilibrium without reference to human behavior and, therefore, to human cognition. Following Hayek many theorists define equilibrium in terms of the plans and expectations that individuals have. A situation is said to be in equilibrium when people's plans are mutually consistent and realistic; that is to say, when people have plans that are mutually compatible and can be successfully carried out. In such a situation there are no surprises, no one has any reason to "change his/her mind." There is no change in an economically meaningful sense.

Such an equilibrium never exists as such, though aspects of individual plans must be consistent if we are to have life as we know it. In the realm of economic activity, however, and particularly in the area of the "new technology" industries there is no equilibrium to

speak of. The whole process is driven by *differences* in opinion and perception between rival producers and entrepreneurs. Where people have different expectations about the same situation, at most one of them can be right. The values they place on the resources at their disposal or which they trade, are not, in any meaningful sense, equilibrium values. They reflect only a "balance" of expectations about the possible uses of the resources. One cannot use such values meaningfully in any assessment of efficiency (in the sense discussed above).

Path dependence. The concept of path dependence is also not new and it also has links to the natural sciences. In economics it gives expression to the common sense idea that equilibrium values might depend on the path taken to get to equilibrium. This is most obvious already in the realization that "false trading" may imply income effects that affect the final prices and quantities of a set of products in a general equilibrium system. But it is much more general and, one suspects, ubiquitous. One should surely not be surprised to find that the equilibrium values of most economic systems is likely to be affected by events that lead up to the attainment of equlibrium, that is, the equilibrium values are not insensitive to the path taken to get to equilibrium. The fact that, as explained, we may never get to equilibrium speaks to the relevance of the whole discussion. May we assume that it is relevant to an assessment of which path is chosen even if equilibrium is never attained?

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. I shall consider this in due course. In the meantime we should note the definition he 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 and we should stipulate here, that *one cannot ignore history and do good economic analysis*. And it is no doubt true that neoclassical economics is hopelessly short on history. The real issue, in this particular debate however, concerns economic policy (not the neglect of history) I turn to this in the next section.

Lock-in. In the current discussion, path dependence gains added relevance because it is seen to attach to systems that exhibit network-effects. In particular, the fact that the benefits of being in the network depend, in part, on how many individuals already belong, suggests that, among competing networks, whichever gets started first may foreclose the development of the others simply by virtue of being there first, and not from any economic merit. This could then be seen as a "market failure," a failure of the market to unfailingly deliver the "best" standard. To be sure, lock-in may or may not be a problem. It is only a problem if one becomes locked-in to an inferior standard.

We are now in a position to consider some of the theoretical contributions in this field.

3.3. Theory and Efficiency

It is fair to say that the theoretical contributions in this field of economics are almost exclusively in the form of a series of exercises designed to show how various types of sub-optimality can occur. A typical example is the set of articles collected in the *Journal of Industrial Economics*, March 1992. In the introductory overview, Richard Gilbert (the editor of the symposium articles) provides a sampling of the findings.

> The need for standardization is a constraint on product variety ... The five papers that appear in this symposium address how successfully markets make this tradeoff.[12] . . . Unfortunately, coincidence between the compatibility choice that is best for producers and the choice that is *best for economic performance* is not likely to hold in many real situations (Gilbert 1992:1, italics added).

For example (it is worth quoting at length to get the flavor of the assertions made):

> Katz and Shapiro (1992) showed that in a market with network externalities, the sponsors of technologies that differ in the size of the installed base may have different preferences for compatibility. For example, a dominant firm might prefer a technological design that is incompatible with other alternatives, thereby denying a new entrant the benefits of the installed base.... [We may require] firms to produce products that conform to set standards ... [but this] is a potentially costly requirement. Standards limit flexibility ... [and] may constrain technological progress. ... [an alternative is] the development of products that allow consumers to use different technologies. Farrell and Saloner (1992) study the economics of (two way) "converters."
>
> ... *Markets fail* to give consumers the right incentive for investment in imperfect converters.
>
> ... *Markets may fail* to give consumers the correct incentives to join one network instead of another. As a result, the success of a network is likely to be determined by

> consumers' expectations about which network will prevail and by choices made in the past.
>
> ... *converters can exacerbate the problem* [italics original] of incorrect market incentives. Converters encourage consumers to choose the technology that best suits their private preferences. But consumers fail to take account of the benefits that their patronage would confer on an alternative network [and this] ... does not generate as much total benefit for society as a whole. In Farrell and Saloner (1992) standardization provides greater total surplus unless consumers have sharply differentiated preference for different technologies ... (Gilbert 1992:1–3, italics added, except where noted)

And so on.

> [There is] a common theme. When production and consumption decisions are interrelated, either through network-effects or through complementary products, a competitive market does not necessarily send the right signals to firms and consumers for the delivery and purchase of goods and services. The *market fails* to reward consumers for the benefits they bring to a network or for the costs they impose by leaving the network. As a result, consumers who follow their own private interests may support more (incompatible) competing products than *would be desirable for total economic surplus*. The market would make the wrong tradeoff between product variety and network economies of scale, sometimes leading to too much variety and not enough technological compatibility (Gilbert 1992:7, italics added)

Note that either too much or too little variety my emerge from these models-they are models in which suboptimal "lock-in" *and* "lock-out" may occur. Suboptimal standards may be adopted too early (as with QWERTY) or the adoption of an optimal standard may be suboptimally delayed—there may be too much or too little variety as opposed to uniformity. So uncertainty about the emergence of optimality applies on both sides of the issue and would necessarily also be attached to any remedial policy. It is noteworthy that nowhere in Gilbert's introduction, nor in the papers are we told how we could identify such "market failures" or what we could or should do about them. Almost as an afterthought Gilbert adds two caveats:

> Market forces might produce new institutions, *not addressed in these models* to deal with these inefficiencies.... In addition there are a myriad of unknowns concerning the performance of new technologies, the ability of firms to deliver desired services, and consumer behavior, *all of which could influence the efficient structure* of supply in markets with network-effects and complementary products (Gilbert 1992:7, italics added).

3.4. Models and Methods

The above is offered as typical of the kind of work that is being done in this area. It is this type of work that Liebowitz and Margolis criticize in much of the book. In doing so they

raise crucial questions not only about this work, but also about the relationship between models, methods and reality in general.

Their attack proceeds on two broad fronts:

1. They point out that the building of models is not a substitute for empirical, historical research—the investigation of real case studies in order to decide which of the infinite types of models that can be constructed is likely to be relevant, and they provide a number of such studies in the book.
2. They subject the prevailing theory to in-depth examination and demonstrate that a much wider range of results than those typically derived (and illustrated above) is not only possible but is likely. Applying what we know about the historical functioning of markets there are theoretical (in addition to empirical) reasons for doubting the existence of the so-called inefficiencies purported to be characteristic of these situations.

I consider these in turn, the first point more briefly.

"Economists often like to make their work appear to offer the same certainty as mathematics, presenting much of their work as 'proofs'" but "proofs in economic models are not proofs about the world. However much a model may prove, it can never prove that the assumptions it starts off with are the right ones" (49). An examination of the historical case record suggests that "QWERTY worlds are awfully hard to find" (50). They look at a series of cases including the VHS versus Beta case for videocassettes, the WindowsDOS versus Macintosh case, and a whole series of cases in the software industry (to which we shall return). In each case they find no evidence for any kind of inefficient lock-in such as the dominance of early starters, lack of rivalrous activity, absence of technological innovation, or even the diminution of competitive activity.

Of course this raises the basic methodological issue, alluded to above, of how such investigations should proceed in the first place, an issue that neither Liebowitz and Margolis nor David or anyone else in this field seems to have addressed directly. This is an issue we shall have to examine at some length below.

Concerning the second point, Liebowitz and Margolis provide a series of crucial theoretical insights. Let us begin with the concept of path dependence. Liebowitz and Margolis link this concept with its potential policy relevance It is not hard to agree that "history matters." In this context, and in a number of their other comments, Liebowitz and Margolis concede the serious limitations of the model of perfect competition and related constructs. Outcomes in the world depend in a variety of ways on history. So much is clear from observing that the capital stock consists of durable items that are often highly specific in form and function. "Mistakes" in investment endure over time. That is a form of simple path dependence. Persistence in human affairs can be called *first degree path dependence* (52). It has no obvious policy implications. This does not imply it should be ignored, quite the contrary an understanding of the present (without any necessary efficiency judgment) demands an examination of the past.[13]

A slightly "stronger" form of path dependence follows from the observation that many outcomes are (in whole or in part) the subject of regret. That is to say, a retrospective evaluation of an outcome may evoke the opinion that it is not the most preferred of the alternatives that *were* available. This is called *second degree path dependence* (53). It is

also likely to be quite common, although not so common as first degree path dependence. We should note that its identification relies on the (necessarily speculative) identification of counterfactual historical alternatives. Except insofar as an assessment of the past is informative for the future, it too has no obvious policy implications. History is history, what is done is done.[14]

A much stronger form of path dependence refers to outcomes that are judged to be inferior *and were known to be inferior when the past decisions that led up to them were taken*. This is referred to as *third degree path dependence* (54). Liebowitz and Margolis point out that this type of path dependence implies the notion of *remediability*, that is, because of some remediable impediment, like the costs of coordinating decision makers, an outcome that is less preferred by everyone concerned nevertheless emerges. It is only third degree path dependence that has any possible policy relevance. As they argue: "for an inefficiency to be economically relevant, there must be some better alternative that is *feasible* in light of the *information* that we have at the time that we are making a decision" (54, italics added).[15]

This simple taxonomy has at least two very important implications (in spite of vigorous attempts to discredit it by Paul David (1997a, 1997b)).

1. It focuses attention on the key ingredient of any discussion of policy and inefficiency, namely, *the importance of knowledge and who has it*. Asserting that one path is economically inferior to another must presume some knowledge on behalf of the economic-theorist-cum-policy-maker. And if that knowledge is available to the policy-maker, it is presumably also available to the economic agents concerned. This suggests the second implication.
2. If a path dependent inefficiency is known *ex ante* to exist, then, by definition of efficiency as discussed above, this implies that a Pareto improvement can profitably be made. That is, there is scope for someone to profitably remedy the inefficiency, since the gains available outweigh the losses that would be produced by such a remedy.

The existence of a third-degree-path-dependent inefficiency is thus something of a paradox. It begs the question of its existence. 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." This is an impasse that has been noted by many theorists, for example by E. J. Mishan (1975:699) and again by David (1997a, 1997b, 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.

What this discussion clearly does is to place these issues firmly within the realm of familiar Coasian transaction costs economics (Coase 1960). The Coase theorem suggests that, absent transactions costs and transaction-impeding wealth effects, apparent externalities and other inefficiencies would spontaneously be removed by the market process. The identification of any such inefficiencies thus must be seen to rely on these broadly construed "transaction

costs." This is relevant to third degree path dependent inefficiencies. If such inefficiencies exist, that is, if everyone would prefer, for example, the adoption of a particular standard, but because they expect everyone else to adopt an inferior standard, themselves all choose the inferior standard, so that we become locked-in to a standard that is Pareto dominated; then such an inefficiency may be said to exist because of the high costs of coordinating the activities of the numerous agents around the adoption of the "correct" standard, that is, because of high transactions costs. More generally, if such a lock-in exists because the agents are ignorant of the advantages of the alternative standard, this too may be characterized as a transactions cost problem, since, if it were possible to cost- effectively inform such agents of their errors and to facilitate a coordinated alternative, it would be done. In fact, from one perspective, all transactions costs are *information costs* (Dahlman 1979).[16] In sum, "it must be possible for someone to have credible information that a better allocation [of resources] is available" (54) for path dependence to be policy relevant.

Liebowitz and Margolis are clearly skeptical of claims regarding the existence of policy remediable inefficiencies, or "market failures." This skepticism reflects their conviction that "mechanisms exist to obviate such failures" (68). Where networks can be owned, benefits will tend to be internalized. Where this is not possible, other mechanisms exist to internalize (in whole or in part) the benefits available, for example, through the provision of complementary goods. In addition, the literature on network-effects is misleading to the extent that it tends to emphasize the possibilities for the emergence of inefficiencies. Liebowitz and Margolis provide an extensive examination of the relevant theory, one that reveals a much wider range of possibilities, even while staying within the static allocative framework (*but which is not meant as an argument for the validity or usefulness of that framework*). As this is less pertinent to my main themes I will provide here only a short overview.

Concerning the benefits of particular standards, Liebowitz and Margolis point out that often these benefits are tied less closely to the total number of other users of the standard and more closely to the number of users who actually interact. What concerns them are ways to achieve greater coordination and synchronization with this smaller subset. These "synchronization effects" (network-effects) may coexist with increasing, decreasing or constant returns to scale. Increasing the number of interactors in a network may add to the value of being in the network, though these additions may diminish. To obtain the total value to the consumer of any good, subject to network-effects, one must add this total "synchronization value" to the value that the consumer would place on the good as a sole user, the "autarky value." If the good has a positive supply price then its net value will be the difference between its total value to the consumer and its supply price. "[I]t is only when the net-value function slopes upward that choices between standards are fundamentally different in character from choices of other goods" (95). And if the supply curve slopes upward it is possible that the net-value function will slope downward.

They use this analysis to show that network-effects do not necessarily, or even probably, imply increasing returns to one standard that can be expected to dominate. Multiple formats are not uncommon and theoretical considerations are quite compatible with this. Niche formats are examples (Macintosh, linex, Betamax, all exist in smaller specialized markets). This result is reinforced if we assume that consumers have different tastes. Thus, "the mere

existence of synchronization (network) effects is insufficient to establish the winner-take-all choice with respect to standards" (99) and in fact the case for single dominance is quite weak.

This suggests that one may expect to see competitive strategies in which entrant firms try to specialize in their products that appeal strongly to particular groups of consumers, while incumbents, on the other hand, might try to create products that appeal to the widest possible audience attempting to foreclose opportunities for competitors (106).

They show further that even when there are increasing returns and/or network-effects more than one standard may emerge.

Economic models are like recipes. You tend to get what you put into to them, but the way they taste depends crucially on how you mix the ingredients. Even models that stay within the static allocative equilibrium framework can vary a great deal in their implications. Liebowitz and Margolis show that winner-take-all dominance is in no way a necessary property of models that incorporate network-effects. The bias observed in the literature in that direction is a result of the presumptions of the modelers. There is no way to assess their relevance for economic policy without some way of deciding how closely they correspond to reality.

3.5. David's Critique[17]

Some of Paul David's recent unpublished work (1997a, 1997b, 1999a) on this subject focuses almost exclusively on Liebowitz and Margolis's theoretical contributions, or, even more narrowly, on their definitional framework. As already mentioned, while alleging that Liebowitz and Margolis misconstrue the history of QWERTY, he has not yet explained this. 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 (and one might say convoluted) discussion of the definition and meaning of path dependence. We recall that in "... The Economics of QWERTY" David defines path dependence as follows:

> 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).[18]

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 this is a manifestly incorrect reading of Liebowitz and Margolis (and it is not the only one). They 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 a wholly unwarranted conclusion. In fact, I shall 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 the long, argumentative discourse (David 1997a, 1997b) on correct definitions. 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."

What the "debate" really seems to be about is economic policy. The protagonists are on opposite sides of a fundamental policy divide and, in an attempt to discredit one another are, in effect, talking past each other. We explore this in the next section.

4. Policy

I request the reader's indulgence to engage in some speculative mind-reading.

Paul David probably suspects that Liebowitz and Margolis's 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.

4.1. Placing the Burden of Proof

This can be illustrated in very familiar terms. 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 1 below:

Table 1. Experimental design in searching for policy relevance.

Experimental design	H_0 = the null hypothesis	H_1 = the alternative hypothesis
Design A	An inefficiency exists $\Rightarrow$ (the status-quo is not efficient)	An inefficiency does not exist
Design B	An inefficiency does not exist $\Rightarrow$ (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 advocate 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 (in a most contemptuous way) that it is a rhetorical trick designed to paralyze economic policy (in reference to the Panglossian impasse discussed above) (David 1997b:11).[19] 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.[20] 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 he has misunderstood, or has chosen to mischaracterize, the 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" (15). "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" (117). 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" (239–240).

David places the burden in a different place. In fact he explicitly addresses this in his most recent paper on this subject (David 1999c).[21] 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, 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.

4.2. 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 it would be profitable to organize.[22]

The "knowledge problem" is the crux and it is implicit in Liebowitz and Margolis's arguments, as I will show momentarily. 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. He seems to be suggesting further that moving beyond such a framework would support the type of policy activism he is proposing here.[23]

The static welfare framework is indeed problematic in the extreme and we may readily join David's call for moving the education of economists beyond it. It is not clear, however, that David understands what a "truly dynamic" framework would imply. Market processes

are truly dynamic in the sense that they take place in "real time," they are evolutionary processes that are driven by the diversity of perceptions and expectations that individuals have of the value of resources and of the process itself. They are processes that are "open ended" and are never in equilibrium. They are processes that are characterized by "radical uncertainty" and *novelty*, their outcomes are inherently unpredictable. For such processes the traditional types of efficiency assessments based on static models of resource allocation are completely meaningless. There is no way, for example, of applying the traditional utility calculus to choices among technologies that are not yet available, but that might emerge as a result of becoming "locked-in" to a particular dominant standard. Such a standard, by David's presumptuous evaluation, may appear to be inferior to others that are available, but it also may lead in the future to the discovery and application of complementary technologies that are vastly superior in a number of dimensions. How are we to assess the likelihood that policy action *itself* may be the "accident" that locks us into an inferior path? Insofar as new technologies are based on future knowledge and insofar as future knowledge cannot be available in the present, we cannot consider future technologies as part of today's choice-set. Neither can we include the new products, new methods of production and new modes of organization that they bring with them. Technological change emerges from the complex interaction of individual visions (including those of the policy-makers) that are partial and incomplete and that are subject to the trial-and-error processes of the market. Static welfare criteria are inapplicable to this process. And David's policy prescriptions are manifestly unworkable.

Can one then say anything about efficiency? If one can it will have to be at another level and it will have to be analytically less precise than (though not entirely unrelated to) static Pareto criteria. It is at the level of the institutional framework that efficiency judgments will have to be made. If we can learn anything from history perhaps it is that certain kinds of social, legal, moral, and economic institutions are generally more conducive to the generation of innovation and prosperity than are others. Policy regimes rather than policies should perhaps be the context for this discussion.

An example is the issue of monopoly policy in high technology industries (for an in-depth survey see Teece and Coleman 1998). Teece and Coleman argue that the nature of these industries has made the current anti-trust environment obsolete. "[A]ntitrust policy cannot realistically aspire to produce 'optimal' outcomes, where 'optimality' is measured against some theoretically defined efficiency or consumer welfare criteria" (Teece and Coleman 1998:815). What is required is a new way of thinking attuned to the truly dynamic processes of rapid and unpredictable innovation and change, in which competition and monopoly do not necessarily mean the same things as they used to.

If Liebowitz and Margolis are correct in arguing that network-effects are seldom if ever an effective mechanism for isolating firms from competition, then the kind of criteria by which we should judge the presence or absence of monopoly are not the usual market share criteria. As they point out "in this world, the firm competes not to take its share of today's market; it competes to take the market for its share of days. Legislators and courts may choose to make some ways of competing legal and some not, but the old structuralist approaches to antitrust will only be misleading" (13). The old notion of competition as a state of affairs needs to be replaced by competition as a process over time in which some firms displace others,

sometimes as the dominant or only firm in the industry. Competition may indeed show up as "serial monopoly." "Competition here takes a very different form from that found in text book models of perfect competition.... There really is no 'competing' by firms in models of perfect competition, except to keep costs down" (63–64).[24] We should not base policy "on a worldview that is taken a bit too literally from an intermediate microeconomics textbook. In the real world, information is not perfect; the future is not known with certainty; products are not perfectly homogeneous; and sometimes what we like is influenced by what others like.... From the perspective offered by... the textbook model of the ideal economy, we might well be confused by strategic alliances, by technology-sharing agreements, by long-term guarantees, or by the continuous addition of functionality to a product." And they reject the claims "that winners might more appropriately be chosen by policymakers than by people making choices in a free market" (243–244). This is hardly the stuff of minds wedded to the presumptions of the static competitive model.

Implicit in their work is the role of knowledge—how it changes over time and how it is related to information. Much more can be said about this. In particular, innovative environments are ones where people are free to make mistakes, that is, where expectations at any time are wrong. Put another way, when we realize that at any point of time different entrepreneurs have different and inconsistent expectations about the same business environment, we must realize that at most only one of them can turn out to be right. Knowledge, unlike information, cannot be shared, and what one perceives as knowledge another will see as (an unjustified) expectation. This is fundamentally a disequilibrium situation (meaning that different people have different expectations). *In such an environment why should one assume that policy-makers are ever in possession of privileged knowledge?* Why are they likely to be right more often than any particular entrepreneur about which technology will turn out to be the best standard for consumers? More to the point, is it not likely that they are in a worse position than private businesses in this regard? In this way Liebowitz and Margolis's work is in sympathy with some much more fundamental and far-reaching critiques of mainstream economics.

4.3. "Testing" the Market Process

How then does one evaluate the market process. Liebowitz and Margolis have provided a variety of in-depth historical examinations. They uncover the "problematic" nature of such "evidence" for traditional anti-trust economics.[25] For example, if we "define standard A to be superior if, for all consumers and any given market share the net value of A is higher than the net value of B when B has the same market share" then it is likely that standards that create greater "social wealth" would tend to dominate and market-share winning behavior may appear to be "predatory." Furthermore, "if a clearly superior technology were offered [to the market] first, we would be unlikely to see a sustained attempt to dislodge the leader by owners of inferior technologies, unless they expect that they can achieve their ends through political means, inasmuch as their expenditures in the market are likely to be futile." (109–110). In fact, there is no presumption that policy attempts to remove a particular firm's dominance may not *itself* be responsible for us getting locked-in to an inferior standard.

This is relevant to the case of Microsoft. In order to investigate whether claims that Microsoft has achieved an inefficient monopoly are credible, Liebowitz and Margolis investigated a variety of different software products by looking at market shares in relation to reviews in consumer reports. Computer magazines frequently and extensively evaluate rival products for quality and functionality. They are obviously not "objective" measures of quality, but they probably come as close as one can to ascertaining whether the products consumers use most fulfill their requirements better or worse than the alternatives. It is possible to argue that consumer reports actually provide a self fulfilling confirmation of the tests, because consumers buy those products that the reports recommend, but this would be an extreme stretch. It is probably true that most computer users don't read the reports but become aware of the characteristics of the products by use or word of mouth over time.

Liebowitz and Margolis looked at spreadsheets, word processors, financial packages, desktop publishers, online services, and browsers. They found that:

- "the products that have prevailed have always been the products that were regarded as best by software consumers and by the reviewers in specialist magazines" (237).
- "a large market share, even an extraordinarily large market share, is not a good predictor of a price increase" (238).
- Changes in market share are smooth and continuous. There is no evidence of "tipping" (the reaching of a threshold beyond which lock-in sets in) (192).
- in many products Microsoft achieved dominance in the Macintosh market considerably earlier than in the PC market (195).
- Microsoft tended to charge lower prices in the market where it was dominant than in the market where it was competing (196).
- Rivalrous competition continues to be vigorous.

All in all, the evidence that Liebowitz and Margolis present strikes this writer as sufficient to cast substantial doubt on the case that the government has brought against Microsoft. In some products Microsoft does not dominate, in others it has won its dominance by producing (often with extreme effort) better products. Where it does dominate, "one does not need to appeal to other factors such as those the government has focused on in its case against Microsoft... to explain Microsoft's increasing share of the market" (202–203). We are back to the issue of burden of proof, this time in a very literal and compelling way.

5. 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 such 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 could bare greater emphasis. There is also a lack of recognition of the huge volume of work on the dynamics of the market processes that has appeared in recent years and that might have informed their critique. They appear innocent of the epistemologically-based criticisms of standard microeconomics, though they have come close to independently rediscovering some of this is a small way.

In the final analysis, Liebowitz and Margolis's work is noteworthy for its solidity (its groundedness). They do not plumb the depths of methodology. Yet at the same time, their work transcends their idiom and is pregnant with profound and far-reaching implications. Common-sense and basic historical research leads quickly to highly significant theoretical results and empirical generalizations, not to mention political implications. The hidden (and no doubt unconscious) message is much more revolutionary than it appears.

Liebowitz and Margolis provide a formidable array of objections to some standard arguments. Anyone interested in the issues surrounding the emergence of new technologies and standards and the related policy questions cannot afford to be ignorant of their work. One may hope that this book will at the very least stimulate discussion and further research.

Notes

1. Of course neither may subscribe in the broader sense to the schools of thought I have used to characterize their positions.
2. They also argued that, far from this literature constituting a "new economics" that moves beyond the old obsolete microeconomics, the latter is fully capable of explaining everything that these situations presented. The economics of natural monopoly, of externalities and public goods, of transactions costs and other related well-established themes serve to establish that these new "information age" industries present nothing that is really new in principle, though some interesting new perspectives do emerge. (For example, in reexamining the alleged market failure due to consumption scale economies- the fact that the value of the product depends in part on how many people use it- they point out that this can be interestingly interpreted as the traditional "tragedy of the commons" in reverse.) This general line of reasoning is capable of being misunderstood. To claim that traditional microeconomics can be made to incorporate almost every imaginable case is probably true, but this does not contradict the criticism of the use of perfect competition as a standard in policy discussions.
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. "The occurrence of this 'lock-in' as early as early as the mid-1890's *does* appear to have owed something also to the high cost of software 'conversion' and the resulting *quasi-irreversibility of investments* in specific touch-typing skills. Thus, as far as keyboard conversion costs were concerned, an important asymmetry had

appeared between the software and the hardware components of the evolving system: the costs of typewriter software conversion were going up, whereas the costs of typewriter hardware conversion were coming down." (David 1985:335–36, first italics added.)

5. Unless otherwise stated, page numbers refer to Liebowitz and Margolis 1999.
6. A notable recent exception is the online discussion on the EH.NET referred to in note 1 above and also Puffert 1999.
7. 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."
8. In a sense, network-effects are complementarities in consumption and production and were anticipated by Marshall in his work on industrial districts, trade alliances, etc. and by Gary Becker in his work on family or household economics.
9. I shall use the word "good" to refer generically to a good or service, any "economic good."
10. Actually, this discussion conflates "knowledge" with "information" in an illegitimate way. Information is public in a way that knowledge is not (Lewin 1997, 1999 Chapters 3 and 12). This will become more important below when I discuss the role of knowledge in policy and research.
11. Of course a vast literature on this exists. The work of Hayek (1988) on the evolution of social institutions is relevant as is that of Douglas North (1990). See also Lewin (1997) for a discussion of the connection between stability and flexibility in evolved institutions.
12. This is a typical misstatement. The papers do not address anything about real world markets. There is no real "history" in them. Instead they address how it is possible to produce sub-optimality in theoretically conceived simulations of real world markets.
13. This is not the place to examine the deeper questions of whether, and in what way, any outcomes can be said to be strictly "determined" by the behavior of antecedent conditions, and, if so, what remains of true "choice." 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 shall argue, a fundamental asymmetry between understanding and prediction (much methodological discourse to the contrary notwithstanding).
14. This absolutely does not imply that history is irrelevant or that path dependence (of the second degree) is not present and useful in discussing economic history. Such an assertion seems to have been falsely attributed to Liebowitz and Margolis (see for example the online discussion on the EH.NET and Puffert 1999).
15. 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). I discuss this further below.
16. 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)?
17. There might be less excuse to present so much detail relating in an *ad hominem* way to aspects of the debate were it not for the fact that the subject matter is so vital for economic policy. Paul David's famous article on QWERTY (1985), surveyed above, is widely quoted and cited (his most cited article by far). As shown, the basis of its argument has been seriously questioned and yet its influence continues apparently undiminished. (In this regard, one might quote David's own remarks: "It is an unfortunate fact that by repeating untrue things often enough and doing so in a very confident way, you may eventually manage to surround them with and aura of creditability" (1997b:8)). In addition, David has been less than forthcoming and somewhat

patronizing and cavalier in the incomplete responses he has provided to date.

18. 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).
19. 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!'" (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..." (13), which leaves one wondering whether all this name-calling is itself some sort of rhetorical game.
20. The situation is closely analogous to, but not *equivalent* to, an experiment in which one may assume random sampling from unknown fixed populations. I have not yet discussed what might be considered appropriate tests for inefficiency, but, as should become clear, I have serious doubts about whether classical statistical tests are meaningful in this context.
21. As explained in note 1 above, on first writing this section I was unaware of David's paper. Upon reading it, the rival positions became even clearer.
22. One is tempted also to 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")?
23. 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.
24. Cf. Hayek (1978).
25. "[A]nything that a firm does to compete can be, at some point, viewed as an attempt to monopolize. And anything that a firms does to improve its products, extend its standards, or reach additional markets will look like an attempt to monopolize" (11).

References

Arthur, W. B. (1983) "On Competing Technologies and Historical Small Events: The Dynamics of Choice Under Increasing Returns." Technological Innovation Program Workshop Paper, Department of Economics, Stanford University, November.

Arthur, W. B. (1989) "Competing Technologies, Increasing Returns, and Lock–in by Historical Events." *Economic Journal*, 99: 116–131.

Boettke, P. J. (1996) "What is Wrong With Neoclassical Economics (and What is Still Wrong With Austrian Economics)?" In: Foldvary, F. E. (ed.) *Beyond Neoclassical Economics* Aldershot: Edward Elgar.

Coase, R. H. (1960) "The Problem of Social Cost." *Journal of Law and Economics*, 3: 1–44.

Dahlman, C. (1979) "The Problem of Externality." *Journal of Law and Economics*, 17: 357–376.

David, P. A. (1985) "Clio and the Economics of QWERTY." *American Economic Review*, 75: 332–337.

David, P. A. (1986) "Understanding the Economics of QWERTY: The Necessity of History." In: Parker, W. N. (ed.) *Economic History and the Modern Economist*. New York: Basil Blackwell.

David, P. A. (1997a) "Path Dependence and the Quest for Historical Economics: One More Chorus of the Ballad of QWERTY." University of Oxford, Discussion Papers in Economic and Social History, November 20, 1997, http://www.nuff.ox.ac.uk/economics/history/paper20/david3.pdf.

David, P. A. (1997b) "Path Dependence, Its Critics, and the Quest for 'Historical Economics': Keynote Address to the European Association for Evolutionary Political Economy." Athens Meetings, 7–9 November 1997.

David, P. A. (1999a) "Myth'-Informing The Public About Public Goods and QWERTY." http://www.eh.net/ehnet/Archives/eh.res/apr-1999/0006.html.

David, P. A. (1999b) "At last, a Remedy for Chronic QWERTY-skepticism!" Prepared for Presentation at the European Summer School in Industrial Dynamics (ESSID), held at l'Institute d'Etudes Scientifique de Cargese (Corse), France, 5th–12th September 1999, http://www.eh.net/Publications/remedy.shtml.

Farrell, J., and Saloner, G. (1992) "Converters, Compatibility and Control of Interfaces." *The Journal of Industrial Economics*. XL: 9–36.

Gilbert, R. J. (1992) "Symposium on Compatibility: Incentives and Market Structure." *The Journal of Industrial Economics*, XL: 1–8.

Hayek, F. A. (1978) "Competition as a Discovery Procedure." In: *New Studies in Philosophy, Politics and Economics and the History of Ideas*. London: Routledge.

Hayek, F. A. (1988) *The Fatal Conceit: The Errors of Socialism*. Chicago: University of Chicago Press.

Katz, M. L., and Shapiro, C. (1983) "Network Externalities, Competition and Compatibility." Woodrow Wilson School Discussion Paper in Economics No. 54, Princeton University, September.

Katz, M. L., and Shapiro, C. (1992) "Product Introduction with Network Externalities." *The Journal of Industrial Economics*, XL: 55–84.

Lewin, P. (1997) "Hayekian Equilibrium and Change." *Journal of Economic Methodology*, 4: 245–266.

Lewin, P. (1999) *Capital in Disequilibrium: The Role of Capital in a Changing World*. New York and London: Routledge.

Liebowitz, S. J., and Margolis, S. E. (1990) "The Fable of the Keys." *Journal of Law and Economics*, 33: 1–25.

Liebowitz, S. J., and Margolis, S. E. (1994) "Network Externality: An Uncommon Tragedy." *Journal of Economic Perspectives*, 8: 133–150.

Liebowitz, S. J., and Margolis, S. E. (1995a) "Are Network Externalities a New Source of Market Failure?" *Research in Law and Economics*, 17: 1–22.

Liebowitz, S. J., and Margolis, S. E. (1995b) "Path Dependence, Lock-in, and History." *Journal of Law Economics and Organization*, 11: 205–226.

Liebowitz, S. J., and Margolis, S. E. (1996) "Should Technology Choice be a Concern for Antitrust?" *Harvard Journal of Law and Technology*, 9: 283–318.

Liebowitz, S. J., and Margolis, S. E. (1998a) "Path Dependence." Entry in *The New Palgrave Dictionary of Economics and Law*, Newman, Peter (ed.) London: Macmillan, 17–23.

Liebowitz, S. J., and Margolis, S. E. (1998b) "Network Externalities (Effects)." Entry in *The New Palgrave Dictionary of Economics and Law*, Newman, Peter (ed.) London: Macmillan, 671–675.

Liebowitz, S. J., and Margolis, S. E. (1998c) "Dismal Science Fictions: Network-effects, Microsoft, and Antitrust Speculation." Washington D.C., *Cato Institute, Policy Analysis* #324.

Liebowitz, S. J., and Margolis, S. E. (1999) *Winners, Losers & Microsoft: Competition and Antitrust in High Technology*. Oakland: The Independent Institute.

McCloskey, D. N. (1999) http://www.eh.net/ehnet/Archives/eh.res/apr-1999/0005.html.

Machovec, F. M. (1995) *Perfect Competition and the Transformation of Economics*. London and New York: Routledge.

Mishan, E. J. (1975) "The Folklore of the Market." *Journal of Economic Issues*, 9: 690–720.

North, D. C. (1990) *Institutions, Institutional Change and Economics Performance*. Cambridge: Cambridge University Press.

Puffert, D. J. (1999) "Path Dependence in Economic History." www.vwl.uni-muenchen.de/ls_komlos/pathe.pdf.

Teece, D. J., and Coleman, M. (1998) "The Meaning of Monopoly: Antitrust Analysis in High-Technology Industries." *The Antitrust Bulletin*. Fall-Winter: 801–857.

Yamada, H. (1980) "A Historical Study of Typewriters and Typing Methods from the Position of Planning Japanese Parallels." *Journal of Information Processing*, 2: 175–202.

Yamada, H. (1983) "Certain Problems Associated with the Design of Input Keyboards for Japanese Writing." In: Cooper, W. E. (ed.) *Cognitive Aspects of Skilled Typewriting*. New York: Springer.