
Roundtable discussion Nathan Rosenberg Memorial Issue

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Abstract

During an *Industrial and Corporate Change* conference at the University of California, Berkeley in December 2016, a roundtable discussion on “The Legacy of Nathan Rosenberg and the Importance of Economic History” was held on December 12th, 2016. The discussion was chaired by Professor David J. Teece and included Professors David C. Mowery whose introductory comments form the basis of the present Introduction to this Nathan Rosenberg Memorial Issue. It also included roundtable interventions by Professors Kenneth Arrow, Giovanni Dosi, Uve Granstrand, Richard R. Nelson, and Gavin Wright. The following text constitutes a revised version of the contributions by each participant.

Kenneth Arrow¹

Nate was a very close friend of mine and as a person he was really full of anecdotes, a lively conversationalist, a man of extremely broad interests, well beyond even his study of machinery and developments of new products. I read his papers with great pleasure, though probably not with a deep understanding, because it came from such a different viewpoint than mine. But I always found somehow I was reading a narrative, which seemed to be just kind of a story. By the end of it, it was more than a story. I didn't just learn about machine tools. I somehow learned about the process in a way which I couldn't reduce to a nice formula, but nevertheless it was clear somehow you got an insight in a way which was both concrete and general at the same time.

His influence lives on. He had a great impact on the innovation studies community. The very size of the gathering here is remarkable considering the small number that were involved in this a few years ago.

Of course, I'm interested in why do we have this peculiar set of institutions, including those governing the generation of innovations. We have markets. Why don't markets work? Now, markets don't work in a lot of cases. As soon as you get rigorously into general equilibrium theory, you find out what's wrong with it. One of the things is that you look at the number of markets that's supposed to be there and is not there. Even something as simple as buying a commodity 3 years in advance is hardly possible. When I put up a plant to produce automobiles, I might want to sell the products years out. Why can't I do so? I can do that a little bit with agricultural goods and minerals but only for maybe a year or two. With automobiles, you don't do it. Why not? You don't know what an automobile is

1 Kenneth Arrow was a Nobel Prize laureate and professor of economics at Stanford and Harvard Universities. Professor Arrow died on February 21, 2017, seventy days after these comments were delivered. Professor Arrow's transcript was edited by Professors David J. Teece and Giovanni Dosi.

going to be like in 5 years. I mean, there are other reasons, but that certainly is one of them. You can't specify the automobile.

The fact of innovation as such in a way creates "problems" for economic theory as a whole. Not only is the market for innovations a difficult one, but it creates problems for other markets. What you have here is knowledge, and knowledge is already a commodity. It's a commodity, it's costly, and it's valuable. But it's reproducible at cost much less than the cost of production. That means, furthermore, if you transmit information, you have no less of it. Once you have sold a roll of steel, you don't have it anymore. Whereas with information, once you sell it you still have it *and* the other person has the information too. And, of course, it can be made use of and probably to your disadvantage as the seller. This creates all sorts of problems even within the innovation process itself.

Well, one of the problems is that information is not completely transmissible, so if I am sure that something's going to work, I may nevertheless need financing to test it, and I've got to persuade somebody else of this. And this, by the way, does not imply simply the expected profitability of the innovation, but it could also well apply within, let's say, a nonprofit organization, the military, for example, which has a very large R&D component.

Or consider the pharmacological industry—which doesn't seem to be mentioned today—it's a place where, I think, patents play the biggest role of all compared to other areas. Drugs are generally privately produced, but it's subject to a lot of regulation, and there are also social consequences. So it's not a simple kind of market where you have the enormous expense of testing and things like that. Even before that you have the question of getting to the point of testing, getting the financing to get to that point—and with a high failure rate, by the way. By no means, it's certainly nontrivial. It's a failure rate in the sense of not achieving the results—even before you get to the testing—and that's expensive. But then on top of that when you get to testing, when you get to a point where something *might* work, and you go through the really quite expensive tests, you wind up with a situation where, as in many drugs, the cost of development is huge—while the cost of manufacturing is trivial. That's not always true, but there's a lot of drugs for which that's true.

Financing creates problems because I may be convinced that my idea's certainly going to work, but I've got to get somebody to finance it. But how do you convey, when that information is a little vague, that you've got some evidence that something works? You're convinced that it's worthwhile. You've got to convince others, and so there's a very imperfect kind of information first and, possibly, competition next. Another issue relates to the appropriation of the benefits. The patent system has evolved enormously.

I can remember, being a little older than the average person here, that there was a point when universities—if they developed a product, for example a pharmacological product—it would be considered to be wrong to patent it. Of course there were drug manufacturing firms all along, but if a nonprofit group, if a university developed things, it was considered improper to patent it. It was subject to that it should have been given out free, and I think one of the first examples was the University of Wisconsin developing some kind of substitute for cod liver oil in delivering Vitamin D. If you've ever tasted cod liver oil, you appreciate the difference! The whole idea of vitamins was new then, and the University of Wisconsin developed a more palatable version. They decided they would patent it but, of course, the funds were reserved solely for the purpose of further medical research. Now, no university today would take that decision.

Now, let me comment on the so-called "linear model" (of innovation) and the alternatives to it. In the first National Bureau Conference on the Rate and Direction of Inventive Activity, of which Dick Nelson and I may be the sole representatives here, there was a paper by Simon Kuznets, and it took for granted the linear model. Simon Kuznets—one of the great figures in national income accounting and so forth—just took for granted that scientists went along in a totally exogenous way driven by intellectual curiosity. Later, somebody says, "Oh, this can have a practical application," and then the steps would be made to explore it. Now, of course, there's one very obvious exception to that, which has been true for millennia, or at least a millennium, and that's one of the products of innovation may be instruments for measurement. The telescope is an example. After all, lenses were known since antiquity—but somebody decided to put two lenses together. I don't know why or how. And he said, "Oh, you can detect ships coming in." That was the idea. So telescopes are around. Galileo gets hold of one of them and says, "Well, let's look at the stars instead." It was a revolution, a scientific revolution, in that case. And, in that case, pure science.

Well, it wasn't in the long run so pure because observations of the heavenly bodies turned out to be very, very useful in navigation; so it fed back again. So this idea that improvements in modern astronomy—this detection of

gravitational waves, for example, that was a recent accomplishment at incredible increase of accuracy—are all based on the fact that there's been technological innovations. Certainly, the art of telescoping has been totally transformed.

Now, one question is, to what extent, outside the loops between “pure science” and applied technologies, do “needs” matter? We need something. Or at least the word “need” shouldn't be thought of literally because the object or service is perhaps something we would very much like to have, but some things are impossible, other things are difficult, and yet others are even difficult to imagine. And one of those sad examples to me is the electric car. Electric cars, I guess, are about as old as internal combustion engine cars, maybe a little less, but certainly, if I recall the figures, in the 1900s there was something like 1500 car manufacturers in the United States. A third of them were steam-driven, a third of them were driven by internal combustion engines, and a third of them were electric cars. And the electric cars were quieter and smoother. They were operational. You didn't have to crank them. This was before the invention of the self-starter. And you could be injured if you were trying to start a gasoline engine car and the engine suddenly came to life. I think it was not until 1911 that the self-starter was invented, by Charles Kettering.

Now, there's only one trouble with electric cars, and that's that the battery gives out. And we've known this for 100 years or more. There would be a big market for long-lasting electric batteries or for some way of recharging them or replacing them with something else—it seemed to be perfectly evident that the momentary gain was enormous. So we have to assume that people just didn't know how to do it. Needs were there, but technological constraints at that time were too hard.

I think I'll stop now and we'll have other opportunities.

Giovanni Dosi²

The point that I want to make here is that Nate has been for sure not only one of the Founding Fathers of the Economics of Innovation, but also of Evolutionary Economics. But, somewhat paradoxically, he has always been reluctant to recognize the second child.

All the basic building blocks of evolutionary theory are insightfully there in his work.

Let me just name telegraphically seven:

One, innovation as driver of economic growth. In turn, elements of innovation are “endogenous” in that they are driven by profit motivations of individuals and firms, while others, as Ken was reminding us a minute ago are “coming down” from noneconomic motivated search, with technology preceding from science. Incidentally, on all that, nowadays the denial of the “linear model” has gone too far. The loop goes both ways, as Nate insightfully analyzed, but science is pushing the “endless frontier” heralded by V. Bush (Vannevar B., 1945, not other later Bushes!).

Second, the radical uncertainty linked with innovation, as Ken again repeated. If you know exactly what is an innovation, it's not an innovation [laughter] because it's already there. But, if it is not, there are all the consequences that we mentioned a second ago, about nonprobabilizability, absence of future markets, etc. Ken, again, was reminding it.

Third, given all that, the intrinsic impossibility of maximizing behavior. Rather, actual behavior ought to be depicted as driven by heuristics, routines, habits, rarer deviancies and innovative discoveries. At the same time, when accounting for people and organizations actually do, much more attention must be devoted to the context and constraints as compared to whatever purported reconstruction of the purported motivations. If you want to describe what someone serving time does, you better start by saying that he is serving time, describe the cell, etc. Possibly on the behavioral side the only information worth writing home about is about his possible plans to escape.

Fourth, relatedly, a procedural view of production and innovative search, rather than choice over input and output combinations. Production and innovation is never, “I choose A, or I choose B because they meet expectation on what A and B will yield,” but basically, it is a sequence of cognitive and physical acts that might get me there or might not get me there. And I know if I'm there or not only when I arrive somewhere.

Fifth, heterogeneous technological expectations essentially based on vague intuition, feelings and dreams on the future breakthroughs and technological trajectories. Given that, of course, notice the distance between this type of expectation and the rational expectation that you find in the textbooks, not to talk about *rational technological* expectation. That is just an oxymoron.

2 Giovanni Dosi is a professor of economics at the Sant'Anna School of Advanced Studies, Pisa.

Sixth, dynamic increasing return of various kinds, both associated with the division of labor and with cumulativity in learning.

And finally, *seventh, the typical disequilibrium condition of all markets*, when they exist, as again, Ken was saying.

These are all building blocks of evolutionary theory, indeed.

Why didn't evolutionary theory become a dominant paradigm, in general, and why didn't Nate fully commit to the enterprise?

My view is this is because Nate always refused to take up the confrontation with Ken [laughter]. In fact, Ken would have been very, very happy to have this confrontation. Indeed, he was so happy and eager that in several occasions, lacking challengers, he took up the confrontation against himself! [laughter]: some of his finest work is indeed Arrow against Arrow. Consider for example the article that Ken wrote for *Industrial and Corporate Change* in 1996: it is absolutely devastating against General Equilibrium and the equilibrium theories in general. With technical information, you lose all the properties of the standard general equilibrium, and maybe you even lose existence. But very few of us has taken up the lead.

Yes, we have this monumental work of Nelson and Winter. But this should have been the beginning of the story, and, for some of us like me, awaiting even more radical departures from the orthodoxy. However, the Nelson-Winter book flagged, as I see it, almost a bifurcation, with a part of the "Innovation Community," not so ready to follow along.

That meant that part of our community, especially the younger ones, especially those educated in North America, began living in some sort of parallel worlds. I mean, those of you who are relatively young and did microeconomics—where did refine microeconomics? Most likely with Mas-Colell et al., or Varian, or Tirole, or other advanced texts in a similar spirit.

Why didn't we all develop on the intuitions of Nate, of Chris Freeman, of Dick Nelson?

One of the reasons, I suggest, is that few dared "waging war against Ken." Putting it in a more disciplined way, not enough scholars acknowledged (or dared to acknowledge) that the economics of innovation pioneered by Nate, Chris, Dick implied also a radical departure from General Equilibrium, Nash, Principal-Agent models, etc. This is where one bifurcated.

Think of the theory of production, the theory of industrial dynamics, international trade, not to talk about, for God's sake, macroeconomics.

Since the 1990s, my feeling is that there has been some sort of "Counter-Reformation" whereby, in a sense, homeopathic quantities of the contribution of Nate, Dick, Chris and many others have been sucked into the standard paradigm.

There are three major inter-related divides here.

First, in the interpretation of the dynamics of innovation should one start from *the dynamics of knowledge or from the structure of the incentives* the economic agents face?

Second, relatedly, is the assumptions of agents doing max (something) a good enough approximation to what the agents actually do?

And, third, do we get anywhere by assuming that whatever observation from out there in the world must be an equilibrium one?

Consider some examples:

New growth theory. Well, in a sense it should be considered an important theoretical advance because now we have innovation endogenous. At the same time though this becomes endogenous at a very high price, in that we have pushed innovation within an enlarged production function in which also innovation is an act of optimum allocation of resources. So is it progress? Maybe not.

Another example. Since the 1980s, we have seen the emergence of theories of "*equilibrium evolution*," an oxymoron, which is more or less like saying that I'm anorexic [laughter].

More recently, one has also seen equilibrium rationalization of *microheterogeneity*. We have a lot of microdata now, for example, on firms, and they show that they are all different in every possible respect. I would have thought that any reasonable observer should have concluded that the world is not in equilibrium, in any reasonable sense. On the contrary, you've very clever people who come along and say, "Well, you say that because you did not see a cost somewhere, which *must* be there because we *must be* in equilibrium (?). . ." Then with that unobservable, I can tell you a story that heterogeneity can be reduced to an equilibrium observation. Dr. Pangloss in his glory!

Or the efforts to rationalize behaviors as equilibrium ones. I'm sorry that my friend Bob Gibbons is not here. But when Bob writes—not long ago, again, in *Industrial and Corporate Change*—that routines are devices to solve the multiplicity of equilibria associated with the Folk Theorem, this, I think, is the ultimate perversion! In an evolutionary view, routines, heuristics, etc., are the way actually people behave because we live in a world of uncertainty and innovation, in which even the theorist is unable to assess whether equilibria exist at all.

Needless to say, you do not find any of this in the works of Nate. My warm invitation is to go back to their freshness, and forget the Counter-Reformation.

Ove Granstrand³

Okay. First of all, a deep thanks for the organizers of this important event, maybe a milestone paralleling the Venice conference 30 years ago. I'm not a student of Nate, unfortunately. I would have liked very much to be that. But on the other hand, I had the privilege of getting to know him closely in some kind of intellectual camaraderie over the years of many talks and beach walks, (big?) talks, which I really enjoy. Really. So Nate once remarked that his interest in technological change transformed him from being an economist to becoming an economic historian. And that illustrates how technological change not only can transform an economy, but also an economist [laughter]. And in that way, you could say—supposing Nate could be, to a certain extent, representative of his colleagues—that those economists who have not yet been transformed into historians have no interest in technological change, which, in a way, is perhaps what you can observe when it comes to the traditional ignorance of technology change.

This is, in a way, an important illustration of how technology is important for economic history as well as economic history's importance for technological change that is illustrated by Nate's own works. It's also an illustration of one recurrent theme in Nate's work, and that is the discovery of feedbacks or mutual interactions at microtechnological levels and these techno-economic feedbacks are crucial for endogenizing technology into economic theory and modeling, as we all know. And, in that way, economic history and economic historians such as Nate have been tremendously important for economic theory and not only just for the joy of understanding technological phenomena.

On the other hand, it is more difficult to find where economic theory has led economic historians to new discoveries of phenomena that could have been hypothesized on theoretical grounds. Often it's the case in natural science, but perhaps less so in social sciences. So, feedbacks may guide modelers at the same time as they make life more complicated for econometricians trying to assess causality in certain range or other way. And you could also draw a conclusion that it seems practice runs ahead of theory, both when it comes to technological as well as economic domains.

Then, coming to another phenomenon, is Nate's conceptualizing of technological convergence, which had also been conceptualized by people like Erich Jantsch in the 1960s under the heading of technological confluence and later on by Fumio Kodama under the label of technology fusion. So there was some general recognition of this important phenomenon, and Nate was the one who, I think, first recognized it and conceptualized it in important ways, as we have heard about throughout the day today. And, in a way, that foreshadows the important emphasis on combining technologies, and Nate is using those language and words in several instances. So that's another example that we've discovered a phenomenon that Nate has provided us all with. This has led to studies of general purpose technologies, about the technology innovations and technological diversifications as key drivers of economic growth, and to ideas and theories of recombinant growth. So you could, in a way, generalize the phenomena of technological convergence into technological combinations, which then turn out to be important phenomena and important drivers for economic growth.

So, again, economic history, with its virtue of proving existence and perhaps nonuniqueness of phenomena, has led to new theories, and new growth theories in particular. However, these combinatorial processes are highly uncertain and complicate life for forecasters, some of whom, by the way, are historians—feedback again—and you can just think about the combinatorial explosion that is around the corner when it comes to combining all these areas of new technologies in ICTs, in robotics, nanomaterials, biotech, molecular machinery, etc., and this leads to my next and final point, that this is increasing the already high levels of uncertainty of various sorts, probabilistic, or genuine and certain, and Nate's work amply illustrates in the sobering ways how difficult it is to forecast technological and economic changes deriving from technological changes, and his amusing and nice stories about various

3 Ove Granstrand is a professor of industrial management and economics at Chalmers University of Technology.

miscalculations throughout the history of technology and the history of economics are a warning that it is difficult, and it surely will be more and more difficult, to make any predictions about the future impact of new technologies and innovations.

One can ask, on balance, do new technologies solve more problems than they create in the long run? So that leads me to the last and more important question, how important is economic history for our economic future? And I leave it open by that, and we can all contemplate what Nate would have tried to say in answering such a question. Thank you. [applause]

Richard R. Nelson⁴

I want to continue the discussion about the role of knowledge of economic history for understanding how the present economy works. This discussion is important because large parts of our discipline do not appreciate how vital such knowledge is for good economic analysis, or why economic history should be returned to having a strong place in the economics curricula.

I say “returned” because when I was a graduate student economic history shared the stage with economic theory and econometrics as bodies of knowledge and skill that all young economists were expected to master. However, beginning around the mid-1960s a large number of highly reputed economics departments dropped their requirements that graduate students study economic history, and today many of these departments no longer have an economic historian on their faculty. This erosion of support for economic history was part of a broader movement within the discipline to see the goal of the advance of economic science as becoming more like physics.

The following two quotes are widely known and admired by many of today’s best known economists. One was made by Lord Kelvin at the start of the 20th century: “When you can measure what you are speaking about and express it in numbers you know something about it; but when you cannot measure it, cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind.” The other is from Galileo several centuries earlier: “The universe cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written, and it is written in the language of mathematics.”

These propositions are broadly true of the subject matter of physics. But they do not fit the subject matter of any other scientific discipline that I know about.⁵

As a striking and important case in point, biology does not fit the picture. Thus in subfields of biology like anatomy and pathology the subject matter studied is partly characterized by numbers, but most of the description is qualitative. Some mathematical modeling may be used to describe and analyze simplified causal relationships, but most of the causal analysis is in the form of narrative. The same is true of subfields like evolution and ecology. And a reading of journal articles and texts in molecular biology shows the same thing.

It is interesting that Marshall, at the beginning of the 20th century, about the time of the quote from Lord Kelvin, remarked: “The Mecca of the economist lies in economic biology rather than in economic dynamics.” By the latter term he clearly meant, basically, physics. But during the 20th century economics as a discipline did not go in the direction Marshall thought it ought.

In most fields of science while some aspects of the phenomena and causal relationships being studied can be described by numbers and characterized mathematically, a large portion of what the field is trying to learn about cannot be fit neatly into the Kelvin-Galileo mold. Above I mentioned biology. Within the physical sciences geology and meteorology are good examples. Both fields make extensive use of numbers and mathematical (or computer simulation) modeling. But much of knowledge in the field is qualitative. And while many of our brothers and sisters are trying to deny it, the same is true of economics and the social sciences more generally.

Of course this is the hallmark of economic history and history in general. An important reason why graduate students in economics ought to have a good grounding in economic history is that they thereby get an appreciation for the kinds of knowledge and the kinds of research methods and skills that are involved. In my view one cannot be a well-rounded scholar of industrial economics, or labor economics, or international trade and finance, or public sector economics, unless one has this set of appreciations and skills as well as those one learns in theory and econometrics.

⁴ Richard R. Nelson is a professor of economics at Columbia University.

⁵ I develop this argument in detail in Richard R. Nelson, “The Sciences are Different and the Differences Matter,” *Research Policy*, 45:9 (2016), 1692-1701.

There are two aspects of the perspective of historical analysis that I want to highlight. One is that what is going on at any particular time cannot be understood adequately without knowledge of how it got there. The other is that the characterization of what is going on is relatively rich and detailed, and while some features of it may be described in numbers much of the description is qualitative. My argument is that both of these aspects are relevant to virtually any effort to understand economic reality.

Consider the field of industrial economics where for many years my principal graduate teaching was concentrated. If one is studying, say, the computer industry it certainly is useful to construct or obtain numbers that tell how many firms were in the industry, and various parts of it, over the years, along with Herfindahl measures of concentration. It is vitally important to get as good measures as one can of prices and performance and how these have changed. One can learn a lot about the mechanisms driving change by building and analyzing stylized formal models.⁶ But these kinds of descriptions and techniques of analysis only make sense if they can be interpreted in the context of a relatively rich and detailed characterization of the industry and its history.

However, in my conversations with young economists I find that many of them these days, while competent in econometrics and in modeling, have little feel for the broader largely qualitative aspects of the context they are analyzing. Too often they have no inclination to find out the economic history that has shaped the context on which they are focused because they don't see its relevance. Of course this limitation of vision is reflected and supported by many of today's main line journals, which often are inclined to accept articles that are all econometrics or modeling and no context. But my argument above all is that, without knowing the context, it is virtually impossible to make sense of the modeling or the econometrics.

To return to the field of study where Nate did much of his work, clearly both sophisticated modeling and econometric studies have contributed greatly to the advance in our understanding achieved over the past half century regarding how technological advance comes about and its economic sources and impacts. However, for many of us working in this field the kinds of studies Nate did have shaped the core of our understanding, and the way we interpret the modeling and the numbers. This is why we believe so strongly that it is important to get study of economic history back again to the center of our discipline, and the knowledge we have.

Gavin Wright⁷

I was Nate Rosenberg's colleague in the economic history group at Stanford for more than 20 years. In preparing these remarks, I took the opportunity to re-read Nate's writings from the 1960s and early 1970s, before he came to Stanford in 1974. It did not take long to see the sources of Nate's reputation as an original thinker and conceptualizer with respect to the economic history of technological change.

The first thing I re-read was "Technological Change in the Machine Tool Industry," published in the *Journal of Economic History* in 1963. That piece came with a story that Nate loved to tell: although it had been invited by and presented to the Conference on Income and Wealth, the paper was then dropped from the conference volume because it had no numbers! Yet according to historian David Hounshell, the article "remains to this day perhaps the single most influential essay ever written" in the history of technology. What made it so distinctive? Nate described the origins of specialized machine-tools firms as spinoffs from the machine shops associated with the early American textile industry, and he then recounted the extension by these firms of their accumulated machine-making capabilities to a sequence of otherwise distinct products in diverse industries—sewing machines, locomotives, typewriters, bicycles, and eventually the automobile. This identification of technological commonalities across conventional industry lines Nate called "technological convergence," a term quoted several times in these sessions. With the advantage of more than 50 years of hindsight, we can now see that Nate was describing processes now commonly discussed under the heading "General Purpose Technologies." But Nate's historical formulation is better than calling it a GPT (it seems to me), because he was describing not a "technology" with certain attributes, but a *system* of innovation through which progress was channeled in particular directions.

Nate did not see the machine-tools industry as passively responding to demands from a changing sequence of users, but as aggressively promoting and seeking out new markets for their accumulated knowledge and machine-

6 See, for example, Franco Malerba, Richard R. Nelson, Luigi Orsenigo, and Sidney G. Winter, *Innovation and the Evolution of Industries: History Friendly Models* (Cambridge, UK: Cambridge University Press, 2016).

7 Gavin Wright is a professor of American economic history at Stanford University.

making expertise. To a boy with a hammer, the whole world looks like a nail. To an industry with know-how, every other industry might be a potential customer. If you want a different example, although this is not in the article itself, it was the active role of the textile machinery industry in promoting the transfer of new technology from north to south. Many of the southern communities that became mill villages were very unpromising indeed, lacking skills, work experience, knowledge about markets. The textile machine makers stood ready to assist with all of these gaps, including technical expertise, even advancing credit if local capital markets were inadequate.

Over the next decade, Nate developed and extended these insights in several directions. In much of this work, he was contending with the nihilism of theorists who questioned the entire idea of “directed technological change.” If factors of production are paid their marginal products, and inputs are combined in optimal cost-minimizing fashion, why should there be any incentive to economize on one factor rather than another? In response, Nate recounted historical settings—laying out the technical issues with great specificity—in which major players (producers or designers) perceived a “technological imbalance,” whereby breaking through a particular bottleneck would open the door to a wide range of new technologies that could draw upon relatively abundant resources. His broader point was that technological trajectories could be driven by “internal compulsions and pressures”—not the same as saying heavily influenced by “non-economic” motives—but in order to identify these, one really has to venture “inside the black box” of the technological world, rather than creating aggregated empirical proxies such as total-factor-productivity or patent counts and then treating these as though they represent the state of technology itself. Nate has been wonderfully influential, but on that broad intellectual strategy, I fear it is a losing battle because young scholars doing empirical work are so tempted by these readily available indicators.

The Rosenberg perspectives have indeed been influential for research and writing in the history and economic history of technology. Many examples could be cited, but the one I will mention here is my long-term project with Gary Saxonhouse on the technological progress and diffusion in cotton spinning, the first truly global industry. Very much in the spirit of Nate’s account of technological trajectories driven by an unfolding internal logic, we found that the rise of American ring-spinning in the 19th century defined not just a technical alternative to Britain’s mule-based system, but an alternative technological *paradigm*, an option available to emerging industries around the world. Although the term “diffusion” may imply that the newer technology was superior to the old, in reality both ring and mule were technologically progressive, and each had their engineering advocates, divisions tending to fall along lines of national engineering networks, as would not surprise a diligent reader of Rosenberg. This era of dueling paradigms fundamentally changed, however, when Japanese producers in the 1890s figured out how to adapt the American ring technology to their labor-abundant setting, by adopting a new package of complementary components, including importation of longer-staple American cotton and adding a labor-intensive cotton mixing stage adapted to the more stringent material requirements of ring spinning. A Schumpeterian innovation indeed!

Even for the sympathetic audience assembled here, I suppose the question on many people’s minds is to what extent these historical insights from the 19th century carry over to the more technologically-advanced economies of the 20th and 21st centuries. That profound question is one to which Nate devoted much attention in the latter part of his career. I will not attempt to summarize his perspective here, but I will offer the proposition that discussions of the pace and impact of technological change in our own times would be much enriched by greater awareness of and familiarity with Nate’s legacy. When the Society of the History of Technology awarded Nate the Da Vinci medal in 1996, the citation described him as having “almost single-handedly changed the way economists and economic historians think about technology and technological change.” That is beautiful language, but my rueful comment on it was: “Perhaps so, but today’s growth theorists and cliometricians would be well advised to read him in the original.”

As a case in point, consider Robert Gordon’s best-selling *The Rise and Fall of American Growth*. Gordon asserts that the pace of technological change has declined since its heyday in the mid-20th century, and that this alleged fact accounts for the stagnation of American living standards and productivity in recent times. These propositions have generated a division among reviewers, between “pessimists” who agree with Gordon and “optimists” who have faith that technology can rise to new challenges in the future as in the past. But Gordon’s book has no citation to Rosenberg and reflects little appreciation of the perspective that Nate developed over his lifetime, namely that gains in productivity and living standards are not direct reflections of “technological progress,” instead reflecting complex adoption-adaptation interactions between technology-generators and technology users. Nate described many of these contingencies beautifully in his essay on “Uncertainty and Technological Change.” New technologies typically arrive in a primitive stage, taking years before becoming effective and reliable; their *economic* (as opposed to technical) success may require improvements in related technologies and investments in complementary inputs; indeed,

productivity gains from new technologies may call for thorough redesign and reconfiguration of physical arrangements, job descriptions and personnel policies. Virtually all the micro-studies of the U.S. productivity spurt after 1995 found that gains did not come merely from adopting the latest computer hardware, but from using new technology as a vehicle for reconceptualizing and rearranging ways of doing business—almost always well within the high-tech frontier of the times.

To any participants in the optimism-pessimism debate who aspire to a more informative understanding of technology as part of a national and now global innovation system, my advice would be the same: begin by reading Nate Rosenberg in the original.

David J. Teece

Well, we've heard from everybody. What a fantastic panel! I just ask the panelists to keep their notes because this is worthy of publication. Let me ask you all to please acknowledge their wonderful presentations [applause].