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The Production Function and Productivity

In the debate over the New Economy (Fall 2000, pp. 3–74), the participants rely on the hypothesis of a neoclassical aggregate production function with neutral technical change. According to this hypothesis, some past productivity growth can be attributed to movement along an existing production function as firms choose more automated technologies that already exist but have not yet become economic to exploit. All the rest is called “multifactor productivity growth” and attributed to technical change. But what if this hypothesis is incorrect?

An alternative hypothesis is that technical change takes the form of a sequence of more automated methods of production that are quickly adopted because they are profitable. The whole family of information technologies, from zebra scanners at the grocery store to computer-aided manufacturing on the shop floor, arguably fits this description. From this perspective, the apparent production function reflects the past history of such biased, capital-using technical changes. We have called this the “fossil production function” (Foley and Michl, 1999; Michl, 1999) because it comprises old methods of production. A fossil production function denies the prevalence of capital deepening based on pre-existing methods of production. At any given time, firms are already using the most capital-intensive methods yet invented and available commercially.

To illustrate what difference this makes, consider Robert Gordon’s (Fall 2000, pp. 49–74) evidence against the New Economy. His Table 1 shows a sharp increase in the growth of the capital-labor ratio (from 1.27 percent per year over 1972–1995 to 2.62 percent per year over 1995–1999). He interprets this as movement along a pre-existing production function, which diminishes the case for a New Economy driven by multifactor productivity growth. But if technical change is capital using, this acceleration in capital intensity reflects technological dynamism, and it does not make sense to mark down the overall impact of technical change because of it. The acceleration in cyclically adjusted labor productivity shown in line 8 of his Table 2 would then have to be reinterpreted as evidence for the New Economy. Of course, Gordon makes other useful points, which do not depend on the neoclassical hypothesis, such as his observation that technical dynamism seems restricted to a few industrial sectors. We do not wish to endorse either side of this debate, only to point out their reliance on the neoclassical hypothesis by using his contribution for purpose of illustration.

There may be a tendency among students new to this area to think that because the Bureau of Labor Statistics publishes data on multifactor productivity, this accounting device is as unproblematic as, say, value-added accounting for national income, when in fact it is heavily dependent upon the hypothesis of an aggregate production function with specific neoclassical features.

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Response from Robert J. Gordon

For decades, economists have debated the relative importance of capital accumulation and technical change as sources of economic growth. Not only do I agree with the main point in the letter by Duncan Foley and Thomas Michl, but I feel that it understates the importance of technical change as a source of capital deepening. Indeed, I would argue that in the long run all capital deepening is caused by technical change rather than being an independent source of growth. However, this is not a new point that can be attributed to the two 1999 citations in their letter, but rather an old point that goes back at least three decades to the work of Thomas K. Rymer (1971). Earlier and independently, I had made the same point in an unpublished working paper (Gordon, 1968).

A simple example demonstrates the deep truth that capital deepening—that is, the growth in the capital-labor ratio—must be directly attributable to technological change. If in the year 1770 all capital equipment consisted of vintage 1770 Watt-Bolton steam engines, and if technical change were all disembodied (that is, figuring out how to rearrange the Watt-Bolton steam engines to boost production), then capital accumulation would have ground to a halt within a few decades of 1770, exhausted by diminishing returns. The entire contribution of capital deepening to labor productivity growth since 1770 is attributable to trillions of dollars of investment in railroads, autos, trucks, airplanes, electrical machinery, computers, and much else that was invented and further developed after 1770 and would not have occurred without technical change taking place after 1770.

This point applies only to capital deepening, not to all capital accumulation. Technical change is not necessary for growth in the capital stock that keeps pace with growth in labor input, maintaining a fixed capital-labor ratio, with investment consisting entirely of equipping the additional members of the population with additional machines and structures of a given technology. But because all capital deepening ultimately requires technical change, existing measures of multifactor productivity (MFP) growth cannot be interpreted as measuring the pace of technical progress, since the capital-deepening effect (due also to technical change) is subtracted out in calculating MFP growth. Further, the official United States measures of MFP growth follow the procedure long advocated by Dale Jorgenson, which is to subtract out a further component called the capital-composition effect, representing shorter-lived capital having a higher marginal product than long-lived capital. However, this practice further buries the impact of technical change, since the movement from long-lived structures to shorter-lived equipment to very short-lived computers reflects much faster rates of technical progress in computers than in structures.

Why, then, keep track of investment and capital at all? Technical change is necessary for capital accumulation, but is not sufficient. There must also be saving to finance the investment. Because technology-enabled capital investment requires saving, the national accounts are justified in separating out investment, and the growth literature is justified in separating out the part of labor productivity growth attributable to technology-enabled capital investment.

The Foley-Michl point can be applied to a further understanding of the productivity revival of the late 1990s in the United States. All calculations show that MFP growth revived, indicating an acceleration of technical change, but this understates the role of technical change, which together with an abundant supply of capital directly created the investment boom and hence the capital-deepening effect of the late 1990s. If the post-1995 acceleration in the rate of technical change were to disappear, this would erode the foundations of the investment boom, thus eliminating not just the revival of MFP growth, but also the contribution of capital deepening.

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Response from Stephen E. Oliner and Daniel E. Sichel

Duncan Foley and Thomas Michl highlight an important point about the sources of economic growth. As they note, capital deepening and technological progress—while often regarded as independent sources of growth—are, in fact, closely connected: Firms adopt more capital-intensive methods of production in response to technological advances that reduce the relative price of capital services. The rapid growth in the stock of information technology capital—especially computing equipment—clearly illustrates this point. Firms have invested heavily in this equipment because new models provide greater power at ever lower constant-quality prices. These price declines have occurred largely because scientists and engineers have found ways to pack more and more circuitry on each square inch of silicon. At root, the growth contribution from information technology capital deepening has been driven by efficiency gains in the design and production of these technologies. Indeed, as Robert Solow (1970) noted long ago, the growth models that underlie growth accounting exercises such as ours have the property that steady-state growth in labor productivity equals the rate of technological progress.

We agree, as well, with a second key point in Foley and Michl’s letter—that movements in both multifactor productivity (MFP) and capital accumulation are relevant for judging whether a “New Economy” has emerged. Even if the growth of MFP remained constant across a broad swath of industries, rapid growth in the stock of information technology capital could be taken as support for a “New Economy” view. This is so because the robust accumulation of information technology capital is being spurred by technological advances in the part of the economy that produces this capital.

Our only disagreement with Foley and Michl concerns the rather negative tone of their comments about the usefulness of growth accounting. Their letter, in effect, restates the well-known critique that growth accounting decompositions can identify only the proximate sources of growth, not the ultimate sources. We also mentioned this limitation in our paper. However, even the limited objective of pinning down the proximate sources of growth can answer many interesting and important questions, particularly during periods when the economy is not in steady state. To take a couple of examples from our paper, the growth accounting framework allowed us to estimate the contribution from the boom in information technology capital spending during 1995–1999 to the observed acceleration in labor productivity. It also enabled us to estimate how much of the rise in MFP growth during this period was centered in the computer- and semiconductor-producing part of the economy. Answers to these questions are important for understanding the mechanisms through which economic and technological developments have affected growth and for assessing the prospects for growth in the future.

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References


Sociology and Social Interactions

Charles Manski (Summer 2000, pp. 115–36) makes a compelling argument for sustained empirical research on social interactions, and the Journal of Economic Perspectives is to be applauded for bringing this topic to the attention of the profession. However, Manski makes an assertion that is all too common in economics. In discussing the contributions of another discipline, in this case sociology, Manski dismisses them outright as incoherent and lacking formal rigor. Yet, it is clear that Manski either did not dig too deeply into the relevant literature or simply chose to ignore it.

Manski’s position is that economics needs a better understanding of social interactions but that there are two primary obstacles to achieving this understanding. First, he argues that current research on social interactions lacks any significant rigor largely because researchers borrow concepts that are poorly defined from other disciplines. Second, and more fundamentally, he cites the problem of drawing inferences from the data, which he states has relied on ambiguous interpre-
tations. Manski’s suggested solution is to conduct surveys on the subjective expectations of individuals and to use them in combination with revealed-preference data and experimental data. Manski cautiously offers this approach as a potential solution to the problems involved with the inference of preferences and expectations utilized in typical econometric analysis.

My critique of Manski’s approach is twofold. First, he relies on an underdeveloped reading of the literature in sociology, not to mention social psychology and psychology as well. All three have quite coherent research programs focused on social interactions. With regard to sociology, Manski (p. 121) discards the manifold contributions of sociology because of “the sheer number of concepts that sociologists employ” and the “dearth of formal analysis.” This attitude is widespread among economists, and it warrants more serious consideration than Manski’s rather simple discussion of network analysis and the concept of social capital. For instance, Manski might have found an interesting complement for his work with social-exchange theory, which shares the fundamental insight of microeconomics—choice as a calculus of rewards and costs—and has over 20 years of sustained empirical research to its credit (for example, Cook and Whitmeyer, 1992). More glaringly, Manski overlooks the entire research program on expectation states, which examines individuals’ expectations to understand and to explain better group processes (for an introduction, see Knottnerus, 1994).

Second, Manski’s advocacy of survey data as a tool to understand social interactions better is problematic. While most economists might be critical of Manski’s use of surveys, this aspect of his work would not cause many sociologists to blink an eye. Rather, a sociologist would object to the notion that these data can reliably provide a better understanding of social interactions (Collins, 2000). While subjective expectations tell us what individuals are thinking about the future, they do not tell us how that individual would interact with other individuals. Indeed, subjective expectations are social in nature, developed through a constant feedback process where expectations are mediated in social interactions. As with typical statistical and survey data, the aggregate of Manski’s subjective expectations does not tell us much about the choices and behavior of individuals in a specific interaction setting. Even when combined with revealed-preference data and experimental data, as Manski advocates, the result is more information about individual choice but not necessarily how those choices are negotiated by interacting individuals.

This latter critique gets to the heart of the differences between economics and sociology, and why it is important to understand and respect their respective approaches and subject matter. While both microsociology and microeconomics are focused on individual choice, the foundation of microeconomics rests on the assumption that individuals make choices that advance their well-being or satisfaction. In contrast, microsociology, and sociology in general, argues that variation in individual choices and behavior can be traced to specific situational contexts (for a lucid introduction to the topic of microsociology, see Collins, 1981). In other words, microeconomics seeks to apply a very limited notion of individual choice to understand all of human behavior, while microsociology stresses the variability of choice and behavior across a wide variety of social contexts.

Ultimately, Manski does a disservice to economists who are genuinely interested in studying social interactions because he does not give serious consideration to the contributions of sociology. Clearly, sociology is a discipline that is marked by a great diversity of theories, approaches and methods (and quality, I might honestly add), and it can be daunting and frustrating at times to wade through the literature. But for those who are truly interested in going beyond the narrow confines of mainstream economic theory, it is well worth the effort.

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References


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