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Chapter Title: Hedonic Price Indexes and the Measurement of Capital and Productivity: Some Historical Reflections

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6 Hedonic Price Indexes and the Measurement of Capital and Productivity: Some Historical Reflections

Zvi Griliches

6.1 Introduction: History

More than thirty years have passed since I stumbled onto the topic of "hedonic" price indexes. More than twenty years have passed since Dale Jorgenson and I pointed to "quality change" problems as a major potential "explanation" of productivity growth as it was then being measured. It may be opportune, therefore, on this festive occasion to reminisce a bit about from where and how far we have come and also how much still remains to be done in this, as in many other, areas of empirical research.

Before I get very far, however, I should first enter a disclaimer. There was nothing particularly original about my first hedonic price indexes paper (Griliches 1961). The notion that one might use regression techniques to relate the prices of different "models" or versions of a commodity to differences in their characteristics, "qualities," and discover thereby the relative valuation of such qualities is reasonably obvious and has been rediscovered a number of times by many people. The earliest references I know of today come primarily from agricultural economics: Fred Waugh's Columbia thesis on vegetable prices (Waugh 1928, 1929) and Vail's (1932) work on mixed fertilizer prices. At the time, in the late 1950s, when I went looking for references to buttress my own regressions, I was pointed first to Stone's (1956) analysis of liquor prices and Court's (1939) explicit use of the hedonic label for his automobile price regressions. At the theoretical level the issues had been discussed by Hofsten (1952), Houthakker (1951–52), Adelman (1960), and others. It was clear to me then, and I think it is also clearly stated in the 1961 paper, that the idea

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The author is indebted to the National Science Foundation for its support of this work over the last thirty years. A significant portion of this paper has been adopted from the Introduction to the collection of my early papers (Griliches 1988b).

itself was not particularly original. What was impressive about that paper is that it took the idea seriously, did a lot of work with it, and showed that something interesting can indeed be accomplished this way. Showing that something interesting is actually doable had a significant impact on the subsequent literature, generating much new work in this style and also quite a bit of theoretical controversy and elaboration. But I am running ahead of my story.

There were two influences, two lines of research that led me to work on this problem. In my thesis on hybrid corn (Griliches 1956, 1957b) I had studied the diffusion of an innovation as it was affected by various economic forces. Central to that work was the concept of a diffusion curve or path, I had used the logistic for this purpose, in which "time" is essentially exogenous (as it was also to be in the concurrent and subsequent theories of technological change). The model specified an adjustment path to the new equilibrium, but the equilibrium level itself, the "ceiling" level for the new technology, was fixed and unchanging over time (though I allowed it to differ cross-sectionally). I was not entirely happy with such a formulation and had already explored in an appendix to my thesis an alternative model that made the rate of adoption a direct function of profitability with improvements in the "quality" of the technology (rising relative yields of hybrid vs. open pollinated corn) and the fall in its price as its major driving forces. The arrival of partial-adjustment distributed-lag models at Chicago via Cagan (1956) and Theil (who had brought Koyck's 1954 model to Nerlove's and my attention) led me to try them as an alternative framework for the analysis of technical change in my work on the demand for fertilizer in agriculture (Griliches 1958a). That work interpreted the growth in fertilizer use as a lagged response to the continued decline in its real price. For that I needed, however, a reasonable price series, and I was not satisfied with the official USDA price index on this topic. The "quality" of the fertilizers used was changing rapidly, the use of nitrogen was increasing relative to the other components, and the official price series were not capturing it adequately. An alternative was available to me in the form of a series of "total plant nutrients used" and an estimate of the average price per plant nutrient unit could be derived from it and the total fertilizer expenditure series. But that series gave equal weight to each of the three major plant nutrients (nitrogen N, phosphoric acid P, and potash K), which looked wrong to me. It was then that I ran my first hedonic regression, though I did not know its name at that point [in 1957], relating the prices of different mixed fertilizers to their "formula" (the mix of their ingredients) to derive better weights for the construction of a total "constant quality" fertilizer quantity and price series. This regression, which yielded 3.5, 2, and 1 as the approximate "correct" weights for the three major plant nutrients (N, P, and K, respectively) instead of the equal weights implicit in the total plant nutrients concept, is buried in a footnote in the final published version (Griliches 1958a, 599). I had not realized yet what was going to sprout from it in the future.

The other line of work that merged with it was the direct measurement of "technological change" using output over input indexes. This line was based on earlier work in agriculture by Barton and Cooper (1948), was summarized for me by Schultz (1953), and had been pursued at Chicago by Ruttan (1954, 1957), before the topic was transformed by Solow's (1957) elegant reformulation and its subsequent elaboration by Jorgenson and Griliches (1967). Similar work had been done in industry by Schmookler (1952) and Abramovitz (1956), among others. The stylized facts that had emerged were quite clear. The lion's share of the observed growth in output was attributable to "technical change" or, more correctly, to the "residual."

Having come to this problem with a background in econometrics, I had used Schultz's numbers to estimate the social returns to public investments in agricultural research (Griliches 1958b), I found the spectacle of economic models yielding large residuals rather uncomfortable, even when we fudged the issue by renaming them as "technical change" and then claiming credit for their "measurement." My interest in specification analysis (Griliches 1957a) led me to a series of questions about the model used to compute such residuals and also, especially, about the ingredients, the data, used in the model's implementation. This led me to a research program that focused on the various components of such computations and alternatives to them: the measurement of the services of capital equipment items and the issues of deflation, quality change, and the measurement of a relevant depreciation concept; the measurement of labor input and the contribution of education to its changing quality; the role of "left-out" variables (inputs) such as public and private investments in R&D; and formula misspecification issues, especially economies of scale and other sources of disequilibria, which led me to a continued involvement with production function estimation. This program of research, which was announced, implicitly, in "Measuring Inputs in Agriculture" (Griliches 1960) and found its fullest expression in my two papers on agricultural productivity (Griliches 1963, 1964a), served me rather well in subsequent years and to this date. It was in certain aspects rather similar to the task pursued by Denison (1962) at about the same time, except that I put more emphasis on its econometric aspects, on the explicit testing of the various proposed adjustments, and the "sources of growth" attributions.

It was in this context, when I turned to the examination how the various capital measures were being constructed and especially deflated, that I escalated my incipient efforts in agriculture into a more general staff report for the Stigler Committee (NBER 1961), resurrecting thereby the "hedonic regression" approach to the measurement of quality change problem. This paper appeared at a rather opportune moment, just as data, computer resources, econometric training and sophistication, and general interest in this range of topics were all expanding, and a whole literature developed in its wake, influencing the measurement of real estate prices, wage equations, environmental amenities, and other aspects of "qualitative differences." This literature has become too vast for one person to survey it. I tried to do so earlier on in its development in the introduction to the volume of essays on this topic (Griliches 1971). More recent surveys can be found in Triplett (1975, 1987), Berndt (1983), and Bartik and Smith (1987). Here I can only indicate what I consider to be a few of the highlights of this literature.

6.2 Hedonics Revisited

There are three major issues that tend to be addressed, in different proportions, in the hedonic literature. There is a range of theoretical questions: How should different "qualities," characteristics, of commodities (outputs or inputs) be modeled, entered into utility or cost functions, and translated into demand and supply functions and the resulting market outcomes? Can one give a theoretically consistent interpretation to "quality adjusted" price indexes, and can one derive valid restrictions from the theory that the empirical price-characteristics regressions should satisfy? There is also a wide range of empirical problems. What are the salient characteristics of a particular commodity? Under what conditions should one expect their market valuation to remain constant? How should the regression framework be expanded, what variables should be added to it, so as to keep the resulting estimates "stable" in face of changing circumstances? And there is also a whole host of econometric methodology issues associated with the attempt to estimate a relationship that can be thought of as being the result of an interaction of both demand and supply forces, and with the use of detailed microdata, often in the form of an unbalanced panel of data for a fixed number of manufacturers, but a different and changing number of "models" (commodity versions).

The theoretical literature tends to focus either on the demand side (Lancaster 1966, 1971; Muellbauer 1974; and Berndt 1983, among others) or the supply side (see, e.g., Ohta, 1975) with very few (Rosen 1974 being a notable exemption) attempting a full general equilibrium discussion (see also Epple 1987 for a recent discussion). There is much finger pointing at the restrictive assumptions required to establish the "existence" and meaning of hedonic "quality" or price indexes (see, e.g., Muellbauer 1974; and Lucas 1975). While useful, I feel that this literature has misunderstood the original purpose of the hedonic suggestion. It is easy to show that, except for unique circumstances and under very stringent assumptions, it is not possible to devise a perfect price index for *any* commodity classification. With finite amounts of data, different procedures will yield (hopefully not very) different answers, and even "good" formulae, such as Divisia-type indexes, cannot be given a satisfactory theoretical interpretation except in very limiting and unrealistic circumstances. Most of the objections to attempts to construct a price index of automobiles from the consideration of their various attributes apply with the same force to the construction of a motor-vehicles price index out of the prices of cars, trucks, and motorcycles.

My own point of view is that what the hedonic approach tries to do is to estimate aspects of the budget constraint facing consumers, allowing thereby the estimation of "missing" prices when quality changes. It is not in the business of estimating utility or cost functions per se, though it can also be very useful for these purposes (see Cardell 1977; McFadden 1978; and Trajtenberg 1983 for examples.) What is being estimated is actually the locus of intersections of the demand curves of different consumers with varying tastes and the supply functions of different producers with possibly varying technologies of production. One is unlikely, therefore, to be able to recover the underlying utility and cost functions from such data alone, except in very special circumstances. Nor can theoretical derivations at the individual level really provide substantive constraints on the estimation of such "market" relations. (See the detailed discussion of many of these issues, in the context of estimating the value of urban amenities, in Bartik and Smith 1987.) Hence my preference for the "estimation of missing prices" interpretation of this approach. Accepting that, one still faces the usual index number problems and ambiguities but at least one is back to the "previous case." In this my views are close to those articulated by Triplett (1983a, 1986). The following passage from Ohta and Griliches represents them reasonably well:

> Despite the theoretical proofs to the contrary, the Consumer Price Index (CPI) "exists" and is even of some use. It is thus of some value to attempt to improve it even if perfection is unattainable. What the hedonic approach attempted was to provide a tool for estimating "missing" prices, prices of particular bundles not observed in the original or later periods. It did not pretend to dispose of the question of whether various observed differentials are demand or supply determined, how the observed variety of models in the market is generated, and whether the resulting indexes have an unambiguous welfare interpretation. Its goals were modest. It offered the tool of econometrics, with all of its attendant problems, as a help to the solution of the first two issues, the detection of the relevant characteristics of a commodity and the estimation of their marginal market valuation.
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> Because of its focus on price explanation and its purpose of "predicting" the price of unobserved variants of a commodity in particular periods, the hedonic hypothesis can be viewed as asserting the existence of a reduced-form relationship between prices and the various characteristics of the commodity. That relationship need not be "stable" over time, but changes that occur should have some rhyme and reason to them, otherwise one would suspect that the observed results are a fluke and cannot be used in the extrapolation necessary for the derivation of missing prices. . . .
>
> To accomplish even such limited goals, one requires much prior information on the commodity in question (econometrics is not a very good tool when wielded blindly), lots of good data, and a detailed analysis of the robustness of one's conclusions relative to the many possible alternative specifications of the model. (1976, 326)

The theoretical developments have been useful, however, in elucidating under what conditions one might expect the hedonic price functions to be stable or shift and which variables might be important in explaining such shifts across markets and time. My own work in this area has had more of a methodological-empirical flavor to it though there were also nonnegligible attempts to formulate and clarify the theory underlying such measurement techniques in Adelman and Griliches (1961), Griliches (1964b), and in Ohta and Griliches (1976, 1986). The last two papers represent also my efforts to pursue additional empirical work in this area. In the 1976 paper with Ohta we extended the earlier approach to the analysis of used automobile prices and investigated differences between performance and specification characteristics and pricing differences between manufacturers of different makes of automobiles. The 1986 paper focuses on the role of gasoline price changes in shifting the hedonic price relationships for cars, extends the theory to incorporate operating costs components, and shows that allowing for such price changes leaves the "extended" hedonic function effectively unchanged, permitting one to maintain the stability of tastes hypothesis in this market. See also Gordon (1983) and Kahn (1986) for related work.

The major recent "success" of hedonic methods has been their acceptance by the official statistical agencies after many years of resistance. Hedonic methods had been used for a long time by the Bureau of the Census to compute its index of single family houses, and much experimental work was carried on at the Bureau of Labor Statistics, but it was not until January 1986, when the *Survey of Current Business* announced a revision of the U.S. National Income Accounts that incorporated a new price index for computers based on the hedonic methodology, that one could feel that they had received the official imprimatur. This index is described and discussed in Cole et al. (1986) and Triplett (1986); see also Gordon (1989) for alternative computations. It would be interesting to speculate why it has taken so long for these methods to penetrate into the "official" circles. This is not, of course, the first use of such methods by the statistical agencies. The Bureau of the Census has used hedonic methods for years in the construction of its residential housing price indexes and there has been significant experimental work with these methods at the BLS, by Gavett, Early and Sinclair, Triplett, and others. But the recent computer price indexes revision is the first time an agency has embraced these methods publicly in a significant way.

It is easy to forget how vehement the opposition was. One needs to go back to the 1962 and 1965 exchanges between Gilbert, Denison, Jaszi and myself to recapture the flavor of some of these arguments (see Griliches 1962 and 1964b and the associated comments). The objections could be caricatured as either saying that it could not be done, or it should not be done, or it was already being done by the standard conventional methods. The fact that it is difficult to do, that an actual empirical implementation calls for much judgment on the part of the analyst and hence exposes him to the charge of subjectivity, is still the most telling objection today. The fact that the standard procedures also involve much judgment and "ad-hockery" is usually well hidden behind the official facade of the statistical establishment. Hedonic methods are difficult. They require more data and more analysis and judgment. Their virtue is that they use more data and that they expose some of these judgments to the final user of the results, providing an implicit warning of their tenuousness. Here, as everywhere else in economics, there is no free lunch.

The notion that one should stick only to "cost" based quality differentials was preposterous at the time and has been largely given up by its proponents. The difference between "resource" use and "utility" based quality adjustments was first stated by Fisher and Shell (1972) and further clarified by Triplett (1983a) in his debate with Gordon (1983). It is now well understood that both concepts make sense in different circumstances and that both are interesting and useful, especially when they do not coincide.

The notion that the statistical agencies were already doing all this under the guise of "linking" was largely wishful thinking, though matters have improved greatly over the years. The problem was not that a detailed "all models" Divisia index would not come close to a hedonic regression result. It might even be superior to it. It was just that it was not being done, in part because the detailed data were not being collected and new products and new varieties of older products were not showing up in the indexes until it was much too late. The hedonic approach was one way of implementing what they should have been doing in the first place. It was also more willing to carry the "linking" idea further, across models that differed significantly in more than one dimension. It could not solve the really new product problem, that is, the appearance of a product whose uses and dimensions had no precedent or anticipation. But it was willing to push comparisons much further than they had been pushed before, not giving up as easily in the face of a changing world. Buried within the hedonic idea was already the germ of Becker's (1965) "household production function" and the notion that one should look at the relevant activity as a whole, at its "ultimate" product in terms of utility or productivity, and not just at the individual components. In this sense, there remains still much to be done in this area. I do not think that we have actually been daring enough. We have not yet produced a decent price index of "health" nor have we done the simpler task of tracing through the relevant history of the price of computation, from the days of the abacus, through the electric desk calculators of my student days (who remembers still the Marchants, Monroes, and Friedens of yore?) the electronic mainframes of our youth, and the PC revolution of recent years. I think that it is doable and I believe that it is worth doing, whether we use the results to revise the National Income and Product Accounts or not.

6.3 Capital Measurement

The work on hedonic price indexes connected to my more general interest in the measurement of capital for the analysis of productivity change. A rather complete statement of my original position on this matter can be found in the Yehuda Grunfeld Memorial volume paper (Griliches 1963). This was to be refined later in joint work with Jorgenson (Griliches and Jorgenson 1966; Jorgenson and Griliches 1967). The difficulty with the available capital measures then, was, and to a great extent is still now, in my view, the fact that they were being overdeflated and overdepreciated, that items with different expected lives were being added together in a wrong way, and that no allowance was being made for changes in the utilization of such capital. The overdeflation issue was already alluded to in the discussion above; it was fed by the strong suspicion that the various available machinery and durable equipment price indexes did not take quality change into account adequately, if at all. This issue connects also to the "embodied" technical change idea (Solow 1960) and the literature that flowed from it. My view on overdepreciation remains controversial (see Miller 1983). I turned early to the evidence of used machinery markets to point out that the official depreciation numbers were too high, that they were leading to an underestimate of actual capital accumulation in agriculture, but I also argued that the observed depreciation rates in secondhand markets contain a large obsolescence component that is induced by the rising quality of new machines. This depreciation is a valid subtraction from the present value of a machine in current prices but it is not the right concept to be used in the construction of a constant quality notion of the flow of services from the existing capital stock in "constant prices." The fact that new machines are better does not imply that the "real" flow of services available from the old machines has declined, either potentially or actually. The point is illustrated visually in figure 6.1, taken from the original 1963 paper that plots the information on different performance concepts for farm tractors as a function of their age. These data and my subsequent attempts to explore some of these issues econometrically (see esp. Pakes and Griliches 1984) all throw doubt on the current practice of assuming that the services of physical capital deteriorate at a rapid and fixed rate, independent of their age. But the available data on types of machinery in place and their actual age structure have been rather sparse, and there has been less progress in this direction than I think is desirable or perhaps even possible.

6.4 Explanation of Technological Change

Several strands of this work came together in "The Explanation of Productivity Change" (Jorgenson and Griliches 1967) in an attempt at a more complete accounting of the sources of economic growth. Given its twentieth anniversary in 1987, it may be worthwhile to review some of the issues raised there.

In 1967 we argued that a "correct" index number framework and the "right" measurement of inputs would reduce greatly the role of the residual ("advances in knowledge," total factor productivity, disembodied technical

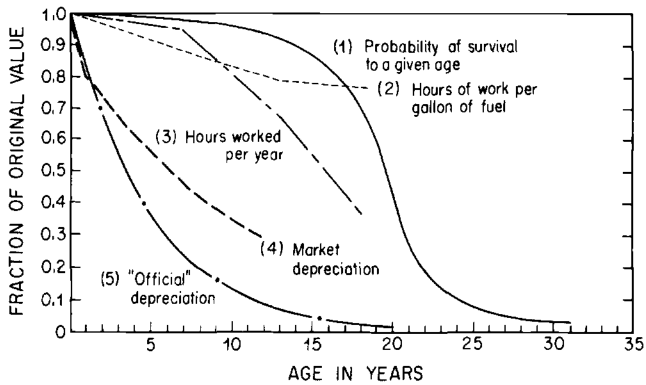

Fig. 6.1 The aging of tractors

change, and/or other such terms) in accounting for the observed growth in output. It brought together Jorgenson's work on Divisia indexes, on the correct measurement of cost of capital, and on the right aggregation procedures for it, with my own earlier work on the measurement of capital prices and quality change and the contribution of education to productivity growth (Griliches 1960, 1963, 1964a). It produced the startling conclusion, already foreshadowed in my agricultural productivity papers, that an adjustment of conventional inputs for measurement and aggregation error may eliminate much of the mystery that was associated with the original findings of large unexplained components in the growth of national and sectoral outputs. It did this with a "Look Ma! No hands!" attitude, using neither additional outside variables, such as R&D, or allowing for economies of scale or other disequilibria. This did indeed attract attention and also criticism. The most penetrating criticism came from Denison (1969) which led to an exchange between us in the May 1972 issue of the *Survey of Current Business*.

Denison found a number of minor errors and one major one in our computations. By trying to adjust for changing utilization rates we used data on energy consumption of electric motors in manufacturing, a direct measure of capital equipment utilization in manufacturing (borrowed to a large extent from Foss 1963), but extrapolated it also to nonequipment components of capital in manufacturing and to all capital outside of manufacturing, including residential structures. There was also the uneasy issue of integrating a utilization adjustment within what was otherwise a pure equilibrium story. Once we conceded most of the utilization adjustment, our "explanation" of productivity growth shrank from 94% to 43% and with it also our claim to "do it all" (without mirrors).

I still believe, however, that we were right in our basic idea that productivity growth should be "explained" rather than just measured and that errors of measurement and concept have a major role in this. But we did not go far enough in that direction. We offered improved index number formulae, a better reweighting of capital input components, a major adjustment of the employment data for improvements in the quality of labor, revisions in investment price indexes, and estimates of changes in capital utilization. The potential orders of magnitude of the adjustments based on the first two contributions, index number formulae and the reweighting of capital components, are not large enough to account for a major part of the observed residual. The labor quality adjustment was not really controversial, but the capital price indexes and utilization adjustments deserve a bit more discussion. We argued for the idea that technical change could be thought of, in a sense, as being "embodied" in factor inputs, in new machines, and human capital, and that a better measurement of these inputs via the nontautological route of hedonic index numbers for both capital and labor could account for most of what was being interpreted as a residual. It became clear, however, that without extending our framework further to allow for R&D and other externalities, increasing returns to scale and other disequilibria, we were unlikely to approach a full "explanation" of productivity change (see the last paragraphs of Jorgenson and Griliches 1972).

It may appear that adjusting a particular input for mismeasured quality change would not have much of an effect on productivity growth measurement since one would need also to adjust the output figures for the corresponding industry. But as long as the share of this industry in final output is less than the elasticity of output with respect to this input, the two adjustments will not cancel themselves out. Since the share of investment in output is significantly lower than reasonable estimates of the share of capital in total factor costs, adjusting capital for mismeasurement of its prices does lead to a net reduction in the computed residual. Empirically it is clear that even without considering any of the potential externalities associated with new capital, there are enough questions about the official price indexes in these areas to make further work on this topic a high priority.

The utilization adjustment fit uneasily within the rather strict competitive equilibrium framework of Jorgenson and Griliches (1967). The analogy was made to labor hours, calling for the parallel concept of machine hours as the relevant notion of capital services. We had also in mind the model of a continuous process plant where output is more or less proportional to hours of operation. Since we were interested primarily in "productivity" change as a measure of "technical" change, a change that is due to changes in techniques of production, fluctuations in "utilization," whether a plant worked one shift or two, 10 months or 12, were not really relevant for this purpose. But while labor unemployment was happening offstage as far as business productivity accounts were concerned, capital "underemployment" was difficult to reconcile with the maximizing behavior with perfect foresight implicit in our framework.

There are two somewhat separate "utilization" issues. Productivity as measured is strongly procyclical. Measured inputs, especially capital and labor services, fluctuate less than reported output. The resulting fluctuations in "productivity" do not make sense if we want to interpret them as a measure of the growth in the level of technology or the state of economically valuable knowledge of an economy. The U.S. economy did not "forget" 4% of its technology between 1974 and 1975. Nor was there a similar deterioration in the skill of its labor force. (National welfare did go down as the result of OPEC-induced worldwide rise in energy prices, but that is a separate story.)

What is wrong with the productivity numbers in this case is that we do not measure accurately the actual amounts of labor or machine hours used rather than just paid for. Since both capital and labor are bought or hired in anticipation of a certain level of activity and on long-term contracts, actual factor payments do not reflect their respective marginal products except in the case of perfect foresight and only in the long run. Underutilization of factors of production is the result of unanticipated shifts in demand and various rigidities built into the economic system due to longer term explicit and implicit contracts (and other market imperfections) between worker and employer and seller and buyer. If our interest is primarily on the "technological" interpretation of productivity measures, we must either ignore such shorter run fluctuations or somehow adjust for them. This was the rationale behind our original use of energy consumed by electric motors (per installed horsepower) as a utilization adjustment.

We used energy consumption as a proxy for the unobserved variation in machine hours and not on its own behalf as an important intermediate input. Used in the latter fashion it is a produced input which would cancel out at the aggregate level (as was pointed out in 1969 by Denison in his comment on our paper). Alternatively, one could adjust the weight (share) of capital services in one's total input index to reflect the fact that underutilization of the existing stock of resources should reduce significantly the shadow price of using them. (This is the approach suggested in Berndt and Fuss 1986). Unfortunately, it is difficult to use the observed factor returns for these purposes, both because prices do not fall rapidly enough in the face of unanticipated demand shocks and because of a variety of longer run contractual factor payments arrangement that break the link between factor rewards and their current productivity.

This reflects, in a sense, the failure of the assumption of perfect competition that is the basis for much of the standard productivity accounts. The actual world we live in is full of short-run rigidities, transaction costs, immalleable capital, and immobile resources, resulting in the pervasive presence of quasi rents and short-term capital gains and losses. While I do not believe that such discrepancies from "perfect" competition actually imply the presence of a significant market power in most industries (as argued, for example, by Hall 1986), they do make productivity accounting even more difficult.

The other aspect of utilization is the longer run trend in shift work, the length of the workweek, and changes in hours of operation per day by plants, stores, and service establishments. Consider, for example, a decline in overtime or night-shift premia due, say, to a decline in union power. This would reduce the price of a certain type of capital service and expand its use. If capital is not measured in machine hours, we would show a rise in productivity even though there has been no "technological" change in methods of production. I would prefer not to include such changes in the definition of productivity since I interpret them as movements along (or toward) a stable production possibilities frontier. But there did occur an organizational change that allowed us to get more "flow," more hours per day or year, from a given stock of equipment or other resources. One way to look at this is to think of two types of activities: output production that rents machine and labor hours and the supply of capital services (and also effective labor hours) from the existing resource levels. A decline in overtime premia would be similar to a decline in the tariff on a certain kind of imported input. It would lead to an improvement in "efficiency" but not necessarily to a "technical" change.

It is still my belief that we need to adjust our data for such capacity utilization fluctuations for a better understanding of "technical" change, the issue that brought us to the analysis of such data in the first place. A consistent framework for such an adjustment will require, however, the introduction of adjustment costs and ex post errors into the productivity measurement framework. (See Morrison 1985 and the literature cited therein for recent developments in this area.) It is not clear, however, whether one can separate longer run developments in the utilization of capital from changes in technology and the organization of society. Much of capital is employed outside continuous process manufacturing and there the connection between its utilization and productivity is much looser. The rising cost of human time and the desire for variety and flexibility have led to much investment in what might be called "standby" capacity with rather low utilization rates. The hi-fi system in my home is operating only at a fraction of its potential capacity. Much inventory is held in many businesses to economize on other aspects of labor activity. Nor is it clear that an extension of store hours with a resulting decline in productivity per square-foot-hour of store space is necessarily a bad thing. Thus it is difficult to see how one could separate long-run trends in utilization from changes in production and consumption technologies. It is, however, a topic worth studying and a potentially important contributor to "explanations" of apparent swings in the statistics on measured productivity.

Whether we include or exclude such changes from our "productivity" concept will affect our ability to "account" for them. But that is not the important issue. We do want to measure them, because we do want to understand what happened, to "explain" productivity. The rest is semantics.

Many of these problems arise because we do not disaggregate adequately and do not describe the production process in adequate detail. A model that would distinguish between the use of capital and labor at different times of the day and year and does not assume that their shadow prices are constant between different "hours" or over time would be capable of handling these kind of shifts. We do not have the data to implement such a program, but it underscores the message of our original paper: much of what passes for productivity change in conventional data is the result of aggregation errors, the wrong measurement of input quantities, and the use of wrong weights to combine them into "total factor input" indexes.

Something more should be said about the rather vague notions of "explanation" and "accounting." National Income and Product accounts and associated index numbers are economic constructs, based on an implicit model of the economy and a variety of more or less persuasive logical and empirical arguments. They are not well adapted to "hypothesis testing" or debates about causality. In proposing a better measure of, say, labor, we rely on the evidence of market wage differentials. By bringing in more evidence on this topic we are not just reducing the "residual" tautologically. But the fact that it goes down as the result of such an adjustment does not make it right either. A different kind of evidence is required to provide a more persuasive justification for such adjustments. That is why I turned early on to the use of production functions for econometric testing. Without moving in such a direction one tends to run into various paradoxes. For example, capital growth accelerated in the 1970s in many industries without a comparable increase in the growth of output. In the index-number sense of growth accounting, capital "explained" a larger fraction of the growth of output, and we did, indeed, have a smaller residual. But in spite of this "accounting" the mystery only deepened.

The "econometric" approach to growth accounting involves one in the estimation of production functions. This allows one to test or validate a particular way of measuring an input or adjusting it for quality change; to estimate and test the role of left-out public good inputs such as R&D and other externality generating activities; to estimate economies of scale; and to check on the possibility of disequilibria and estimate the deviation of "true" output elasticities from their respective factor shares. Production function estimation raises many problems of its own, including issues of aggregation and errors of measurement and simultaneity, but it is one of the few ways available to us for checking the validity of the suggested attributions of productivity growth to its various "sources."

My work on agricultural productivity (Griliches 1963, 1964a), which used production function estimation as its main organizing device, left me with the conviction that education, investment in research, and economies of scale (both at the level of the firm and at the level of the market) were the important sources of productivity growth in the long run. Since in the paper with Jorgenson we had not allowed for the two latter sources of growth, I was not too surprised or disheartened when it turned out that we could not really explain all of aggregate productivity change by formula and labor- and capital-quality adjustments alone. It was clear, however, that one would need more and better data to make such additional adjustments more reliable and convincing. I turned, therefore, to trying to amass more data and more evidence on these topics, especially the measurement of the contribution of education (Griliches 1977) and the role of R&D (Griliches 1980, 1986).

Even though we now have more data, more advanced econometric technology, and better computer resources, the overall state of this field has not advanced all that much in the last 20 years. We are really not much closer to an "explanation" of the observed changes in the various productivity indexes. A tremendous effort was launched by Jorgenson and his co-workers (Christensen, Fraumeni, Gollop, Nishimizu, and others) to improve and systematize the relevant data sources, to produce and analyze a consistent set of industry-level total factor productivity accounts, to extend and generalize our original labor-quality adjustments, and to extend all of this also to international comparisons of productivity. In the process, however, rather than pursuing the possibly hopeless quest for a complete "explanation" of productivity growth, they chose to focus instead on developing more precise and detailed productivity measures at various levels of aggregation and devising statistical models for their analysis. Denison (1974, 1979), in parallel, was pursuing his quest for a more complete accounting of the sources of growth, putting together as many reasonable scraps of information as were available, but not embedding them in a clear theoretical framework or an econometrically testable setting. The incompleteness of both approaches and the unsatisfactory state of this field as a whole was revealed by the sharp and prolonged slowdown in the growth of measured productivity, which began in the mid-seventies. Despite the best attempts of these and other researchers, it has not been possible to account for this slowdown within the standard growth accounting framework without concluding that the "residual" had changed, that the underlying total factor productivity growth rate fell sometime in the late 1960s or early 1970s (see Denison 1984; Griliches 1980a and 1988a; Kendrick 1983; and many others).

I do not believe, however, that this slowdown can be interpreted to imply that the underlying rate of technical change has slowed down, that we have exhausted our technological frontiers. In my opinion, it was caused by misguided macro policies induced by the oil price shocks and the subsequent inflation and the fears thereof. Without allowing for errors in capital accumulation (which continued initially at a rather high rate, in spite of the sharp declines in aggregate demand) and widespread underutilization of capacity, it is not possible to interpret the conventional productivity statistics. Surely "knowledge" did not retreat. Moreover, I do not believe that one can use statistics from such periods to infer anything about longer term technological trends. If we are not close to our production possibilities frontier, we cannot tell what is happening to it and whether the underlying growth rate of an economy's "potential" has slowed down or not. We need a better articulated theoretical framework, one that would allow for long-term factor substitution and short-term rigidities and errors, before we shall be able to understand better what has happened to us recently. We also need better data, especially on output and input prices and various aspects of labor and capital utilization.

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Comment Robert E. Lipsey

These historical reflections on hedonic price indexes grossly understate Zvi Griliches' contribution. As he mentions, his contribution was not original. However, even though there had been some earlier instances, the fact is that the idea was totally dead before he revived it, although it offered some hope

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to problems that appeared completely intractable. Although the earliest examples cited go back to 1929 and 1932 and Andrew Court's ingenious paper on automobiles prices was published in 1939, von Hofsten wrote a whole book about *Price Indexes and Quality Changes* in 1952 without mentioning hedonic indexes or Court's work. Richard Stone (1956) did calculate a hedonic price index, referred to Court's study of automobile prices, and endorsed the idea of pricing the characteristics of a product, but his advocacy, perhaps because this was only one of many topics considered in his book, had little impact. I recall that when I studied the literature on price indexes in the early 1950s before beginning my work on export and import price indexes, I was not pointed toward Court's work in any of my reading. When I did come across it accidentally, I was astonished and fascinated, but I did not think of actually using such an unconventional method. What Griliches did was to be enterprising enough to take this idea seriously, bring the methods and the analysis up to date, and start a whole new branch of research on price and quantity measurement. He was not the inventor, but he certainly was the crucial entrepreneur.

One of the points in Griliches' paper is that the hedonic method gives us a method of estimating "missing" prices: prices that have not been observed. The prices may be missing either because we failed to observe them when the transactions took place, as in the case of a new product that escaped the notice of price collectors in its early stages. Or they may be missing because they were unobservable, as, for example, if no transactions took place.

In our international price studies (Kravis and Lipsey 1971), Irving Kravis and I came across an interesting example of the use of the hedonic method to estimate a missing price on the part of noneconomists who seemed to be unaware of the economic literature. The engineering staff of an aircraft company was faced with the task of estimating the price the company would have to pay for engines, not yet in production and with specifications outside the range of existing engines, for a proposed new aircraft. The method they used was to run a multiple regression relating prices of existing aircraft engines to various characteristics such as thrust, the main influence, and many other characteristics in a number of different equation forms. This calculation was performed in 1962, very shortly after the time Griliches published his first paper on hedonic price indexes. They were speaking hedonics without knowing it. It would be interesting to know if this was a common practice among engineers.

The hedonic method also offers a solution to another problem that has proved extremely difficult in price collection. The producers of price data have been urged for many years to move toward the use of transactions prices in place of list prices, and the Price Statistics Review Committee urged the BLS to try to collect prices from buyers rather than, or in addition to, sellers. However, in most types of complex products such as machinery, that objective conflicts with the standard procedure of pricing the same specification in period after period, because no two purchases are of products with exactly the same combination of specifications or exactly the same terms of sale. The choice is between getting fictitious prices for a consistent specification or collecting actual transaction prices and somehow adjusting them for inconsistent specifications to some consistent basis. The first use of that solution that I know of, and one that did use a hedonic analysis on actual transaction prices, was by Dean and de Podwin (1961).

An alternative method, which was described in an article about the electrical equipment conspiracy cases of the early 1960s (Kuhlman 1967), was to derive the prices of characteristics from the companies' price lists and, given the characteristics of each individual transaction, to calculate the amount "off-book" that it involved. The combination of an index of list prices over time, based on specification pricing from the price lists, with changes in the percentage "off-book," gives an estimate of the movement of transaction prices. The chief drawback of the method, in comparison to hedonic price measurement, is that the relative values of the product characteristics are determined by the seller in his list price formulation, rather than by the consumers in the market. But the method does permit its user to make use of actual transaction prices in a way that is not possible if the usual specification pricing is used.

On another topic, I am skeptical about the usefulness of the estimation of production functions as a way of organizing the study of productivity growth. At the necessary level of aggregation, they are fictions far removed from what I would think of as genuine production functions for very specific products or processes. I can see their value for estimating economies of scale, but I am not at all sure that they are particularly suited to judging the contribution of R&D and other "externality-generating activities," such as health, education, public safety, and so on. It seems to me that the production function technology inevitably tends to emphasize direct inputs over indirect inputs.

There is a broader question that is raised by the use of the production function idea for measuring productivity. That is, whether we want to confine our interest to inputs and outputs that go through the business or "production" sector and ignore those outside it. We could, presumably, increase our output per unit of measured input by forcing low productivity labor and capital out of production, for example, by raising wages and imposing taxes on the use of capital goods. Ideally, perfect quality adjustments for labor and capital inputs would break the illusion of productivity growth, but that does not seem to be a likely outcome.

If we want to know about the efficiency of the society as a whole, rather than that of a narrowly defined "production sector," and that is, surely, at least one of the things we want, we should count the inputs and outputs outside the production sector. We should consider the input of time by consumers in shopping, commuting, banking, and so forth, and the difference, as Griliches mentions, between the value of input according to the time of day or the day of the week. A withdrawal from a bank on a holiday or a weekend must have some different value and different cost to the parties from that of a withdrawal during banking hours. In particular, as we become more interested in the output and productivity of the service sector, and if we are skeptical about the official measures, as I am, we will be compelled to think more seriously about the meaning of output and input in service industries and about the relationships between service industry inputs and outputs and inputs and outputs in the home. I suspect that there is more to be learned about the mysteries in recent productivity developments along these lines than in pursuing that picture of the continuous process plant producing a single output from labor and capital inputs.

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