11 Panel Discussion: Implications of BEA's Treatment of Computer Prices and Productivity Measurement

Chair:	Frank de Leeuw	
Statements:	Edward F. Denison	Charles R. Hulten
	Zvi Griliches	Thomas K. Rymes
Comments:	Arthur J. Alexander	René Durand
	Edwin R. Dean	Michael Harper

The panel discussion considered implications of BEA's treatment of computer prices for productivity measurement. Following prepared statements, panelists were given the opportunity to comment on each other's statements. This was followed by comments by members of the audience (most of which appear here).

Discussion

Edward F. Denison

The computer price index of the Bureau of Economic Analysis (BEA) invites attention because it behaves so differently from prices in general. From 1973 to 1988, the implicit deflator for computers fell 91 percent, while that for nonresidential business GNP as a whole rose 138 percent. Based on this comparison, the real price of computers was 4 percent as high in 1988 as in 1973, and it fell more than 19 percent a year. The percentage decline continued to be nearly as large from 1982 to 1988, 18.3 percent a year, as it had been from 1973 to 1982, when it was 20.0 percent.

Use of this index has greatly affected many important economic magnitudes that are measured in constant prices. These include the growth rate and the composition of GNP, the growth rate and the composition of investment and the capital stock, and—of great interest to me—growth rates of productivity in the business sector, in durable goods manufacturing, and in the production of computers. Unit labor costs, often compared with those in other countries, are also much affected. If one starts with data for the rest of the economy and

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then adds computer production, one finds that inclusion of computers greatly improves the record of recent economic performance, both absolutely and in comparison with the past, in all these respects and also with respect to price stability. It has created an incipient recovery in productivity growth.

The sharp drop in the computer price index does not affect GNP in current prices. It does make the current-price series for economic depreciation grow more slowly and, in consequence, current price series for net national product, national income, and corporate profits on a national income basis grow more rapidly.

The special problem in deflating computers is that models change frequently and performance characteristics of successive models are very different. The problem is not confined to computers but is especially pronounced in their case. I take it to be obvious that the deflation of computers should follow rules applicable to other goods of their class.

My own particular interest is in analysis of the sources of past economic growth and alternative ways of changing the future growth rate (Denison 1989). One source of growth is saving, that is, the increase in the capital stock used in production. A second source is advances in technological, managerial, and organizational knowledge of how to produce at low cost. In two-way breakdowns of growth, the former is included in the contribution of total factor input and the latter in output per unit of input.

Such a division of growth is obtained, however, only if net saving or investment, including investment in computers, is measured by consumption forgone and capital is measured as the sum of past net saving.[1] In constant prices, this result is obtained by deflating investment by prices of consumer goods.[2] This procedure, of course, is not followed by BEA in compiling the national accounts but has gained increasing support, including mine. The need for a consumption forgone measure is my main point, but I shall leave it there.

BEA's procedure is to deflate components of investment by their own prices. The method of handling changes in products, both for capital goods and for consumer goods, is to equate the quantities of product or capital that different goods represent by their costs (or prices) at a common date. If one product costs twice as much as another, it is twice as much product. One effect of this procedure, as Tom Rymes taught me about twenty-five years ago, is that advances in knowledge that take the form of improvements in capital goods end up as contributions made to growth by capital and total factor input while all other advances in knowledge raise output per unit of input. This is inconvenient. But, until the computer price index was introduced, the

1. In any given year, consumption forgone (hence net investment) valued in constant prices equals the quantity of consumer goods that resources devoted to increasing the capital stock would have provided if devoted instead to production of consumer goods by the methods used in the base year.

2. This leaves open the question of deflating the smaller part of computer output that does not become business investment. Perhaps it should be deflated like other consumer goods—if only there were agreement on what that means in the case of computers.

amounts involved were not great. They could be approximated and, if one wished, transferred from capital to advances in knowledge so as to permit a clean division of growth sources.

The method of equating different products by their costs at a common date can be implemented by any of several techniques, the choice depending on available information and the nature of the problem. One method is to link price indexes for overlapping models in order to obtain a price series suitable for deflation. BEA has published, but not incorporated into the national accounts or kept up to date, a price index for computers that is based on this technique. It indicates a drop in the real price of computers that is sharp but much smaller than that indicated by the index that *is* used. The procedure differs from that usually applied when new products appear in that the price link is made as soon as the new model appears instead of at a later date. In general, new products tend to come in at prices above those at which they later settle. Unit costs are high until bugs are eliminated and volume expands. Buyers are those for whom the new model is especially useful. The linked model price index probably drops too much because of premature linking.

I believe that the linked index, updated and if possible adjusted for any bias arising from the date of linking, would implement the cost concept for handling new or altered products better than the index used now. This applies to measurement of both national product and capital stock.

Another theoretically possible method of equating different capital goods is by their relative abilities to contribute to production, as measured by their marginal products at a common date. Some of the acceptance of the computer price index probably stems from the belief that it is such a measure, although it is not and BEA does not make this claim.

The only characteristics of various computer models that are compared in compiling the index are output characteristics, such as memory capacity and speed, that indicate what can be done with the computer. None are requirements for other inputs. If one uses the BEA price index to deflate an index of the value of computers produced, one gets an index of the quantity of output that the computer and all other inputs that are used with computers can produce, not what the computer contributes. Enormous resources besides computers themselves go into producing the output ascribed to computers. They include labor, computer programs, phone services, building, and many other items, including the time of many people in this room. BEA's computer price index is the same whether use of a new model requires more labor and other resources as the old or less. For example, if a new model does twice as much as an old and uses half as much of, say, labor to do it, it will be counted as representing only twice as much capital as the old model, whereas an alternative new model that also does twice as much as the old but uses twice as much labor to do so will also be counted as twice as much capital as the old. Thus, output per unit of labor will be four times as high with the first new computer as with the second, but the two computers would be counted as the same amount of capital. I can think of no standard by which this is reasonable.

While BEA does not equate different models by marginal product because it takes no account of inputs, neither—in my view—does it equate them by cost. What it compares is not, as it should be, the cost of producing two models of computers but instead the cost of producing the output characteristics used in constructing the price index. This is the same as an attempt to equate capital goods by marginal product, which, as just stated, fails because it does not consider inputs.

For two main reasons I would find the increase in the constant price value of computers that results from use of the computer price index hard to accept or interpret even if in some sense it measured marginal products.

First, the demand for computers is a schedule, not a point. Users vary greatly in the value of the contributions that computers make to their output, and the value of various uses to which computers are put by a single user also varies greatly. With the real price of computers only 4 percent as high in 1988 as in 1973 and only 30 percent as high as it had been as recently as 1982, it is certain that computers were put to uses that would have been submarginal in earlier years. When computers are deflated by the BEA index, the quantities purchased and in use explode. If the index measured the cost of computer capability accurately, then I would have no problem with the quantity of computer services corresponding to the uses made of computers in 1973 or 1981. But the much bigger quantity purchased for less important uses is given an equally high unit value. In terms of ability to contribute to the nation's production, this causes 1988 output of computers to be overstated relative to earlier years.

Second, computers and peripheral equipment have many capabilities. Not all capabilities are used by any single owner, and some that are used may be unimportant to him. I surmise that, as computers became more versatile and owners more numerous, the average user cared about only a smaller and smaller proportion of the things a computer can do. If so, a quality adjustment based on a computer's features, as distinguished from features actually *used,* overstates the increase in computer input into production.

Jack Triplett has raised an interesting point that invites comment. Suppose that we are comparing output in two years. Everything produced in either year either is or could be produced in the later year, and a valuation can therefore be placed on it. Now we know that in current price, if certain assumptions are made about equilibrium and proportionality of factor cost and market price, both resource costs and prospective marginal products of various capital goods are proportional to prices and hence to each other. This means that, if the latest year is the base year, the weights for different goods are the same whether indexes are based on cost or on marginal product. If quantity indexes for each type of capital good are also the same, indexes for total investment and capital stock based on resource cost are the same as indexes based on marginal product. This is neat but less helpful than it seems.

First, the use of the most recent year does not solve the problem of comparing present models with those no longer being produced, to which a value in the most recent year must be imputed. Imputation by using estimates of the relative production costs of different models (comparable to the linked model method) and imputation by use of output price characteristics (comparable to the BEA computer price index method) will place different values on the discontinued models. Both are said to represent the cost method, so one must choose between them. And the marginal product method, if it could be implemented, would yield still a different value. As one works backward in time and these discontinued models enter the calculations, output in past years, valued in most-recent-year prices, will be different depending on the method adopted to impute the base year value. I should expect cost-based and marginal product–based series to differ substantially as one goes back in time. Second, there is, in any case, no reason to confine oneself to the use of the most recent year as the base year, especially when the choice of base year affects the outcome in important ways.

So much for the computer price index as such. My conclusion is that, despite the admirable care and ingenuity devoted to its construction, it is neither fish nor fowl. It conforms to no sensible criterion.

However, despite the fabulous growth in productivity in computer production that the index implies, the index would have had only a limited effect on business-sector productivity if it had not interacted with two other characteristics of the government's GNP and productivity measures. One is the use of fixed 1982 price weights to combine quantities of different products in constructing real GNP. Before 1982, computers are underweighted relative to the share of resources they used then. In 1973, the share of output of computers measured in current prices was seven times their weight in 1982 prices. In contrast, after 1982 computers are overweighted. By 1988, their weight was three and one-third times as big in constant as in current prices, and, by 1990, it must be four and a half times as big or more.

The other characteristic is the use of GNP instead of NNP to measure output when the Bureau of Labor Statistics (BLS) calculates productivity change. Because the computer has much more weight in GNP than in NNP, the computer price index has much more effect on it. Elsewhere, I have discussed the quantitative effect of interaction between both these measurement practices and the computer index on series for productivity in business and smaller segments of the economy, and, of course, they have an enormous effect on comparisons by industry or end product.

Zvi Griliches

I want to draw your attention to the paper that was circulated before the workshop by Jack Triplett (1991) because it deals in detail with some of the points that Edward Denison raised. Sometimes I have a feeling of déjà

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vu. Some of this, it seems to me, we have been arguing over for thirty years, and I thought it had gone away. But, apparently, old ideas just keep on reappearing.

I think that there are a number of real points that Edward Denison raises, and some I think are beside the point. One of the points that he raises is that the computer index is moving too much, is making too much of a difference, and is not really comparable to the rest of the system. He is right about this, but he is right, not because the computer index is wrong, but because the way it is used is wrong. It is used wrongly because its weight is kept constant. There is also the additional problem of measuring the growth in real value-added, which comes from the fact that the inputs into the computer industry have not been themselves deflated by the same kind of an index. This produces an extra big growth in value-added there relative to somewhere else. That would not matter so much in the total if these inputs were produced domestically, but, since many of these components are imported, we are attributing the growth in foreign productivity to domestic productivity.

It is also true that many other commodity prices are badly measured. This fact does not seem to me, however, to be a good argument for also measuring computers badly. Now, his constructive suggestion is to forget about measuring capital prices entirely and go over to the deflation of capital goods by consumer prices. This is one way of solving the problem, but it will not help if we want to know where, in what industry, the productivity growth is occurring. It is a reasonable approach for welfare measurement except that one does not really get away from the real problem. It assumes that we know how to measure consumer prices, but all the same issues are going to arise there just the same: in the measurement of services, in the measurement of the use of personal computers at home, in the measurement of automobiles and videocassette recorders and other consumer equipment. There is no escape; there is no rest for the weary.

Then there is an assorted set of complaints about hedonic indexes per se, and that is where the déjà vu enters in. I really think that these complaints are fundamentally wrong and that Jack Triplett has basically said most of what there is to say. Hedonics are used, primarily, to do what supposedly the consumer price index (CPI) and producer price index (PPI) are trying to do, to figure out what should have been the price of this item last year if you had been able to collect it. How do you adjust for the fact that the chicory in the coffee mixture has changed? There is no conceptual difference between the amount of chicory in your coffee and measuring the size of the disk in your computer. When one looks at computers, their prices differ depending on what the disk capacity is, depending on the amount of memory, and depending on the clock speed. One can buy inputs that will, in fact, change these computers and bring them up to a different clock speed or to a different memory size.

Now, there is a technical issues as to whether the marginal price of memory or the marginal price of a disk should be computed from the add-on prices that one can also get in the market. But that is a detail, and I do not think that this is the substantive problem. Nor do I think that the use of input measures, the characteristics, instead of performance measures, which is what Rosanne Cole was beating me over the head with yesterday, is the real problem.[3] We did not use output measures in our PC price index but rather resource cost measures. If we had used the right measures of performance, we would have indeed gotten better estimates of how the computers really differ and a better estimate of the price index. This is the issue of how one is to get a good hedonic estimate, but that is a question of implementation, not of substance.

In any case, now we do have in the computer area the evidence on matched models. They do not decline by 30 percent per year; they decline by about 20 percent per year. Well, Ed Denison says that this is still too much because the early price declines are somehow not representative. But, as Erwin Diewert would say, that is again an index number problem. It is not a substantive problem. The right way to compute such indexes is to weight these changes, weight them and change the weights as one goes along. If the price declines are occurring during a period when very few of these models are being bought, then they will have very little weight in the total. And that is all there is to it. There is no special mystery about that.

The objections that are being made against adjusting for differences in quality or capacity could have just as well been applied to all other goods in the producer or consumer price indexes. The same objection could be made to pricing automobiles and considering whether better upholstery matters. Some people, after all, do not care about upholstery. However, others do care, and consequently, there is a market price for upholstery.

One of the issues that floats around is really the issue of whether there is an equilibrium. Another issue is the question (one that most of the index number literature rides roughshod over), Is there a representative consumer? One is aggregating over many different consumers with different tastes and constructing just one number. There will be some people who will gain more from a particular change, and there will be some people who will gain less, and we are, somehow, skating over it. There are some answers to this problem. One can turn to expenditure functions and assume that the marginal utility of income is the same for everyone. But that is just another way of begging the question. Or one can say, Well, I am dealing with only one commodity at a time, and this is not a big problem in this case. But such responses are ultimately not very convincing.

There is, in fact, a real problem here. But my reading of it is that, by and large, it leads to a serious understatement of the gains from new technology. The simplest way of thinking about it is what happens if BLS or BEA were to

3. This refers to Rosanne Cole's comments about the use of the clock rate (logic cycles per second) as a characteristic of microcomputers in the paper on microcomputer prices by Ernst R. Berndt and Zvi Griliches (chap. 2 in this volume).

use a hedonic index, and there are only two or three models available in a particular period when the new machine has just begun to appear in the market. Let us say that all these models are on the line in some quality dimension, that we have agreed that this quality dimension is real and that people actually want it, and that it affects both costs and consumer value. But there are only three models, and they are all relatively small in size. There are people out there, however, who would like to buy a bigger package, a PC with more memory, with more power. But it is not available in this period. Now, next period it becomes available, and, lo and behold, its price is actually right on the earlier estimated price-quality line.

In this case, what the hedonic method will do is to say that the price has not declined. But the new package was not available last year because the true shadow price of that package was higher than may be indicated by its position on the estimated line, both because, in fact, it is not that simple to do the engineering to get it out there at roughly proportional cost and because the market was not there to produce it in large enough quantities, the economies of scale were not there yet that would allow such entry. As long as there is discreteness, the hedonic price indexes will underestimate the gains from either extending the spectrum of models or filling it in.

There is also a debit. The debit is that, to the extent that models go out, the hedonic index procedure will underestimate the loss to somebody of these models disappearing. Thus, if there is a decline in the demand for 4K RAMs and they are no longer produced, there will be a loss to somebody. Now what happens, in fact, is that things like 4K RAMs become a specialty item and that the few people who actually want them can still get them, but only at much higher prices. There is then a subset of models whose prices are actually rising, even though positive technological change is occurring throughout. It may be possible to capture some of that with appropriate weighting of the various components.

While there are real problems of measurement, my feeling is that, for most new goods, if we do not use suitable chain indexes and adjust for quality change, we will be underestimating the improvements in quality that are occurring. But, as far as long-run comparisons are concerned, I do not think that they are really possible. They are not really possible in Ed's framework, nor is it really possible to get good estimates in the framework that I prefer.

We have now, approximately, three times the per capita income of what our grandparents had, perhaps even more. Are we three times as happy as they were? Are we that much wealthier? I do not think that this is a question that can really be answered. One can easily think of things that go in the other direction that have been lost. In different ways, this is related to the fact that what these indexes measure is just a subset of outputs, a subset of consumer consumption. Much of what consumers get or do not get does not go through the market. It is in the environment, in our interrelationships among ourselves. There are externalities, both positive and negative, and they are just not being measured by our statistical system. But we should not use the fact that we cannot measure everything right to prevent us from trying to do what we can do slightly better.

Charles R. Hulten

One of the most important consequences of the BEA computer adjustment is, for me, the insights that it provides into the lingering controversy between Edward Denison, on the one hand, and Dale Jorgenson and Zvi Griliches, on the other. This debate had seemed to me to be about the appropriate methodology of growth analysis. But, in light of Denison's recent book (Denison 1989), it now appears that it is really about the underlying goals of productivity analysis.

Total factor productivity (TFP) is conventionally defined as the ratio of output to total factor input, measured in terms of the resource cost. In the neoclassical interpretation of Solow-Jorgenson-Griliches, the appropriate resource cost associated with capital is the sum of past investment adjusted for physical depreciation and retirement. The resulting TFP ratio is then interpreted as the shift in the aggregate production function for a given level of capital and labor. In this "Hicksian" framework, the Denison-Jorgenson-Griliches debate seemed to be about the most appropriate definitions of output and capital for measuring the magnitude of the shift.

However, Denison's endorsement of the Rymes approach to productivity analysis sheds new light on the old debate. In the Rymes view, technological change is seen as reducing the quantity of resources needed to reproduce the existing stock of capital. Thus, the conventional perpetual inventory estimate of capital overstates the actual amount of saving (consumption forgone) implied by the stock. Or, put differently, the conventional measure includes a component that is more appropriately classified as technical change.

The appropriate concept of TFP, in this alternative paradigm of capital, would exclude this technological component of capital from the denominator of the TFP ratio. The resulting concept of "TFP" is a ratio of output to the resources needed to sustain the level of output, and, as such, it incorporates part of what had previously been assigned to capital (in the old TFP concept). As a consequence, a source of growth analysis based on the alternative "TFP" concept would assign a larger role to productivity change and a smaller role to capital formation.

It would thus seem that the core of the Denison-Jorgenson-Griliches debate is, in the final analysis, a debate over whether "TFP" is a better measure of productivity change than the neoclassical TFP. But, before addressing this

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issue, it is useful to consider the question of how one might measure the alternative "TFP." The answer to this question sheds surprising light on the issue of which concept is the more appropriate measure of productivity change.

T. K. Rymes proposes to measure "TFP" using a Harrodian measure of technology in which "TFP" is equal to the conventional TFP divided by labor's share of income. This amounts, conceptually, to defining "TFP" as the Harrod rate of technical change, H. The Harrodian H is, itself, defined as the rate at which the production function shifts, measured along a constant capital-output ratio (instead of along a constant capital-labor ratio, as with Hicksian TFP). In terms of figure 11.1, Hicksian TFP is associated with the shift in the function $f(K/L, t)$ between the points a and b, whereas H is associated with the shift measured between a and c.

Furthermore, since the growth rate of output, $\dot{Q}$, is the same under either concept of productivity, the use of "TFP," cum $\dot{H}$, implies that capital must also be modified by subtracting $\dot{H}$ from the growth rate of the conventional (commodity) measure, $\dot{K}$. In other words,

$$\dot{Q} = (1 - \pi)\dot{L} + \pi \dot{K} + \text{TFP} = (1 - \pi)\dot{L} + \pi(\dot{K} - \dot{H}) + \dot{H},$$

where $\dot{L}$ is the growth rate of labor, and π is capital's income share. The resulting Rymesian concept of capital, $\dot{K} - \dot{H}$, strips commodity capital of the

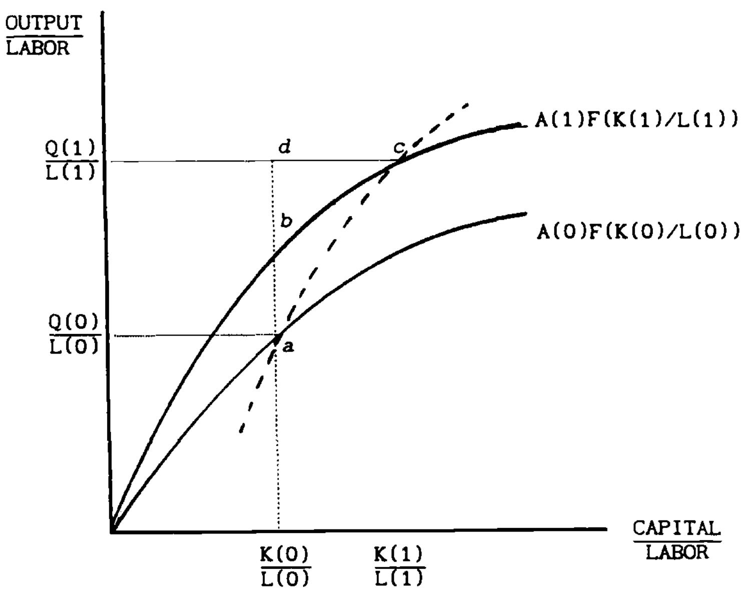

Fig. 11.1 Two views of the importance of technical change as a source of economic growth

technological component and thus corresponds to the notion of capital measured in terms of the resources needed to reproduce it.

I have offered an alternative solution to the problem of measuring "TFP" (Hulten 1975). I proposed a measure of the *consequences* of technical change for the growth rate of output. This differs from the "amount" of technical change when some inputs, like capital, are a produced means of production and therefore change as a result of the shift in technology. This "capital endogenous" measure of the effect of technology, $\dot{Z}$, was shown to be related to both the Hicksian and the Harrodian parameters of technical change:

$$\dot{Z} = \dot{H} + \frac{\pi}{(1 - \pi)} \dot{B} = \left[1 + \frac{\pi}{(1 - \pi)} \sigma\right] \dot{A} + \pi\sigma\dot{R},$$

where $\dot{H}$ is the rate of Harrodian technical change, as before, $\dot{B}$ is the bias, and $\dot{A}$ and $\dot{R}$ are the corresponding Hicksian parameters. When technical progress is Harrod neutral, $\dot{B} = 0$ and $\dot{Z} = \dot{H}$, implying that the $\dot{Z}$ defined above is equivalent to the Rymesian solution to measuring "TFP."[4]

The $\dot{Z}$ function defined above has a straightforward interpretation in terms figure 11.1. Suppose that the economy is in a steady state at point *a* with a static level of technology. Suppose, further, that a once-over shift occurs that makes capital and labor more productive. The output per worker will immediately jump to *b*, and this additional income will go, in part, to increasing the size of the capital stock. This generates still more income, and thus more saving, etc., until the diminishing marginal returns to capital bring the economy to rest at, say, point *c*. The $\dot{Z}$ measure of technical change can be interpreted as the rate of growth of output between the points *a* and *c*—that is, as the *total change* in output due to the shift in the technology—and this is larger than the initial change from *a* to *b*.

This leaves the following question. There are two ways of looking at technical change: the Jorgenson-Griliches-Solow conception of TFP, which is based on the Hicksian classification of growth and the perpetual inventory concept of capital, and the Harrod-Rymes approach, associated with the Harrodian classification of technical change and the Rymesian concept of capital, *K*/*H*. Which of these alternative paradigms is correct?

These alternatives seem fundamentally incompatible, and, indeed, there are heavy overtones of the Cambridge Controversies in capital theory. It is thus surprising that the correct answer is that *both* approaches are correct. Correct, that is, for different questions. The Jorgenson-Griliches approach is correct for answering the structural question of how much the production function has shifted relative to a given capital-labor ratio. In this approach, the total change in output per worker (*a* to *c* in fig. 11.1) is decomposed into two elements: the Hicksian shift in technology (*a* to *b*) and the change in output associated with capital formation (*b* to *c*).

On the other hand, our discussion of the $\dot{Z}$ function indicates that the entire

4. Note that TFP $= \dot{A} = (1 - \pi)\dot{H}$.

change (*a* to *c*) was the result of the shift in technology. The $\dot{Z}$ approach, or, more generally, the "TFP" approach, is appropriate for answering the question, How much more output growth is there because of technical change? or, How important is technical change as a source of output growth? This leads to the conclusion that it is *not* appropriate to use the Hicksian approach to conclude that technical change explains a/d percent of the growth rate of output.

To summarize, the questions, How much? and, How important? are separate issues that require different pieces of information to answer correctly. The advocacy of either the (A, K) view of the world or the $(H, K/H)$ view as a *joint* answer to both questions is thus wrong. Moreover, since knowledge of (A/K) leads directly to knowledge of $(H, K/H)$ because $\dot{A} = (1 - \pi)\dot{H}$, it should be clear that these supposedly contradictory paradigms are really different aspects of the same problem.

This leads us back to the Denison-Jorgenson-Griliches debate. Denison's endorsement of the Rymes paradigm can now be interpreted as a desire to measure the consequences of productivity change on output growth. The objective of Jorgenson and Griliches, on the other hand, was to measure the amount of technical change, and what was really a difference in goals became an unresolvable debate of methods.

Thomas K. Rymes

I agree with much—very much—of what Charles Hulten has said. If I have been guilty, in previous discussions, of claiming to be a source of truth, I can only apologize. My only defense is the usual one of seeking academic product differentiation.

I would like to reiterate how much I am indebted to Lawrence Read. He found a way of making operational Harrod's conception, which I shall be using, of technical progress.

Modern advanced economic systems are characterized by very large flows of outputs, only a small part of which is final consumption. I shall assume—just for purposes of exposition—that the flow of consumption output is homogeneous. The rest appear as additions to capital stocks and intermediate outputs. The economic system has very large flows of inputs. There are assumed, for simplicity, to be two kinds of primary inputs, inputs not produced, that is, by the economic system: (1) the flow of working, labor, and (2) the flow of waiting, the willingness of individuals to postpone present consumption to carry, maintain, and augment the stock of capital. The remaining inputs constitute the capital and intermediate inputs and are simultaneously part of the flow of outputs. I shall concentrate on the distinction between the primary and the produced inputs.

I shall compare two price indexes for capital goods such as computers. One

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is the BEA index, the other an index that is not adjusted, or not as fully adjusted, for quality change as is the BEA index. I will use the supposed difference between the BEA index and one not adjusted for quality change to get at just how the quality-adjusted price indexes affect total factor productivity, both at the aggregate and at the industry level. This is what I was invited to do. Even while I believe that the BEA indexes are perfectly meaningful, I wonder along with Edward Denison and others exactly how we should be using them.

Let me go to the equations. Let c and dk be the growth rates of the output of the consumption and capital goods industries; l, k, w, and r the growth rates of labor input, capital input, wage rates, and rate of return, with subscripts c or k indicating the consumption and capital goods industries; p_c and p_k the growth rates of the price of consumption goods and the price of capital goods; SL and SK the share of labor and capital inputs, with subscripts c or k indicating the industry; and t and h two different conceptual rates of total factor productivity called Solow and Harrod residuals, with subscripts c or k indicating the industry. Using Divisia indexes, the traditional measure of total factor productivity advance, or Solow residual, in the consumer good industry is

$$c - \{\mathrm{SL}_c l_c + \mathrm{SK}_c k_c\} = t_c = \{\mathrm{SL}_c w_c + SK_c(r_c + p_k)\} - p_c.$$

If p_k is said to be overstated and is replaced by a p_k^* such that $p_k^* < p_k$ and $k_c^* > k_c$, then the revised Solow residual is

$$c - \{\mathrm{SL}_c l_c + \mathrm{SK}_c k_c^*\} = t_c^* = \{\mathrm{SL}_c w_c + \mathrm{SK}_c(r_c + p_k^*)\} - p_c.$$

It follows that $t_c^* < t_c$ or the Solow residual will be reduced.

In the computer-producing or producer goods industry, however, the Solow residual would be

$$dk - \{\mathrm{SL}_k l_k + \mathrm{SK}_k k_k\} = t_k = \{\mathrm{SL}_k w_k + \mathrm{SK}_k(r_k + p_k)\} - p_k,$$

and, when the "quality adjustments" are made,

$$dk^* - \{\mathrm{SL}_k l_k + \mathrm{SK}_k k_k^*\} = t_k^* = \{\mathrm{SL}_k w_k + \mathrm{SK}_k(r_k + p_k^*)\} - p_k^*,$$

and, even though the "quality-adjustment" appears for both produced outputs and inputs, the Solow residual for the producer goods industry would be increased.

You are familiar with these Solow residuals. The rate of growth of the output of the consumption good less the competitive shares (equal to partial elasticity weights) times rates of growth of the labor input and the capital stock in the production of consumption equals the Solow residual in that industry. And so on.

In the full elaboration of the work that Alexandra Cas and I do on this case (Cas and Rymes 1991), the k's, as vectors, represent the flows of the services of capital goods of many kinds, the net services of capital goods that earn net returns to capital, capital consumption allowances, and the whole flow of intermediate inputs as well.

The Solow residual can be expressed in terms of the rate of change in input prices minus the rate of change of output prices. Let us focus on the computer-producing industry. (It is understood that there are many capital goods industries.) Suppose that the residuals had been derived with price indexes that are not adjusted for changes in quality and that now they are derived with price indexes adjusted for quality. Because adjusted price indexes fall much more dramatically, the output of the computer good industry rises much more dramatically, and the stock of capital or the flow of computer goods services in the production of consumption goods rises much more rapidly.

What happens to the Solow residuals? The rate of growth of the stock of capital in the production of the consumption goods is increased. The rate of change in the price of the capital goods in the production of consumer goods is decreased. If you increase the rate of growth of the capital in the production of the consumption goods and decrease the rate of growth of one of the prices of an input in the production of consumption goods, the result must be that the rate of technical progress, or the Solow residual, for the production of consumption goods must fall. As illustrated in the equations, $t_c^* < t_c$.

In the computer-producing industry, the use of the quality-adjusted price indexes increases the rate of growth of the output of the industry. If the computer good industry happens to be using some of its own output (the computer good industry certainly uses computers in the production of computers), a component of the capital input will also be rising more rapidly. Nevertheless, since the output of the industry is larger, the net result is that the Solow residual for the computer good industry is increased. What we have done is to redistribute increases in total factor productivity away from the production of the consumption good to the computer industry or to capital goods industries in general.

Now, as we have heard in this conference, the sharp fall in the quality-adjusted price index for computers is really a function of the sharp fall in the quality-adjusted price of semiconductors. So I disaggregate and make the qualitative adjustment for the semiconductors in the production of computers. The rate of growth of the capital input in computers will be raised, and one of the prices of an input into the production of computer goods will also be lowered. The result is that productivity advance, originally shown as taking place in the production of computers, is shifted over to the semiconductor industry.

However, in pursuing this further, the sharp fall in the quality-adjusted price index for semiconductors basically reflects a sharp fall in the quality-adjusted price index for chips, so I go back to the chip-producing industry and run the same drill. Indeed, as Ellen Dulberger suggests, I should make the quality adjustment for the ceramic inputs being used in the production of the chip industry, and that shoves some of the productivity advance back into the nonmetallic minerals industry.

You see the basic thrust of my argument. If I say that there is a difference between the unadjusted and the quality-adjusted price indexes for capital goods and the quality-adjusted price indexes always run below the unadjusted indexes, all I end up doing is trying to backtrack through the set of industries that are interrelated, trying to find out where the productivity advance actually occurs. That is the basic idea behind the work of Zvi Griliches and others because they are really interested in the problem of where in this interrelated system of industries the productivity advance does occur.

One final note on this sort of chain that I ran through. You obviously have a fair amount of trouble with traditional measures of total factor productivity if it turns out that the nonmetallic industry uses a substantial amount of computers in its operations. However, such interdependence is not insurmountable because we have, with advances in national accounting, input-output systems to handle the interdependence.

If there is a difference between the BEA and the non-quality-adjusted price indexes for produced outputs and inputs, we really do have a very severe problem in allocating traditional measures of total factor productivity among industries.

The conception of Roy Harrod, made operational by Lawrence Read, is that the primary inputs in the economic system are working, labor, and waiting, the postponement of present consumption, which is embodied in and appears as stocks of capital goods. You have to study advances in the productivity of those inputs in an economy characterized by technical interdependence. You can get measures by industry of rates of technological advance, total factor productivity, or Harrod residuals, which are not the same as the traditional measures advocated by Zvi Griliches and, I believe, Erwin Diewert.

Here are the alternative measures. In the computer-producing or producer goods industry, the Harrod residual would be

$$dk - \{SL_k l_k + SK_k(k_k - h_k)\} = h_k = \{SL_k w_k + SK_k(r_k + p_k + h_k)\} - p_k.$$

Again, if p_k is said to be overstated and is replaced by a p_k^* such that

$$p_k - p_k^* = dk^* - dk = k_k^* - k_k,$$

the revised Harrod residual would be

$$dk^* - \{SL_k l_k + SK_k(k_k^* - h_k^*)\} = h_k^* = \{SL_k w_k + SK_k(r_k + p_k^* + h_k^*)\} - p_k^*.$$

The Harrod residuals for the producer goods industries would be raised, but not to the same extent as the Solow residuals.

The Harrod residuals in the consumer good industry would be

$$c - \{SL_c l_c + SK_c(k_c - h_k)\} = h_c = \{SL_c w_c + SK_c(r_c + p_k + h_k)\} - p_c.$$

The revised Harrod residuals for the consumption good industry would be

$$c - SL_c l_c + SK_c(k_c^*(k_c^* - h_k^*)\} = h_c^*$$
$$= \{SL_c w_c + SK_c(r_c + p_k^* + h_k^*)\} - p_c.$$

Since $k_c^* - k_c = p_k - p_k^* = h_k^* - h_k$, it follows that the Harrod residual for the consumption good industry would be essentially unchanged.

In the Harrod representation of total factor productivity, with factors being the nonproduced primary factors of production, when one talks about the rate of capital growth in the production of consumption goods, one must take into account the fact that those capital goods, themselves, are being produced with ever-increasing efficiency. The "deflator" here is h_k, the rate of technical progress or total factor productivity in the production of the capital goods.

By adjusting the rate of growth of capital in the consumption good industry for the rate of technical progress in the production of such capital goods, I obtain the rate of growth of primary inputs involved in the production of the capital goods so that h_c, the Harrod residual, in the production of consumption goods, gives the measure of the rate of technical progress in the production of consumption goods in terms of the working and waiting directly and indirectly involved throughout the economic system in the production of consumption goods. One does the same thing with respect to production of capital goods.

As the Divisia equations illustrate, the revised price indexes would result in the gross output of the computer good industry being increased, which by itself would result in the Harrod residuals being increased. The increased flow of semiconductor inputs, because of the revision in their price indexes to take account of their qualitative improvement, which would reduce the Solow residuals, would be offset by the fact that the Harrod measures would adjust for the productivity advance in the semiconductor industry, in the chip industry, in the ceramics industry, and so forth. The Harrod residuals would measure the productivity improvement of the primary inputs of working and waiting, directly and indirectly involved in the production of computers, taking account of the complete interdependence of technology in modern economies.

That is really the basic point of my presentation. I was asked to take into account the effects that the introduction of the BEA computer price index would have on the measurement of total factor productivity. The traditional measures or Solow residuals are arbitrarily changed as one tries desperately to trace the qualitative improvements in produced inputs through all the industries of the economy. I have tried to demonstrate very simply that the Harrod residuals are very largely invariant to this because they always take into account the interdependence of modern economic systems in measuring productivity advance.

Comments by Panelists

Denison: Let me first correct one misunderstanding by Zvi Griliches. When I was speaking of the effect of using gross rather than net output, I was talking about depreciation.

Griliches: I know, but I was talking about different things.

Denison: Yes, intraindustry sales, and that is why I agree with what you said about industry data. I've written about that elsewhere, but today time restricted me to the totals.

Now as to the point about different capital being appropriate for different uses and users. Forget computers for a minute, and let me go back to the 1920s, when I was growing up in Chicago. The truck had replaced the horse and wagon in almost everything, but there were two exceptions. One was milk delivery. The horse went from house to house, and the milkman ran out to the back door and looked to see whether you wanted your usual two quarts of milk or had left a note saying that you wanted a quart of cream instead or only one quart of milk. Then he ran back for the correct order. The horse was much more efficient than the truck in that sort of stop-and-go activity. The other activity was ice delivery. As the iceman came by, he looked in your window, where you had placed a four-sided card. If you wanted ice, the side of the card showing twenty-five, fifty, seventy-five, or one hundred pounds was placed on top to indicate the amount. The horse had to stop at the house of each customer while the iceman looked at the card, cut the ice to the desired thickness, brought it inside, and put it into your refrigerator. The horse was smarter than the truck, which required more direction, and worked out much more satisfactorily.

Now what I'm trying to illustrate is that what's good for one use isn't good for another, as I said earlier. For these uses, the horse was still better than the truck. Horses continued in use until the electric refrigerator replaced ice and purchase of milk at the chain store became so much cheaper than home delivery that the latter ceased. Where one really sees quality improvement is not in the price, except by chance, but in quantities. The quantity of trucks increased, and the quantity of horses and wagons went down, and that's how you knew there was a quality improvement and that technical progress was occurring. And you might never have known it from any change in prices. If the truck had come in and its price had then stayed flat forever and the price of the horse and wagon had also stayed flat, you would know it only from changes in relative quantities; you would not know from prices that there had been any technical progress. And that's one of the things that makes quality adjustment difficult if one is trying to use marginal products to equate things.

The other complication has to do with the time of linking because, if you're trying to equate products by relative cost, you want, I think, to compare costs at a time when the new product is in quantity production like the old. You do not want to compare costs when production of the new product is at the beginning of the learning curve because it is so much more expensive than an established product and it is being used by only a very few people for very specialized uses.

Now, the last point really doesn't prove anything, but, nevertheless, it is remarkable that the period of very slow productivity growth and the period of the spread of the computer happen to be the same. One suggested explanation is that the input into computer use has been expanding and everyone's been spending his time learning how to use computers and so on but that the output hasn't happened yet. Another explanation has been that everyone is busy using the computer to help him compete with somebody else. He may be taking someone else's business away as a result, but, when he and his competition are combined, nothing has happened. Well, these explanations may be correct, or it just may be that we're overestimating how much the computer is contributing to efficiency, but there is certainly a bit of a mystery there.

Griliches: I think that the part of Edward Denison's concern about the unrepresentativeness and the timing of the introduction of new products into the price index would be taken care of by proper weighting; new products with low sales would get very little weight. This is no different from the current treatment of Cadillac purchases: they have little effect on the final index since their sales are rather low, relative to the car market as a whole.

I mostly agree with T. K. Rymes except that my conclusion is different. I am interested in the structural equation. He is interested in the reduced form. I am interested in knowing where the productivity change occurred because I am interested in understanding it and, possibly, also in affecting it. I would like to be able to connect the productivity numbers to R&D expenditures in industry and the particular scientific advances responsible for the productivity advances. Unless I know where the productivity change is coming from, I will have no way of affecting it. But, if all I can observe is some homogenized piece of manna falling down, then I am left without any useful explanation. It is true, however, as both Charles Hulten and T. K. Rymes emphasize, that some of the productivity growth would not occur without additional investment and that some of this investment is induced by technological change. A complete causal analysis would, in fact, account for it. But productivity accounting should be viewed as a tool for, an input into, such an analysis; it is not a substitute for it.

I am not sure that I see the relevance of Edward Denison's horse and tractor example except that it illustrates how difficult index construction can be. In fact, as old commodities decline in importance, they may find a particular niche in which they are still superior, and their prices would actually go up while quantities are declining. Again, appropriate weighting would take care of most of this problem. I think that Ed was saying, implicitly, that the quantities are important. I agree. They help us interpret what is happening to prices. The question of validating a particular interpretation of observed price changes, using quantity data, is also implicit in the Norsworthy and Jang paper (chap. 4 in this volume). In their framework, the hedonic price index is being validated by putting it into the demand functions for inputs and the supply functions for outputs and asking whether that kind of a respecification of the demand structure explains the facts better.

Hulten: A lot of the debate over the total factor productivity concept is, in my view, obscured by the use of terms that mean different things to different people. I would therefore like to make two proposals about the terminology of productivity analysis. First, I'd like to propose that the term *total factor productivity* be reserved for the shift in the production function measured at the prevailing capital-labor ratio (i.e., for the partial derivative of the production function with respect to time, holding capital and labor constant). As I noted in my preceding remarks, TFP, defined this way, is a measure of the extent to which technical change has improved the productivity of a given dose of inputs.

This implies that proponents of the Rymes-Harrod view of capital and technology must find a term for the *total factor productivity* to describe their effects. But, in exchange for this concession, the neoclassical school should acknowledge that TFP is *not* a valid measure of the importance of technical change as a source of economic growth. Capital is an endogenous variable in the set of equations determining the dynamic behavior of the economic system, and a shift in the production function (i.e., *total factor productivity* in the neoclassical sense) will cause capital stock to expand. Equivalently, one can think of technical change as making capital less costly to produce, so that a given rate of saving will generate a larger capital stock. Either way, the total increase in output that results from a shift in the production function exceeds the size of the total factor productivity residual.

As I noted previously, it is precisely the question of "importance" that motivates the concepts of capital and technology advocated in the various articles and books by T. K. Rymes and in my work also (see esp. Hulten 1979). The consequence of technical change is not the same thing as the amount of technical change, and the distinction should be clearly labeled. So my second proposal is that the neoclassical camp drop the term *importance of TFP as a source of growth* in describing the results of their growth analyses.

Rymes: I want to make three points. First, Charles Hulten really has a Fisherian concept, which is shared with René Durand at Statistics Canada (see Hulten 1975). It's a question of the timing of entrance of the improved capital goods into the stock of capital; it is not Harrod's concept.

Second, I want to disabuse Zvi Griliches of one thing. Harrod residuals are not, repeat not, only for steady states. And there is no infinite regress involved in Harrod residuals. Maybe I'm giving Harrod too much credit, but his axioms on economic growth are not locked into steady states or infinite regresses.

But let me come back to the point that Zvi Griliches makes. He says, "What I want to know is where it occurred, in what industry did the productivity advance occur?" And I share that view. The calculations that I presented show different rates of productivity advance by industry in the Harrodian sense. What I claim is that the Harrod residuals provide a more useful picture in terms of, say, a much better prediction of the behavior of relative prices than do the traditional measures of where technical progress occurs.

We can aggregate the Harrod residuals to get measures for the aggregate economy. The weights that are attached to each industry's residual are its weights in the final output of the economy by industry. So Edward Denison's end use approach is being met as well.

Finally, we still need to consider the fact that, in a world in which technical progress takes the form of constant changes in the characteristics of produced inputs used in the economy, characteristics price indexes for such inputs will always be falling relatively to the price index for consumption goods. This will be the case even if the consumption goods price index is adjusted for characteristics changes as well. In other words, constant price net capital formation will always be shown as rising relatively to the output of consumption goods, and the price indexes of capital formation will always be falling relatively to the price indexes of consumption goods.

I'm not sure that the governor of the Bank of Canada, John Crowe, if confronted with the facts that the prices of consumption goods were flat and the prices of new capital goods, or net capital formation, were declining, would conclude that price stability would require that price indexes of consumption goods be allowed to rise. I share the basic point expressed, I think, by Edward Denison and Charles Hulten that the end thing that we should be focusing on is the flow of consumption in the economy because that is what maximizes welfare.

The BEA (and new BLS) computer price indexes are "correct." Should they be used to "deflate" gross fixed capital formation? A case can be made for the deflation of gross fixed capital formation with consumption good price indexes. I have also suggested that the use of the BEA indexes for the calculation of total factor productivity at the industry level is all right, provided those estimates are Harrod rather than Solow residuals.

Comments from the Floor

Arthur J. Alexander: As somebody who's been working in product characteristics space for about twenty years, I'm a little less sanguine about it now than I used to be. We haven't taken sufficient account of the question of whether the mapping from characteristics space into utility or production or profitability space is really happening in our statistical estimates. Is this transformation or mapping really there? We don't look at final user's utility because it's very hard to get at; instead, we go on to the analysis of characteristics because it's a lot easier to deal with, rather than looking at how productive is that computer in one use or another use, or how good is a VCR or a stereo system, or how much does it contribute to utility—and making quality adjustments that way.

We have taken the easier step of finding the characteristics that seem to do the job as an intermediate product as a proxy for utility; we've accepted that proxy and worked with it, but we haven't verified that, in fact, the transformation and the mapping are doing the job that we want them to do. There's accumulating evidence that they may not be doing the job, that true measures of quality or performance or productivity are moving somewhat differently than measures based on characteristics. We have to take some time now to do some more work to see whether, in fact, the use of characteristics is appropriate and whether the transformations and the mappings that we are assuming are there are really there and really doing the job that we want them to do.

Edwin R. Dean: Edward Denison's book dealt almost as much with the BLS productivity program as it did with the BEA computer deflator, and the present discussion seems to have dealt with both. It might be a good idea to relate Ed's ideas to the BLS productivity program.

First of all, just for your information, where do things stand? Both in its labor productivity series and in its multifactor productivity series, BLS is currently using output measures based on BEA NIPA data, after adjustments.

For the capital part of its denominator in the multifactor productivity series, BLS begins its rather complex calculations of capital services by using the BEA capital investment series. Of course, like the output series, since December 1985, these investment series have reflected computer investment calculated by BEA with the deflator currently being discussed.

So we have continued to use these BEA measures. We are not necessarily

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pleased with every detail of the BEA computations, but we are certainly in accord with the general approach that underlies the BEA computer deflator.

The intellectual antecedents for our multifactor model are numerous. Four of them have been before you in person this afternoon. Certainly, Edward Denison's work has been one of our main intellectual antecedents. Charles Hulten, Erwin Diewert, Zvi Griliches, and others, including Dale Jorgenson, have also been important in this work.[5] But on the questions of the overall model and of capital inputs, our general approach has been closer to that of Erwin Diewert and Zvi Griliches than it has been to Edward Denison. And it would be redundant for me to try to repeat some of the things that Erwin Diewert and Zvi Griliches have said in explaining their views.

I think one point is worthy of some emphasis as far as our general approach is concerned. Zvi Griliches made a distinction between a structural equation approach and the reduced-form approach, as he would characterize T. K. Rymes's general model. We are very interested in issues of substitution between inputs, and you do not do a good job of capturing the prices that producers pay attention to in adjusting their input mix if you adjust the actual market prices paid by producers for productivity change, as T. K. Rymes would have us do. So, since one of our purposes is to be able to shed light on policy questions related to substitution among inputs, we are strongly attracted, in addition to the reasons that Erwin Diewert and Zvi Griliches outlined before, to an approach that takes into account quality adjustments, but we do not want to adjust these prices further for productivity changes. To adjust input prices for productivity change, as T. K. Rymes does, is to break the link between input prices paid by producers and the input prices used in productivity measurement.

That doesn't mean that we're entirely happy with BEA's use of base-period weighting in computing output. It doesn't mean that we're entirely happy with the kinds of input prices that have gone into the BEA computer model, but we are in accord with BEA's general approach to the computer price index.

René Durand: I would like to make two comments. The first relates to T. K. Rymes's presentation. In his model, as in our dynamic model, the capital stock does not appear as a primary input in the productivity equation. Capital is replaced by what Rymes calls the stock of waiting. Waiting is measured in homogeneous forgone consumption units of some base year and is not affected by quality changes just like hours worked measured in sacrificed hours of leisure. However, in Rymes's model, capital goods still appear as an output of the productive system, and, in that respect, the quality adjustment of the capital stock remains an issue.

In our dynamic framework, capital goods are neither primary inputs nor

5. W. Erwin Diewert participated in the panel discussion but did not submit his statement for publication.

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outputs of the productive process. Output is given by the infinite flow of present and future consumption over an infinite time horizon, and capital as an input is replaced by the stock of waiting, although our measure of waiting, derived dynamically, differs from Rymes's measure of waiting. Consequently, the issue of the quality adjustment of the capital stock deflator as such vanishes.

Over a limited time horizon, optimal growth is characterized by the "maximum" consumption path subject to some side condition for the capital stock at the terminal date. That capital stock represents the discounted value of future consumption and constitutes a pure stock of wealth. Consequently, and following Denison, we believe that the capital stock must be deflated by a final consumption deflator. Of course, the quality adjustment of the price deflator of consumption goods remains an issue, but that problem is of much less acuity than the quality adjustment of the capital goods deflator given the importance of computers in the capital stock and the rapid evolution of computers.

The second comment bears on Charles Hulten's reconciliation of T. K. Rymes's productivity model with the Jorgenson-Griliches model. This reconciliation sheds much light on the two alternative models and on how they are linked, and it is certainly welcome. However, I oppose Hulten's suggestion that we call Rymes's residual something other than multifactor productivity growth. On Hulten's figure 11.1, indeed, Rymes's production gain is decomposed into a shift in the production function (associated with the neoclassical productivity gain) and a move along that production function as productivity growth affects the growth of the capital stock. However, Hulten uses capital as an input rather than the stock of waiting. Were he to use the stock of waiting instead, then the production gain would be attributed entirely to a shift in the production function as both values of the capital stock in period 1 and 2 correspond to the same value in the stock of waiting. Therefore, with waiting instead of capital as an input, the whole production gain results from a pure shift in the production function, and I do not see why this shift would not be called technical progress.

Michael Harper: First of all, I don't think that it's useful to analyze the advance of computers in the context of a steady-state growth model or even in terms of a single shift in a steady-state model. I think that it's a dynamic process and involves ongoing change.

Second, if there is an innovation that improves a capital good, at what point does productivity change occur? Does it occur when the invention is made? Does it occur when the capital good is made, or does it occur when the consumption is actually realized? I think it occurs when the invention is made in the sense that the production possibility set is expanded at that point, but we

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can't measure it yet because we can't really observe it. We can first observe it in a market when investors reveal their evaluation by purchasing capital goods. So I think that that's the reason we at BLS prefer the approach of Erwin Diewert and Zvi Griliches and haven't switched to the other approach.

Rymes: I want to respond to Edwin Dean. I do not agree with the idea that our measures don't answer your questions. I will address this as tightly as I can. You want to know what the determinants are of changing relative prices. You want to know this because maximizing firms choose input combinations on that basis. The goods that fall relatively in price are the ones that they switch over to in their choice of technique.

It is my claim that you want Harrod residuals. Take two industries as an example. You want measures for the two industries, of the rate of technical change, taking account of all the direct and indirect effects through the whole economic system, that will predict—that will be associated with—the change in relative prices that's taking place. My claim, as I thought the use of the computers and semiconductors examples illustrated very clearly, is that the Harrod residuals do this. The traditional residuals simply do not. The price of computers is falling very rapidly, quality adjusted. The price of semiconductors is falling slightly less rapidly, quality adjusted. What the traditional measures of factor productivity will do when you make the adjustment for such change is to show the productivity advance in computers reduced and the productivity advance in semiconductors increased. The movement in the traditional measures of the rates of productivity advance at the industry level that you get does not answer your question. When you, Edward Denison, or I am interested in the basic microeconomics of the movement of, or the prediction of, the relative prices of produced inputs in the economic system and substitution among the inputs, the Harrod residuals give us a clue as to what's going on. As far as I can see, the traditional measures do not. The puzzle is why you still prefer the traditional measures.

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