June 20, 1996

Professor Jeremy Greenwood
Department of Economics
Harkness Hall
University of Rochester
Rochester, NY 14627-0156

Re: #92807

Dear Jeremy:

I am pleased to accept your paper with Zvi Hercowitz and Per Krusell “Long-Run Implications of Investment-Specific Technological Change” for publication as an Article in the AER. I have forwarded your manuscript to the Princeton office. Please contact them if you have questions about the publication schedule.

I am glad that we will be able to publish your paper. It is a very nice piece of work that will appeal to many readers.

Sincerely,

Kenneth D. West
Co-editor
Professor Jeremy Greenwood  
Department of Economics  
Harkness Hall  
University of Rochester  
Rochester, NY 14627-0156

Re: #92807

Dear Jeremy:

I have finally heard from the referee on your paper "Macroeconomic Implications of Investment-Specific Technological Change," who just today faxed the enclosed letter. Unfortunately, there was no report listing points that were hard to understand. I have e-mailed and called the referee asking for the list. If and when it arrives, I will forward it to you, for you to respond to at your discretion.

I have little confidence that I'll actually get the list from the referee in a timely fashion. I am pleased therefore to accept your paper for publication in the American Economic Review, conditional on you sending me three copies formatted in accordance with the enclosed style guidelines. Points particularly relevant to your paper are highlighted. As well, if you make any changes other than formatting ones, or minor stylistic corrections, please list them in your cover letter.

Finally, let me apologize for the outrageous delays in handling your paper. Lest this deter you from continuing to consider the AER as an outlet for your work, let me note that of the 800+ manuscripts that have crossed my desk in my three years as Co-Editor, none have taken remotely as long as yours to handle, and I am confident that any future submissions from you will be processed much more quickly.

Thanks. I look forward to receiving three copies of the final version of your paper.

Sincerely,

Kenneth D. West  
Co-editor  

enclosure
Dear Ken:

Enclosed is my review of the second revision of, "Long-Run Implications of Investment-Specific Technological Change". As I indicate in my report, I still have a hard time understanding a couple of the points the authors are trying to make. However, in my opinion these problems are not sufficiently serious to warrant rejecting the paper. Accordingly, I recommend that this draft of the paper be accepted for publication, and that it be up to the authors’ discretion to decide how to deal with the issues I raise.

I know I’ve made your life, and that of the author, miserable over this paper, and I apologize for that.
January 9, 1995

Professor Kenneth D. West, Co-Editor
*American Economic Review*
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1180 Observatory Drive
Madison, WI 53706

Dear Professor West:

Thanks for your letter. We appreciate the effort that you (and John) put in to get referee reports. In preparing the revision we have tried hard to bridge the gap between the theoretical and applied literatures on economic growth. We would like to provide you with a brief overview of our response to the referee’s concerns. Additionally, please find enclosed a more detailed reply to the all of the referee’s comments.

- **Referee’s point 1:** The referee observed that an equivalence relationship exists within a class of growth models with neutral and investment-specific technological change. In particular, given a model with neutral technological change it is possible to construct one with investment-specific technological change that gives exactly the same consumption and labor allocations, and vice versa. A detailed discussion of this equivalence issue is contained in the enclosed notes. First, we show that models differing in these two forms of technological change are not theoretically equivalent if they are restricted to have a common rate of physical depreciation on capital. Hence, it is meaningful to distinguish between models with these two forms of technological change. Second, we show that these models can be identified (i.e., they are not observationally equivalent) if NIPA accountants use constant units for measuring prices and quantities and are correct in their estimates of physical depreciation rates on capital. These assumptions are standard.

- **Referee’s point 2:** The referee observed that there may be other interesting frameworks that are also capable of explaining the stylized facts in question. To illustrate the point, the referee presents a two-sector endogenous growth model. We now discuss a general form of this two-sector model in some detail in the paper. (Actually, we had discussed this model in footnote 7 of the previous version of the paper. Clearly, though, this point needed amplification.) We argue that this type of model is hard to reconcile with the data since it requires empirically implausible factor shares. In response to the more general point made in the referee’s query, yes, there are other models that can explain the stylized facts. We therefore also included in the paper a brief discussion of three endogenous growth models that generate our model of investment-specific productivity change as a reduced form. The new material is presented in Section V of the revision.
Referee's point 3: The issue of how models should be matched up with data is an important one. A given model may demand that the data analysis is done in its own way. This is now discussed in the paper in some detail. The issue has three (related) aspects:

1. The referee raised some questions about the choice of numéraire—consumption units—for output. Our framework calls for all variables to be deflated by a common numéraire—that is, for all variables to be expressed either in units of consumption or units of equipment. We now illustrate—in Section III.A—that the choice of consumption goods for the numéraire is conservative relative to the other choice (which was the one suggested by the referee) in the sense that it minimizes the contribution of investment-specific technological change to output growth.

2. The choice of numéraire is not the basis of our dispute with Jorgenson and Hulten. We prove—in Section IV.A—that the Domar/Jorgenson model is mathematically identical to the standard neoclassical growth model and therefore that it does not admit the possibility of investment-specific technological change. Furthermore, it predicts that both the relative price capital and the investment-to-GDP ratio should be constant. Both of these predictions are counterfactual. When taking the Domar-Jorgenson framework as given, however, output should be "adjusted for quality". What this corresponds to is deflating consumption and investment spending by their own deflators. Our bone of contention is thus with the framework they use for growth accounting, not with their adjustment of output for quality.

3. Last, the NIPA adjusts output for improvements in equipment quality. The implications that this adjustment has for conventional growth accounting are discussed in Sections IV.B and IV.C.

To conclude, the discussion on point 3 has now been clarified, as suggested by the referee.

In line with your request, we have deleted the business-cycle section of paper as well as the appendix on vintage capital. Since the varying rate of capacity utilization was needed only for the business cycle analysis we have removed it too—and so have eliminated one of the referee's more minor concerns (point 1').

To conclude, we believe that all of the referee's concerns have been dealt with, and hope that you will find the new version of the paper to your satisfaction. Thanks for handling this submission another time.

Sincerely,

Jeremy Greenwood

encl: revised paper, notes on revision
Major Questions.

Question 1.

The referee points to a certain equivalence across models with different kinds of technological change. Two questions then arose concerning our analysis: (1) is it meaningful to distinguish models with neutral technological change from models with investment-specific technological change?; and (2) is it possible to use data to distinguish them?

We argue that the answer to both these questions is "yes". We interpret the first of the two questions as the most important one, but let us start to answer the second, since unless it is answered affirmatively, the first question becomes superfluous. We will then turn to the discussion of exactly what the equivalence means, and why it is not a problem for our analysis.

I. Identification.

Consider the following theoretical setup:

\[ c_t + i_t = z_t k_t^{\alpha} l_t^{1-\alpha} \]  \hspace{2cm} (1)

\[ k_{t+1} = (1 - \delta_t) k_t + i_t q_t. \] \hspace{2cm} (2)

The two equations, together with an initial condition for \( k_0 \) and a specification for the exogenous stochastic processes \( \{z_t\}, \{\delta_t\}, \) and \( \{q_t\} \) constitute a complete description of the production side of our model economy. The system we have specified nests the kinds of technological change under discussion.

I.1 Interpretations.

- The variable names are familiar; however, it is important here to stress that \( \delta_t \) is the rate of physical depreciation.

- If, ceteris paribus, \( z_t \) goes up, then for any given pair of capital and labor inputs, more output can be produced: there is technological improvement in the production of consumption and investment.

- If, ceteris paribus, \( q_t \) goes up, then it becomes less expensive to produce capital (and hence less expensive to produce consumption and investment in the future). We call this investment-specific technological improvement, since unless there is investment at time \( t \), future consumption possibilities are unaffected by the movement in \( q_t \).
1.2 Identification.

Can data be used to identify \( \{z_t\} \) and \( \{q_t\} \)? If so, we can use the model to ask about how much growth can be accounted for by investment-specific and neutral technological change.

The key data is:

1. Consumption and investment in current prices.

2. Price indices for consumption and investment.

3. Physical capital depreciation rates (from capital stock accounts).

The referee points out that the unit of measurement of capital is critical for the identification, and calls for us to be "totally clear about the units", i.e. about how we find an empirical counterpart to the theoretical units of capital.

What is crucial here is the price data. National income accountants' task is to disentangle prices and quantities from total values. Their procedures are not arbitrary. When they measure the capital input, they attempt to measure the number of productive input units: if a computer of vintage 1994 does the exact same productive job as two computers of vintage 1984, this implies that if the 1994 computer is assigned \( x \) input units, the 1984 computer should be assigned \( x/2 \) units (abstracting from physical decay).\(^1\)

The identification thus proceeds as follows.

- The values for \( \delta_t \) are observable.

- The model's \( k_t \) is obviously capital in productive units. The series for \( k_t \) that we use is constructed in accordance with our theory by using investment in consumption units, \( i_t \), and \( q_t \). Since \( 1/q_t \) equals the relative price of new capital in the decentralized version of the model, the relative price data directly gives us a series for \( q_t \).

- The sector-neutral shock \( z_t \) can be identified residually.

Conclusion: the variables \( q_t \), \( z_t \), and \( \delta_t \) can be identified, and thus we can identify which model the data corresponds to within the class of all models of the type (1)-(2).

\(^1\)From a practical point of view, the accountants use hedonic pricing and other methods to compare a given good of different vintages. Gordon's main contribution is a refined implementation of the existing methods. (For computers, Gordon's methods actually coincide with those of the NIPA accountants.)
As an application of this proposition to the potentially most damaging case for us, suppose contrary to our findings that the real world really had no investment-specific technological change, i.e. that \( q_t \equiv 1 \). In such a world, there really is no change in the relative cost of productive capital. This hypothesis thus implies that the recorded fall in relative prices could only represent an ongoing change in the units of measurement: capital must have been sold in smaller and smaller bundles, so that although a 1984 computer would be the same productive input as a 1994 computer, the latter has a lower price because it is sold, say, in half units. Moreover, the hypothesis implies that aside from physical decay, the capital stock appreciate over time, since units are getting smaller (i.e., economic depreciation is less than physical depreciation). This alternative interpretation of the data, of course, cannot be taken seriously.

II. The equivalence.

II.1 A formal statement of the equivalence.

What is the equivalence pointed to by the referee? Define

\[
C(\{z_t\}, \{\delta_t\}, \{q_t\}, k_0) \equiv \{\{c_t, L - l_t \} : \exists \{i_t, k_{t+1}\} : c_t + i_t = z_t k_t^{\alpha} l_t^{1 - \alpha}, \quad k_{t+1} = (1 - \delta_t) k_t + i_t q_t \}
\]

The set \( C \) specifies what one might label the economy's consumption and leisure possibility set: it specifies the set of feasible stochastic processes for consumption and leisure \( L - l_t \) as a function of what is exogenous.

The equivalence can be expressed in terms of how \( C \) varies over its domain. This domain, the class of all processes for the \( z_t \)'s, \( \delta_t \)'s, and \( q_t \)'s can be divided into subclasses over each of which \( C \) remains constant. Consider a given argument process \( (\{z_t\}, \{\delta_t\}, \{q_t\}, k_0) \). Then any alternative process \( (\{\tilde{z}_t\}, \{\tilde{\delta}_t\}, \{\tilde{q}_t\}, k_0) \) such that

\[
z_t q_{t-1}^z = \tilde{z}_t \tilde{q}_{t-1}^z, \quad (1 - \delta_t)(q_{t-1}/q_t) = (1 - \tilde{\delta}_t)(\tilde{q}_{t-1}/\tilde{q}_t) \quad t = 0, \ldots
\]

results in the same \( C \). To see this, transform variables in \( C \) so that \( \bar{k}_t/\bar{q}_{t-1} \equiv k_t/q_{t-1} \). In particular, as elaborated on by the referee, a model that only has investment-specific technological change can be expressed as one with only neutral technological change. With our notation: \( C(\{1\}, \{\delta_t\}, \{q_t\}, k_0) = C(\{q_{t-1}\}, \{1 - (1 - \delta_t)q_{t-1}/q_t\}, \{1\}, k_0) \) for any \( (\{q_t\}, \{\delta_t\}, k_0) \).

\(^2\text{We use } q_{-1} \equiv 1.\)
However, "most" models in this class are not equivalent. In particular, the two models given by \( (\{1\}, \delta_t), (q_t), k_0 \) and \( (z_t), \delta_t, \{1\}, k_0 \) are not the same: \( C(\{1\}, \delta_t, \{q_t\}, k_0) \neq C(z_t, \delta_t, \{1\}, k_0) \). These two models are identical in all dimensions except in that they have different forms of technological change. In particular, note that they have the same rate of physical depreciation, which we assume can be identified from data in the capital stock accounts. Our analysis in the paper consists of computing how much growth is due to each one of these two models.

II.2 Should we worry about whether \( z \) or \( q \) drives growth?

The equivalence property says for each world with \( z_t \equiv 1 \) and increasing \( q_t's \), there exists another world with \( q_t \equiv 1 \) and growing \( z_t's \) with the same (short- and long-run) properties. Should we care about which one of these two worlds we live in? The present framework is obviously not appropriate for dealing with this question, since we treat both \( z \) and \( q \) as endogenous. However: we don't view our results as revealing the fundamental source of growth, but rather its channel. Just like in the standard growth-accounting literature (and in most of economics), we take things as exogenous which obviously are not (but unlike standard growth accounting, we do make capital accumulation endogenous and specify the sources of its growth). Our results suggest that future research should perhaps be more concerned with the endogenous determination of the quality of new equipment than with that of total factor productivity.

We added a section in the paper making clear that our findings about the relative importance of \( z \) and \( q \) do provide preliminary insights also about properties of models which endogenize these variables and are consistent with our quantitative measurements. In particular, we find that our results cast some doubt on general human capital formation as the main source of postwar U.S. growth.

II.3 What does it imply for our measurement?

Finally, do the equivalence properties imply anything about measurement? They do imply that there are different ways of defining capital (as suggested by the referee). For example, capital could be measured in consumption units, \( \bar{k}_t \equiv k_t/q_t \). Rearranging (2), we would obtain \( 1 - \delta_t \equiv (1 - \delta_t)q_{t-1}/q_t \), so in the updating of capital we would need to use economic obsolescence, \( q_{t-1}/q_t \), in addition to physical decay. This economic obsolescence can be measured, obviously, by looking at the relative price decline for capital. Additionally, the relative price is needed in order to back

\(^3\)Unless, of course, \( z_t \equiv q_t \equiv 1 \).
out \( z_t \) from the neutral productivity residual obtained with this alternative measure of capital: \( \ddot{z}_t \equiv z_t q_t^2 \). By construction, this alternative setup is equivalent to our model, and any growth or business cycle accounting would of course lead to the same results as we obtain. We chose our definition of capital only because we found it more convenient.

**Question 2.**

This question specifies an alternative model without investment-specific technological change, which builds on different labor intensities in the consumption and investment sectors, to account for the secular features of the data. The referee then calls for a more extensive discussion of how our model compares to potential alternatives.

In response to this question, we have added a section to the paper. This section describes (i) a class of models encompassing the one suggested by the referee; and (ii) three alternative classes of models which endogenize the price decline for investment goods. The purpose of the discussion is to argue, first, that the model with sectoral differences in input intensities is hard to reconcile with the data. Second, we use the other models to show how our measure of investment-specific technology can be given "deeper" interpretations, without invalidating the more general proposition that there is something specific to the investment sector which drives 60% of aggregate growth.

**Question 3.**

In the third question the referee points out that if output were measured differently, e.g. in investment (efficiency) units, then one would need to adjust GNP to get a correct measure of total-factor productivity. This is correct: in this case, total-factor productivity would be redefined to equal \( \ddot{z}_t \equiv z_t q_t^2 \) (not \( z_t \)), so output in consumption units would need to be multiplied by \( q_t \) in order to respect the resource constraint. As long as the goal is to account for the role of \( q_t \) in the growth of consumption (or, output in consumption units) our results are the right ones. If, however, one wanted to account for growth in output in efficiency units, \( q_t \) would play an even bigger role than we claim it does: \( q \) would affect growth also via \( \ddot{z} \). This is now illustrated in the paper.

The point we make in our critique of Jorgenson, Hulten et al., however, is different. They actually use a different model—one which does not allow a decline in the relative price of new capital—and it is because they use a different model that they need to adjust output. Their technology is like
ours—in particular, we both use the same capital accumulation equation, $k' = (1 - \delta)k + i$, and the same data for $k$, $\delta$, and $q$—except for the formulation of the resource constraint: they specify $c + iq = zF(k, l)$, while we have $c + i = zF(k, l)$.\(^4\) Hulten mentions why a different model is chosen: it is costly to make capital production more efficient, and hence both $i$ and $q$ require inputs to be produced.

On one level, one should be free to choose models. However, the Domar/Jorgenson/Hulten model has several problems, as we elaborate on in the paper. One of these problems is that their budget constraint imposes a constant relative price of investment in efficiency units, which is obviously inconsistent with the large fall in this relative price. Another problem is that $i$ and $q$ enter as $iq \equiv x$ everywhere, and hence the model does not allow $q$ to affect growth.

Finally, the NIPA adjust output for quality in a fashion similar to that implied by the Domar/Jorgenson model. The implications of using NIPA output measures in conventional growth accounting when the world is characterized by investment-specific technological change are now discussed in the paper.

Other Questions.

Question 1.

We adopted the suggestion of the editor and the referee to delete the business cycle section of the paper. Given that a variable depreciation rate is not essential for the long-run analysis and growth accounting, we replaced it with a constant depreciation rate.

Question 2.

This question is related to Question 1 under Major questions. Given that both Domar/Jorgenson/Hulten and we use the physical depreciation rate in the accumulation equation for capital, the distinction between the two forms of technology shocks—$z_t$ and $q_t$—is meaningful. The implementation of the Domar/Jorgenson/Hulten model uses observed physical depreciation rates for $\delta_t$ and assumes that the relative price of investment in efficiency units is constant.

\(^4\)See the resource constraint in Hulten's paper (p. 967, equation 7).
July 20, 1995

Professor Jeremy Greenwood
Department of Economics
Harkness Hall
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Rochester, NY 14627-0156

Re: File #92807

Dear Professor Greenwood:

The refereeing of your paper with Zvi Hercowitz and Per Krusell, "Macroeconomic Implications of Investment-Specific..." has again taken too long, but I have now heard from the referee who was new to the last revision of the paper. He doesn't think you have effectively responded to his previous report. He finds your paper confusing and perhaps confused.

So I cannot accept the current version of your paper. I invite you to give another try to clarifying your argument. Let me suggest once again that you make a big part of your response a toning down of the vigor of your argument. The referee's patience is wearing thin. My impression is that he'll urge outright rejection of the next version if it still doesn't make sense to him, and I'll be hard pressed to convince myself to override him.

I want to apologize again for how long it took to reach a decision on your paper. I look forward to receiving your revision.

Sincerely,

Kenneth D. West
Co-editor

P.S. Minor points: (a) Is the calibration on pp10-13 ever used? Am I right that the period is annual? (b) Why don't you plot Figures 3 and 4 on the same scale.

Enclosure
Ref 2

Referee report on revision of 'Macroeconomic Implications of Investment-Specific Technological Change.'

General Remarks

This paper documents the secular fall in equipment prices and explores the implications of this for identifying the sources of growth. Its basic conclusion is that technological improvements in the manufacture of capital goods ('embodied technical change') is the main source of growth and that there has actually been technological regress in factor-augmenting technical change. This latter conclusion differs from what others have found, but the paper points out that the difference lies in an error made by these other researchers.

Inevitably, a careful project of this type must clarify various difficult measurement issues, the most important of these being the measurement of capital. My main problem with the first draft was its ambiguity on this dimension. Clarity on measurement is vital for the authors' basic conclusion: in my report, I raised the possibility that this conclusion can be turned on its head by a simple change in the choice of units of measuring capital. This led me to request that the authors be totally clear on two issues: (i) the units of measurement chosen for the analysis, and (ii) why their basic conclusion is not merely an artifact of the NIPA accountant's conventions regarding units of measurement.

The authors have been kind enough to write a separate document detailing how they think this and other issues I raised have been clarified in the new draft. I do not think they have been successful. Fundamental issues concerning units of measure have been left completely ambiguous. For example, at various times, the authors refer to capital as being measured in 'productive units', 'efficiency units' and 'physical units'. But, these terms are never defined. Instead, the reader is left to guess definitions from the context in which the words are used. In this guessing process, I was frustrated by a sense that the various terms might be mutually inconsistent. For example, when discussing the measurement of capital in 'productive units' on page 2 of their reply to me, the authors consider the case of technological improvement in computers. They assert that with the introduction of vintage 1994 computers, the 'input units' (?) assigned to 1984 computers should be dropped from $x$ to $x/2$. Does this not imply a very high rate of depreciation on old vintage computers upon the introduction of the new vintage computers? But, this
seems inconsistent with the 'physical' concept of depreciation espoused on the previous page (again, without formal definition). Perhaps all these terms can be defined precisely, and shown to be mutually consistent. But, this is the job of the authors, not the reader. It seems ironic that the authors are so exhaustively formal on the definition of a recursive competitive equilibrium - it is given a whole subsection, two full pages of space and is completely standard - but are so casual about the really fundamental concepts in the paper.

The authors assume a constant rate of depreciation on capital, a normalization which may pin down the units of measure to some extent. But, it does not do so fully, since this normalization leaves unstated the extent to which depreciation incorporates a systematic component in economic obsolescence.

Not only is the paper ambiguous on the units issue, but it is also unhelpful on point (ii) above. In the note to me, the authors end up saying (p.5) 'we chose our definition of capital only because we found it more convenient.' The only charitable interpretation of this statement is that they could not really have meant it. Taken literally, the statement implies that the authors accept my units result, and choose to nevertheless emphasize their own conclusion - rather than the opposite one - merely for reasons of convenience. I am sure I must have misunderstood this sentence. If not, it is tantamount to a statement that their basic result is a statistical curiosity.

The upshot of the preceding remarks is that I am not satisfied with the authors' response to my first question. I raised two other 'important' questions. I am totally satisfied with the authors' response to the second one: I like the discussion in section 5, probing a little deeper into alternative interpretations of 'embodied technological progress', and casting doubt on the factor intensity explanation of the fall in the relative price of capital.

I am not satisfied with their response to my third question. My impression on reading over their discussion of my third question, is that they simply ignored it. My third question raised the possibility that the differences regarding total factor productivity between their results and those of Gordon and Hulton simply reflect differences in the units of measurement of GNP. A much clearer answer like '...yes, the differences do reflect differences in units of measure, but the others use the wrong set of units - total factor productivity using GNP measured in their units reflects not just true total factor productivity, but also investment specific productivity', or something like that. The discussion in section 4.1 about the fallacy of the Domar-Jorgenson
model made little sense to me, since think of that model (equation (26)) as isomorphic to what the Solow model, for the reasons spelled out in my first question.

**Basic Recommendation**

Apart from my frustration with the units issue, my guess is that the authors' conclusion is basically right. I also think that the use of the equipment price data to learn something about the source of technological progress is very sensible, and the fundamental question addressed is extremely important. So, the paper would be a really solid contribution it could just be more precise about its basic terminology! Fixing this can't be a difficult thing to do!

**Detailed Comment on Authors' Response to My First Question**

Following is a detailed comment on the authors' reply to the first question in my referee report. My first question was, what interest is there in distinguishing different sources of growth when one can be transformed into another simply by a change in units of measurement? In the environment adopted by the authors, a change in units of measurement converts investment-specific into any one, or a combination of labor-augmenting, capital-augmenting or factor-neutral technical change. In this sense, these various sources of growth are equivalent. This equivalence is not a feature of all model environments, it's just a feature of the model environment adopted in the paper. My question seems like a straightforward one that any analysis of the sources of growth should be able to address easily. I completely expected this draft to clear this question up and was surprised and disappointed to find that it does not.

The authors' response takes the form of two observations: first, the NIPA accountants follow some unit of measurement convention consistently, and the objects under their measurement scheme can therefore meaningfully be measured. Second, the authors simply assert that it is meaningful to distinguish between investment-specific and other sources of growth, in their model environment. Neither response particularly addressed my concerns. I now discuss each in turn.
1. The first response seems obvious, but beside the point. Moreover, in the course of articulating this response, the authors state several things that I found outright confusing. For example, in part I.1 of the authors' response, they state that the change in units of measurement that converts investment-specific technological progress into some other source of technological progress in general entails a different depreciation rate. I made that point too, when I posed the first question in my referee report. I don't see how restating this point helps answer the question, and it left me spending a lot of time trying to figure out if I had missed something. Quite possibly I did. If so, I think it reflects poorly on the clarity with which this work has been written up.

(a) In addition to pointing out the unsurprising observation that the NIPA accountants' procedures for measuring prices and quantities are consistent over time, the authors also say that they use a 'correct' measure of the rate of physical depreciation of capital. Use of the word 'correct' seems to deny the whole equivalence result whose interpretation is at issue. Again, I may have missed something.

(b) As I stated above, I find the paper very vague and confusing on the issue of the unit of measurement of capital. At one point, the authors say \( \delta \) is the rate of physical depreciation, at another point it is declared that capital is measured in productive units. But never are these objects carefully defined. To try and explain why I found this and other statements by the authors confusing, I computed a simple numerical example. The example is reported using two alternative units of measurement. The first measurement convention, 'measurement \#1', seems to correspond best with what the authors have in mind. The resource constraint is \( c + i = y \), where \( y \) is total output, measured in units of the consumption good. The production function is assumed to be linear: \( y = k \). The capital accumulation equation in this case is \( k' = (1 - \delta)k + iq \), where \( q \) denotes the rate at which investment is transformed into new capital, and \( \delta \) is the rate of depreciation, assumed to be .1. In the example, \( q \) is 1 in periods 0 to 2 and then switches to 2 in periods 3 to 6. This captures the idea, emphasized by the authors, that there is investment-specific technological progress: the rate
at which consumption goods can be transformed into new capital goods doubles. I've assumed, for the sake of the example, that people save enough to just keep \( y \) unchanged. As a result, investment is \( .1 \) in periods 0 to 2, and then drops in half from then on. This implies that consumption is \( .9 \) in periods 0 to 3, and \( .95 \) thereafter. In part I.1 of the authors’ response to my report, they seem to indicate that measurement \#1 is the orientation to measurement adopted in the paper, and by the NIPA accountants. Under this normalization, the capital stock seems to be measured in units which perhaps could be termed, 'physical'. It also corresponds to 'investment-specific technological change' in the sense defined by the authors: technological change having the property that, to exploit it, there must be investment.

An alternative measurement normalizes \( q = 1 \). As noted in my first referee report, this necessitates a new measure of the stock of capital, \( K \), and of the rate of depreciation, \( \delta \): \( K' = k' / q \), \( \delta = 1 - (1 - \delta)^{\frac{1}{2}} \). Then, the production function is written \( y = ZK \), where the state of technology, \( Z \), is \( Z = 1 / q_{-1} \). The resource constraint is \( Z' = (1 - \delta)Z + i \). This measurement scheme ('measurement \#2') offers an alternative interpretation to the events under measurement \#1. Under this interpretation, it is not the rate of transformation from consumption into capital that changes, but instead it is capital itself that has become more productive. A consequence of this is that capital of different vintages are different, and this must be taken into account when computing the aggregate capital stock. In particular, the component that corresponds to 'old' capital must be devalued in relation to new capital. This 'obsolescence' of old capital is reflected in a jump in the depreciation rate in period 3, to 55%. After this, starting in period 4, the quantity of capital in existence is much smaller, though that capital is now much more productive, with \( Z \) doubling. In part I.2 of the authors' response to my referee report, in discussing the approach to measurement taken in the paper, the authors use language better suited to a description of measurement \#2. For example, (p.2) - 'When they [NIPA accountants] measure the capital input, they attempt to measure the number
of productive input units: if a computer of vintage 1994 does the exact same productive job as two computers of vintage 1984, this implies that if the 1994 computer is assigned $x$ input units, the 1984 computer should be assigned $x/2$ units. In the period of the innovation, this implies a very large rate of depreciation, as 1984 computers are reduced in value by one-half, as under the measurement #2 scheme.

Although the authors' own language seems tailored to the measurement #2 scheme, at the end of section I.2 the authors clearly distance themselves from the measurement #2 interpretation. There, they equate $q = 1$ with the state of affairs in which (p.3) '...the real world really ha(s) no investment-specific technological change.' The authors then go on to discuss some alleged implications of the measurement #2 view, which lead them to conclude that '...it cannot be taken seriously.' But, the implications the authors allege seem wrong. The authors (seems to) say that in a world with $q = 1$, but continuing progress of the kind in my numerical example, there would be a 'ongoing change in the units of measurement'. I don't see that: the units of measurement would be $constant$, with new capital always being measured in current consumption equivalent units, and with the depreciation rate and technology growth rate permanently higher than under the measurement #1 scheme.

Example

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</table>

2. At the beginning of their response to my referee report, the authors advertise that their response to my question regarding whether it is mean-
ingful to distinguish between investment-specific and other sources of growth is 'yes'. Yet, when we get to that answer, on page 4, it is stated that an answer cannot be given. Moreover, the claim is made that '...we don't view our results as revealing the fundamental source of growth, but rather its channel.' I thought the point was to show that the source of growth is investment-specific, rather than something else. In the end, the authors simply say, (p.5) 'We chose our definition of capital only because we found it more convenient.'
Professor Kenneth D. West, Co-Editor
American Economic Review
Department of Economics
University of Wisconsin
1180 Observatory Drive
Madison, WI 53706

Subject: Ms. 92307

Dear Professor West:

Please find enclosed a revised version of the paper “Macroeconomic Effects of Investment-Specific Technological” (joint with Zvi Hercowitz and Per Krusell). Both referees provided detailed and thoughtful reports. In preparing the revision, we have taken into account the helpful comments we received from both referees. John Campbell identified one of the referees as Professor Charles R. Hulten, a noted authority on growth accounting. Professor Hulten (whom we have never met) had kindly indicated a willingness to communicate with us, and this hopefully has facilitated the revision process. Here’s a brief overview of major changes that we have made:

- Referee # 3 (Professor Charles R. Hulten): The paper has been thoroughly rewritten. This was done to give the paper a much sharper focus and to make it appealing to broader readership. Following the suggestion of the referee (and John Campbell), the simple model presented in the Appendix A of the previous version is now used as the primary vehicle for the analysis in the main text. The paper is a close cousin of recent work by Hulten (AER 1992), who also stresses capital-embodied technological as a key factor in productivity growth. The methodologies, as well as the findings, are very different in the two papers. The difference between the two methodologies is now discussed in detail. Also, a full accounting for the difference in the findings (along the lines suggested by the referee) is provided. Finally, the referee noted that the analysis did not take into account household capital — which is slightly larger than business capital. This implies that the value of gross housing product should be netted out of the GNP figures before conducting the growth accounting exercises. We have now done this. (Unlike housing, the GNP accountants do not make an imputation for the service flow realized from the stock of consumer durables).

- Referee # 5: We have added neutral productivity shocks into business cycle analysis. As the referee suggested, the model does do better in sense that it can account for a larger part of the cycle. The correlations between output, on the one hand, and investment decomposed into equipment and structures, on the other, are reported.

John noted that there may have been a recent drift in effective tax rates favoring the accumulation of equipment vis-à-vis structures. While interesting, this would be hard to incorporate into the paper. The conventional trick for analyzing nonstationary growth models is to con-
vert them into a stationary representation — this is what we did in the analysis. To the best of our knowledge, this can't be done if taxes are nonstationary. Now, could the recent drift in effective tax rates on capital income be responsible for the observed rise in equipment-to-GNP ratio? We think not for two reasons. First, the increase in the equipment-to-GNP ratio can be traced back using NIPA data to at least 1925. The drift between the effective tax rates on equipment and structures only begins in 1962. Second, a fall in the effective tax rate on equipment should lead to a rise in the relative price of equipment, not the observed decline. All of this is discussed in the revision. Last, we note that Figure 1, which shows the increase in the equipment-to-GNP ratio, uses standard NIPA data. We now make this clear, as John requested. If equipment was expressed in efficiency units the trend portrayed would be even stronger, further bolstering the case for investment-specific technological change.

We thank both referees (and John) for their comments and hope they have been satisfactorily incorporated into the revised version of the paper. We look forward to hearing from you.

Sincerely,

Jeremy Greenwood

Professor of Economics

cc: Professor John Y. Campbell

encl: paper
October 25, 1994

Dear Professor Greenwood:

The refereeing of your paper with Zvi Hercowitz and Per Krusell, "Macroeconomic Implications of Investment-Specific Technological Change," has taken a ridiculous amount of time, but despite repeated phone calls, faxes and EMAIL messages it was not until last week that I heard from the two referees. One of the referees was Charles Hulten, who sent a letter but no report since "I don't think any more interaction with the authors will get them to change their position." The letter also describes your paper as "much improved" and publishable, although Hulten told me in a phone call that if your paper is published he wants to write a rebuttal. The other referee, who did not review the earlier version of your paper, concludes that your paper leaves him confused. This referee's cover letter states that at a minimum your paper requires a rewriting to clarify what is going on. The cover letter also states that if such clarification is successful the paper "would be splitting at the seams" and suggests that the business cycle part of the paper be put into a separate paper. Finally, in a phone call the referee suggested removing the appendix as well.

The bottom line is that I cannot accept the current version of your paper but would be interested in a revision that clarifies your argument. The referee's point 2 (pp5-7 of the report) and point 1' (pp9-11) perhaps require nothing but a brief defense of the functional form you chose. If you agree with point 3 (pp7-9) and point 2' (pp11-12) there may be little required beyond toning down the vigor with which you argue your case. I take the upshot of point 3, for example, to be that even if you have worked through a logically coherent model in which output should not be adjusted, it is possible as well to work out ones in which output should be adjusted. If you agree, the statement on the bottom of page 3 that your approach provides a "decisive answer" is in the referee's (and I guess Hulten's) view one that is misleadingly strong. Finally, response to point 1 (pp3-5) may bring with it a response to points 3 and 2'.

In addition, I endorse the referee's suggestion that both the appendix and the business cycle part of your paper (part IV, and assorted references in the paper) be removed. (Minor
point, which you may want to take into account if you write the business cycle part up as a separate paper; I agree with the comment made at the NBER meeting that those outside the RBC world will likely misinterpret your p3 statement about "20% of the variability" as referring to a variance decomposition rather than a ratio of standard deviations.

It seems possible that nothing but a clarifying re-write will be required. Please make your revision, which I will send to the anonymous referee, double spaced.

I want to apologize again for how long it took to reach a decision on your paper. I look forward to receiving your revision.

Sincerely,

Kenneth D. West
Co-editor

enclosure
Summary

This paper observes that the price of equipment has fallen secularly, while a measure of the equipment to GNP ratio has risen. At the same time, it observes that the correlation, at business cycle frequencies, between the detrended price of equipment and the detrended quantity of equipment is negative. These negative covariances at low and high frequencies lead the authors to infer that exogenous technical progress in the manufacture of new machines ('investment-specific technological change') has been an important factor underlying both long-run growth and business cycle fluctuations. The purpose of the paper is to assess the quantitative magnitude of this source of growth and business cycles. It does so by comparing its contribution to growth and business cycles with that of sector-neutral technical change.

The paper's major conclusions are: (i) that most of long-run growth is due to investment-specific technical change in equipment investment, (ii) a significant fraction (about 20 percent) of business fluctuations reflect the effects of random disturbances in investment-specific technical change, and (iii) most surprisingly, once proper account is taken of the substantial improvements in the technical efficiency of equipment, one is led to conclude that the state of sector-neutral productivity has declined dramatically. Indeed, their Figure 5 indicates that this variable has declined at roughly 0.9 percent per year from 1972 to 1990. The state of sector-neutral technology is now roughly where it was in 1962, and extrapolation suggests that by the year 2000, the state of our sector-neutral technological knowledge will be where it was at the end of World War II.

The paper’s result (iii) appears to be at variance with findings in the literature, and the authors devote some effort to diagnosing this. They compare their work to that of Hulten, whose calculations indicate that multifactor productivity growth has been positive since 1972. The authors argue that their differences with Hulten reflect differences in the way the
NIPA data on output are adjusted to take into account the problems documented by Gordon on the measurement of the quantity and price of equipment. Gordon argued that official measures overstate the price of equipment because they fail to take quality improvements properly into account. Gordon produced an adjusted price series for equipment that both Hulten and the authors use. Obviously, using the adjusted price series requires that one revise upward the official estimates of real equipment investment and, hence, of the stock of equipment.

Multifactor productivity is output divided by the part of output that can be accounted for by capital and labor alone. So, the partial effect of revising upward the quantity of equipment is to reduce multifactor productivity. Whether the total effect is positive or negative, however, depends on how one adjusts output. Hulten adjusted output upward to reflect the upward revision in real equipment investment. The authors claim that Hulten makes a conceptual error in doing so, and that the correct thing is not to adjust output at all. This, according to the authors, lies at the heart of the apparent differences between their results and those of Hulten. Presumably their observations apply equally to Gordon (1990; BPEA, 1993, pp. 271-306). He, like Hulten, uses the Gordon adjusted equipment price and quantity data. He, like Hulten, revises upward the output measure. He, like Hulten, concludes that multifactor productivity has been rising (though not rapidly) since 1972.

Comments

As will be clear from my report, this paper has left me very confused. That is why my report is written in the form of a series of questions. If it turns out that the questions do not point to very severe defects, then the paper should be rewritten in a way that clarifies the questions raised. Such a paper would be a very nice contribution.

I have five questions, with the first three being relatively more important than the last two. Most are requests for a stronger defense of the model used in the analysis. The first question begins by reminding the authors of the 'equivalence' between various sources of
growth (investment-specific, labor-augmenting, capital-augmenting, sector-neutral) in an environment like the one in this paper. It then asks them to explain why their objective of differentiating between different kinds of growth is not rendered meaningless in their environment. The second question in effect also asks for a stronger defense of the model used in the analysis, by drawing attention to other ways of explaining trends in the price and quantity of equipment that do not depend on exogenous technical progress. The third question asks if the difference between the authors and Hulten reflect conceptual differences in the multifactor productivity measures computed, rather than conceptual errors made by Hulten (and Gordon).

The final two questions ask, first, for a stronger defense of the paper's model of variable capital utilization. In particular, the authors should explain why they do not use what I believe is the more conventional model, namely the workweek model. Finally, I have a question about the paper's observations regarding the Domar-Jorgenson model.

Major Questions

1. It is well known that with a Cobb-Douglas production function, like the one used in the paper, sector-neutral productivity growth, labor-augmenting productivity growth and capital-augmenting productivity growth are all equivalent in the following sense. How one decomposes the sources of growth between investment-specific, capital-embodied, labor-embodied, or a combination of all three depends on two things: (i) the units in which one chooses to measure capital and (ii) the order in which one multiplies capital, labor and 'sector-neutral' technology in the representation of the production function. The authors need to explain why the fundamental objective of their analysis - assessing the relative importance of investment-specific versus sector-neutral technical progress - is not rendered meaningless by this observation.

To clarify this, consider the following production technology relating consumption, $c$,
investment, \( I \), the stock of capital, \( k \), the services of labor, \( l \):

\[
c + Ip = k^\alpha l^{(1-\alpha)},
\]

where \( p \) is an exogenous technology parameter which controls the rate of transformation between consumption and investment goods. Let \( \gamma \equiv p/p_{-1} \) denote the growth rate of \( p \). When \( \gamma < 1 \), we say that there is positive 'investment-specific technological change.' The capital accumulation equation is:

\[
k' = (1 - \delta)k + I.
\]

This is a world in which the authors would say there is only investment-specific technological change, and no sector-neutral technical change.

But, this characterization is just an artifact of the units in which capital and investment are measured. Consider, for example, the following change of variables:

\[
\delta' = \delta, \quad \delta = 1 - (1 - \delta)\gamma, \quad z = \left( \frac{\gamma}{p} \right)^\alpha, \quad i = Ip.
\]

(Note, when \( \gamma \) is random, then the depreciation rate, \( \delta \), is random too.) Then (0.1)-(0.2) can be rewritten

\[
c + i = zK^\alpha l^{(1-\alpha)},
\]

and

\[
k' = (1 - \delta)K + i.
\]

This is an economy which the authors would characterize as being driven exclusively by sector-neutral technological change. This economy could also be characterized as growing due to labor-augmenting technological change, simply by reordering the terms in (0.3) as follows:

\[
c + i = K^\alpha (Zl)^{(1-\alpha)}, \quad Z = z^{1/(1-\alpha)}.
\]
context, about the fall in equipment prices and the rise in the ratio of equipment to GNP. One such framework is a two-sector endogenous growth model with no exogenous technical change that is analyzed in Manuelli-Jones (October 1993, 'The Sources of Growth', unpublished, pp.17-21). The planner maximizes

\[
\sum_{t=0}^{\infty} \beta^t u(c_t, 1 - l_t), \quad u(c_t, 1 - l_t) = \frac{c_t^{(1-\sigma)} (1 - l_t)^{\sigma}}{(1 - \sigma)},
\]

(0.5)

where \(c_t\) denotes period \(t\) consumption and \(l_t\) denotes period \(t\) labor services. Consumption goods are produced by a standard Cobb-Douglas production technology involving capital, \(k_t\), and labor, and investment goods are produced using a linear production technology involving only capital in the investment goods sector, \(k_{2t}\):

\[
c_t = Ak_t^{ \sigma/(1-\sigma)} , \quad I_t = bk_{2t}, \quad k_{t+1} = (1 - \delta)k_t + I_t, k_t = k_{1t} + k_{2t},
\]

(0.6)

where \(k_t\) denotes the date \(t\) aggregate stock of capital. Define a balanced growth path as one in which all variables grow at a constant rate. Along such a path, \(k_t, k_{1t},\) and \(k_{2t}\) all grow at the rate \(\gamma_k\), and consumption grows at the rate \(\gamma_c\), where

\[
\gamma_c = \{ \beta[1 - \delta + b] \} ^{\sigma/(\sigma-1)}, \quad \gamma_k = \gamma_c^\delta, \quad \gamma_k > \gamma_c \text{ if } \gamma_c > 1.
\]

(0.7)

GNP in consumption units, \(y_t\), is:

\[
y_t = p_{kt} I_t + c_t
\]

(0.8)

where both \(c_t\), \(p_{kt} I_t\), and, hence, \(y_t\), all grow at the rate \(\gamma_c\). Since \(p_{kt} I_t\) grows at the rate \(\gamma_c\) and \(I_t\) grows at the rate \(\gamma_k > \gamma_c\), it follows that \(p_{kt}\) grows at the rate \(\gamma_c/\gamma_k\), which could be negative (i.e., \(\gamma_c/\gamma_k\) could be less than 1). For example, assuming a quarterly time period and per capita quantities, then \(\gamma_c = 1.004\) and \(\alpha = .36\) are plausible numbers. Then, \(\gamma_k = 1.0112\) and \(\gamma_c/\gamma_k = 0.9929\), i.e., the price of investment goods shrinks at a
-0.7% quarterly rate. Also, the ratio of the stock of capital to \( y \) grows 0.7% per quarter \((= (\gamma_k - \gamma_c) 100)\).

This example shows that exogenous technical progress specific to investment is not at all necessary to account for the trend in the relative price and quantities of capital. The example further raises the burden on the authors to better motivate the particular model they chose to work with in the paper.

3. I found the discussion of the relationship between Hulten's work (and, hence implicitly Gordon's work too) and the work in this paper to be very confusing. I will try to explain why I am confused and perhaps the authors can figure out if there is a way to rewrite the paper to clear up the confusion. My concern is that the apparent inconsistency between Hulten and this paper reflects not that somebody made a conceptual error (as this paper seems to suggest), but that what Hulten calls 'multifactor productivity' is conceptually different from the authors' empirical measure of the state of sector-neutral technology.

I begin by stating my understanding of why the authors feel they need not adjust output to correct for the error pointed out by Gordon. I believe this reflects that in their concept of multifactor productivity, output is measured in units of consumption goods. In this case, investment appears in output in the form of the product of price and quantity. Since no one claims that this product is ill-measured (the only claim made is that the government does not decompose this product properly into price and quantity), it follows that mismeasurement of investment prices does not imply any mismeasurement of output denominated in consumption units.

I will try and make this more precise. Nominal GNP is defined as follows:

\[
GNP_t = p^e_t c_t + p^I_t I_t, \tag{0.9}
\]

in obvious notation. Then, GNP denominated in consumption units is obtained by dividing,
nominal GNP by the consumption price deflator, $p^c_t$:

$$\frac{GNP_t}{p^c_t} = c_t + \frac{p^k_t I_t}{p^c_t},$$  \hspace{1cm} (0.10)

Here, $p^k_t I_t$ is investment expenditures, in current dollar terms. No one claims there is any mismeasurement of this term. The problems have to do with the decomposition of this term between $p^k_t$ and $I_t$. Now, the authors' assumed resource constraint is:

$$c + Ip = z k^\alpha l^{(1-\alpha)},$$  \hspace{1cm} (0.11)

where $z$ denotes the sector-neutral state of technology. Assume that markets have the effect of enforcing the relationship, $p = \frac{p^k}{p^c}$. Then, to compute $z$ in (0.11), one needs to solve:

$$z = \frac{GNP_t}{k^\alpha l^{(1-\alpha)}},$$  \hspace{1cm} (0.12)

The important point to note here is that no adjustments are required in the numerator: nominal GNP and the consumer price deflators are presumed to be correctly measured. Only the denominator requires adjustment.

According to Gordon, the government overstates $p$ and so understates $I$. This means that the capital stock series has to be adjusted upward. In sum, if one seeks $z$ defined in (0.11), then to take into account the measurement problems analyzed by Gordon, one needs only to adjust the denominator in (0.12), and not the numerator. In this, the authors surely must be absolutely right.

Now suppose that instead the authors had written the following resource constraint:

$$qc + I = \tilde{z} k^\alpha l^{(1-\alpha)}$$  \hspace{1cm} (0.13)

where now $q$ and $\tilde{z}$ are the exogenous technology shocks. The difference between (0.11)
I think that anyone living in this simple Cobb-Douglas economy would understand that whether you call the source of growth labor-augmenting, capital-augmenting, or both, it's always the same thing. These different labels just correspond to different ways of measuring capital, and to one's preferred order for multiplying $K, l$ and $z$.

Although I have a suspicion that these questions are damaging to the enterprise in this paper, I don't really know for sure. However, if the authors are aware that they are not damaging, then they should be crystal clear why not. Given the sensitivity of things to units of measure, they should be totally clear about the units in which they measure capital (i.e., what they mean by 'efficiency units'), and they should be sure that the units in which Gordon measures capital indeed correspond to the units in which capital is measured in their model. As a side note, the existing real business cycle literature has been very clear about the units in which capital is measured. That literature generally adopts the measurement convention implicit in (0.3)-(0.4), where new capital is measured in units of the current period consumption good, i.e., the price of new capital is normalized to 1. This restriction is just a normalization in a nonstochastic context (as long as there is a potentially stochastic sector-neutral technology term that is allowed to grow), and is in a stochastic context too, as long as random discount factors that are negatively correlated with $z$ are allowed.

As is well known, these issues with regard to the equivalence of investment-specific and sector-neutral technological change are sensitive to the form of the production function used. Had a general CES production function been used instead, then my arguments would have broken down if the elasticity of substitution between labor and capital differed from unity. In this case the distinction between sector-neutral and investment-specific technological change would have been meaningful. However, as the authors point out in footnote 4, in this case they most likely lose the balanced-growth feature of their model economy. Given their heavy use of balanced growth properties, going to a general CES production economy is probably not a practical option.

2. But, there are alternative frameworks that allow one to think, in a balanced growth
and (0.13) is seemingly trivial (in effect, I have just replaced z by z/p). Yet, it has a substantial impact on the authors' conclusion that when computing the state of sector-neutral technology, real output need not be adjusted to reflect Gordon's correction to the equipment price data. To measure the state of technology, \( \bar{z} \), now requires the following formula:

\[
\bar{z} = \frac{GNP}{\frac{p_t}{k\alpha(1-\alpha)}}
\]  

(0.14)

To compute the measure of sector-neutral technology, \( \bar{z} \), (0.14) indicates the numerator and the denominator must now be adjusted. The problem is that to measure \( \bar{z} \), one now needs output denominated in investment units. That's why GNP must be divided by the price of capital. Now the numerator clearly is influenced by the quality correction in \( p^t \).

So, the authors' assertion that the output measure used to compute sector-neutral technology should not be adjusted to reflect Gordon's quality adjustments seems to be based on the assumption that the relevant real output measure is denominated in consumption units.

Measuring real output in consumption units is certainly unconventional. For example, the official measure of real output is measured in base year prices. As the preceding example illustrates, a switch in the units in which output is measured can reverse the authors' conclusions. While I did not check, I suspect that Hulten and Gordon measure real output for their multifactor productivity calculations in units other than consumption, in which case I believe adjustment to real output is called for. To continue to maintain that Hulten (and Gordon, too) made a conceptual error, the authors need to rule out this scenario.

**Other Questions**

1. The authors adopt a particular model of variable capital utilization. Which model is adopted may matter for productivity computations, and so it is important for the authors to motivate the particular model they choose. Their model specifies that there is a trade-off
in production between the services of labor and the intensity of utilization of capital:

\[ y = z(c^k)^\alpha l^{\alpha-1}, \]

where \( c \) denotes capital services, \( k \) denotes the capital stock and \( c \) denotes the intensity with which it is used (sorry for the slight switch in notation from the paper). Under this technology, the same amount of output may be obtained by decreasing labor services, \( l \), and increasing \( c \). The best example of this type of production process, perhaps, is the assembly line. The same number of cars can be produced with fewer hours of labor by speeding up the line. This example is not completely clean, however, since undoubtedly a reasonable specification of labor services would indicate that it increases as the line speed is increased and workers have to work more quickly. Though it's not a particularly good example, one shortcoming of this 'line speed' model is that it may well be the best example there is. Another shortcoming may be that in practice, variations in line speed are simply not an important source of variation in the services generated by capital.

An alternative to the line speed model of capital utilization is the 'workweek' model. This has a long tradition in macroeconomics, beginning at least with Lucas' paper on capacity and overtime. According to this, the services produced by a unit of capital depend on the number of hours it is worked. This assumes, in effect, that there is only one speed at which a unit can be operated, and the only margin available for increasing output from capital during a period, say a week, is to run it for more hours. The workweek model has been used in several papers, including two recent ones by Kydland and Prescott, and Cooley, Hansen and Prescott. A simple version specifies that the workweek of capital is equal to the per-capita number of hours worked:

\[ y = z(hk)^\alpha (hb)^{(1-\alpha)} = z \left( \frac{h}{c} \right)^\alpha (hk)^\alpha l^{\alpha-1}, \ l = hb, \]

where \( h \) denotes hours of work per person, \( b \) denotes number of people employed ('bodies'),
and \( l \) denotes total hours worked. Thus, the difference between the line speed and workweek models, in terms of their implications for the state of sector-neutral technology, depends on \( \left( \delta \right)^{\alpha} \). Measuring \( h \) by average weekly hours from the household survey (Citibase mnemonic LHCH) and \( c \) by the total industry capacity utilization rate (IPX), and setting \( \alpha = 0.36 \), this ratio shows little trend. Thus, going to this version of the workweek model would not significantly alter the dramatic trend feature of Figure 3. On the other hand, it could alter the assessment of the contribution of sector-neutral technology shocks to the business cycle, since the standard deviation of (the log of) this object is a rather large 1.5 percent. But, this does not exhaust the implications of taking a workweek perspective on capital utilization. The Kydland and Prescott assumption that the capital's workweek is proportional to average hours worked assumes there is only one work shift.

My point is that there are alternatives to the line speed model used in the paper, and that which alternative one chooses may matter for the conclusions reached. It is arguable that the alternative to the line speed model, the workweek model, is the currently standard model. But whether it is or not, it is incumbent on the authors to defend their modeling choice. Their line of defense should be to point to empirical evidence that capital utilization is varied in practice more through variations in line speed than by variations in the workweek.

2. I found the discussion of the Domar-Jorgenson model in section III.D confusing. When the authors say (page 20) the Domar-Jorgenson model excludes a role in growth for 'investment-specific technical change' they seem to contradict the argument about equivalence that I made above. But, that just reflects two features of their representation of the 'Domar-Jorgenson' environment: (i) they leave off a 'sector-neutral' technology term from the front of the production function, \( F \), in equation (32), and (ii) they leave off a time subscript on \( \delta \) in that equation. With corrections (i) and (ii), my claim is that what the authors call the Domar-Jorgenson and Solow environments are actually equivalent. I didn't check, but presumably Domar and Jorgenson would agree to corrections (i) and (ii), and if so, I think it is fair to say that the observations in the paper about Domar and Jorgenson
October 5, 1993

Professor Jeremy Greenwood
Department of Economics
University of Rochester
Harkness Hall
Rochester, NY 14627-0156

Re: Ms. 92807 -- "Macroeconomic Implications of Investment-Specific Technological Change"

Dear Professor Greenwood:

This is to let you know that I have transferred the file on your manuscript to my successor as American Economic Review Co-Editor, Kenneth West. All future correspondence should be addressed to Ken at the Department of Economics, University of Wisconsin, 1180 Observatory Drive, Madison, WI 53706.

Sincerely,

John Y. Campbell
Co-Editor

JYC/bwa
August 25, 1993

Professor Jeremy Greenwood  
Department of Economics  
University of Rochester  
Harkness Hall  
Rochester, NY 14627-0156

Re: MS 92807  --  "Macroeconomic Implications of Investment-Specific Technological Change"

Dear Jeremy:

I am sorry you have had to wait so long to hear about your paper. I had great trouble getting referees to write reports; I had to ask five people before getting two reports, and even those were slow. The reports are by referee 3, who has identified himself as Charles Hulten and thinks I should ask for a revision responding to his comments, and referee 5, who is generally favorable but also has some suggestions.

Given these reports I cannot accept this version of your paper for publication. You may want to revise the paper in response to the referees and resubmit, but the risk of this strategy is greater than usual because at the end of September I will turn over my remaining manuscripts to my successor as Co-Editor, Ken West. Ken will have to make the decision on any revision, and I cannot bind him to any particular course of action.

I have read your paper myself and have a few comments. (Keep in mind that they come from a lame-duck Co-Editor). It seems to me that your paper needs a clearer focus. You should try to appeal to a broad readership (not just real business cycle aficionados) and should offer readers a simple way to think about q-shocks: how to measure them, and how they affect the economy. In this spirit, I would favor putting the simple model of the Appendix into the text, and relegating the complex model to an Appendix or even another paper. I also think you should give the reader as much qualitative discussion and intuition as possible for the fluctuations material in the second part of the paper.
More specific comments: 1) Some macro textbooks (e.g., Abel and Bernanke) discuss shifting effective tax rates on equipment and structures as a major reason for the growth in equipment relative to structures. This really needs to be in your analysis. 2) On page 1 you discuss the equipment-to-GNP ratio without saying how this is measured. It is critical that you measure equipment in efficiency units rather than value units. 3) On page 6 you need to say explicitly that adjustment costs are taken out of final output (so that in a sense there are more than "three purposes" for final output).

Sincerely,

[Signature]

John Y. Campbell
Co-Editor

JYC:bwa
Enclosures
August 25, 1993

Professor Finn E. Kydland
Graduate School of Industrial Administration
Carnegie Mellon University
Pittsburgh, PA 15213-3890

Re: Ms. 92807 -- "Macroeconomic Implications of Investment-Specific Technological Change"

Dear Finn:

Many thanks for your careful appraisal of this paper. A second referee was less favorable (I enclose a copy of the other referee’s report, which I think may interest you). Therefore, after giving the paper serious consideration, I have rejected it while giving the author(s) the option to resubmit a revised version.

Sincerely,

John Y. Campbell
Co-Editor

JYC/bwa
Enclosure
REFEE REPORT

Macroeconomic Implications of Investment-Specific Technical Change

Ms. 92807

This paper investigates an interesting, and under-researched issue in economics: the extent to which improvements in technology are embodied in the design of new capital. This issue was actively debated in the 1960s, but a variety of circumstances caused it to be given less attention in subsequent decades: the ambiguity of the empirical results about the size of the embodiment effect, the great difficulty in finding data on vintage specific capital and output, Dension's argument that embodiment had a largely unimportant impact on empirical growth accounts, and the finding that the steady state properties of the embodiment model were not very exciting. Interest in vintage effects picked up after the energy crisis, as a possible explanation of the productivity slowdown, but the new wave of research in this area was prompted by the 1986 paper by Cole et. al. Using price hedonics, this paper argued that "quality" change - i.e., improvements in design - of computers proceeded at the astounding rate of between 10 an 20 percent per year. This prompted a revision of the national accounts, and was followed by the 1990 book by Gordon, which argues that existing accounting procedures were missing quality improvements in all sorts of equipment, at an aggregate rate of about three percent per year (partly as a result of inadequate index number procedures, and in part because of inherent improvements in technology). My 1992 article in the Review takes off from these results by assuming that the Gordon estimates provide an approximate estimate of the extent by which current statistical procedures understate embodied improvements in quality and by merging the Gordon estimates with the conventional sources of growth framework.

The current paper provides an alternative way of achieving this merger, using a very elaborate model of economic growth. This is an interesting and worthwhile endeavor, since it permits feedback effects that my single equation procedures cannot. However, while I am quite supportive of the paper's aims, I also think that there are some major problems that need to be addressed. These include: (1) expositional problems, (2) ambiguities in the meaning of 'investment-specific' technical change, and (3) the misinterpretation of my 1992 Review article and the consequent failure to recognize that my results are much more supportive of theirs than they realize.

(1) The motivation and exposition of the paper needs more work. As it now stands, the exposition is too oriented to the technical details of the model, with too little exposition of the "primitives" or essential assumptions, and too many equations and too much jargon like "Borel sigma algebra." Much of the technical detail should be suppressed into an appendix, and the text devoted to a description, accessible to the general readership of the American
Economic Review, of why and how this approach is superior to, or at least, different from, other approaches (including the single equation growth accounting model). In particular, there should be some mention of how their paper relates to the literature on embodied technological change (the authors seem to go out of the way to avoid the term "embodiment," to the extent that, on page 15, they use the term "incarnated" for "embodied"). Their revised description should also include some comment on the appropriateness of applying steady state analysis to data for the U.S. economy averaged over recent decades.

The motivation for the paper, as presented in the introduction, should also be revisited. The authors start the paper with the assertion that "macroeconomics has stressed technological change as a key determinant of economic growth." This is a hotly debated issue in the productivity literature, and it is my impression that the New Growth Theory of Rebelo-Lucas-Romer puts more weight on generalized capital formation, including knowledge capital. The authors go on to state that "The premise to be entertained here is that technological change is largely investment-specific." But, isn't this the hypothesis to be tested? There are, for sure, abundant examples of embodied technical change, as they note, but there are also abundant examples of non-investment-specific technological change, like quality circles and the just-in-time system of inventory management. If anything, recent research has brought out the fundamental importance of improvements in management and the organization of production as essential sources of productivity improvement.

(2) Based on the discussion on page 14, it appears that "investment-specific technological change" includes two very different models of technical progress: their "Example (a)" is essentially a model of in which "new more efficient capital goods are invested as time progresses," which, in turn, is really nothing more than the model of embodied technical change, while "Example (b)" is a model in which there is technical progress in the production of capital goods, so that they become relatively cheaper over time. Both types of technical change should be clearly labeled and distinguished at the very outset of the paper, because there is an extensive literature on each.

There are also some issue in the way the paper models both types of technical change. In modeling Type B, it would seem natural to use a two (or more) sector model, in order to allow for differential rates of growth of productive efficiency. A single equation representation could be used in which

\[ F(C(t), I(t)/b(t), L(t), K(t), t) = 0, \]

and this would give rise to the Divisia index
\((*)\) \( (1-\sigma(t)) \hat{C}(t) + \sigma(t) \hat{I}(t) - \sigma(t) \hat{b}(t) = (1-\pi(t)) \hat{L}(t) + \pi(t) \hat{K}(t) + \lambda(t) \).

where hats over variables denote growth rates, \(\sigma(t)\) is the share of investment in output, and \(\pi(t)\) is capital's income share. Instead, the authors use a production function of the form

\[ C(t) + I^*(t) = F(L(t), K(t), t) \quad \text{ (page 11, eqn. (14)),} \]

where I have used \(I^*(t)\) to denote the amount of investment deflated using a consumption price deflator, and, for ease of exposition, I have used only one investment good, where the authors distinguish two types. The two forms are equivalent only under special assumptions about the shape of \(F(\cdot)\), and the Divisia index is the more general.

The formulation of Type (a) technical change presents another problem. The authors note on page 18 that "To avoid the problems associated with accounting for improvements in new equipment the following general procedure for data contraction is adopted. ... the corresponding nominal variables from the NIPA are divided through by a common price deflator." This dodges the key issue in the embodied technical change literature: whether or not to adjust the output of new investment goods for quality change. As I note in my article in the Review, the Solow-Fisher view (which is also the Denison view) is that new investment should not be adjusted although capital input should be (Denison does not concur on this point): in this case

\[ F(C(t), I(t), L(t), \Psi(t)K(t), t) = 0. \]

The Domar-Jorgenson view (supported by Cole, et. al., BEA, and Gordon) is that new investment goods should be adjusted for quality change, under the assumption that quality change takes resources to produce: in this case,

\((**)\) \[ F(C(t), \Psi(t)I(t), L(t), \Psi(t)K(t), t) = 0. \]

The debate over the inclusion of \(\Psi(t)\) is of central importance for the steady-state model presented in this paper. Jorgenson has shown that the inclusion of \(\Psi(t)\) in the specification of technology leads to a situation under Golden Rule steady-state growth in which the effects of embodiment on the output of capital just cancels the effects on the input side. In the notation of my Review paper,
\[ \hat{T}(t) = \pi(t) \psi(t) - \sigma(t) \phi(t) + \lambda(t) = \lambda(t) \]

where \( \hat{T}(t) \) is the growth rate of the Solow residual and \( \lambda(t) \) is the rate of disembodied technical change. Thus, on a Golden Rule growth path, all technical change, as measured by the Solow residual, will appear to be disembodied, regardless of the actual rate of embodiment.

These issues need to be sorted out and treated more fully in the paper under review. This is all the more true since it uses Gordon’s estimates of quality change in equipment — which correspond conceptually to \( \psi(t) \).

(3) The interpretation of my paper in the Review. The authors believe that my results on the importance of embodiment (i.e. that it accounts for 20 percent of output growth) to be inconsistent with their finding that it accounts for 60 percent, and attribute this difference to a failure to capture the declining trend in the relative price of new equipment. In other words, I used the specification

\[
G(t) = C(t) + \phi(t)I(t) = F(L(t),\psi(t)K(t),t),
\]

This leads the authors to assert that "... equipment in efficiency units is aggregated one for one with real consumption. Hence, Hulten's specification implies setting the marginal rate of transformation between these variable to one, and marginal cost pricing then implies a constant relative price."

Both assertion are quite incorrect. It is true that I used (***) in the exposition of my model (it's my equation (7)), but I then move immediately to equation (8), which is a variant of the Divisia index (*), and is consistent with (***) and the more general specification (**). All the subsequent analysis uses the Divisia approach, and therefore does not impose a marginal rate of transformation of one between consumption and equipment, nor does it imply a constant relative price. Furthermore, in the discussion of the empirical work on pages 972-973, it should be clear that I used the actual deflators measured by BEA and used by the BLS. That is, I allow for whatever relative price trends the national accountants pick up in the NIPAs, and the Gordon adjustment for quality change is then added to these BEA deflators.
What then accounts for the difference between my estimate of 20 percent and the 60 percent of this paper? The author's seem not to have realized that my results refer to the manufacturing sector, while their's refer to the aggregate economy (or if they did, not to have appreciated the implications for measurement). I used a real gross output measure of product, which includes intermediate inputs, while they use a real GDP measure, which obviously does not. It runs out that this makes quite a large difference. I found that the embodiment factor increased the average annual growth rates of capital stock in manufacturing from 4.37 to 7.28 over the period 1949-83. The reason that this did not have a correspondingly large impact on output is that equipment's share of gross output was only 10.3 percent during this period. This is less than the 17 percent equipment share that the authors use for the aggregate economy, partly because materials, energy and services are a large share of the gross output of manufacturing, and partly because the my data includes other types of capital besides structures and equipment. It turns out that results much closer to the authors' can be obtained by applying "my" procedure to the aggregate level. Indeed, this seems to be the message of the paragraph starting at the bottom of their page 23.

One point that must be noted about the authors' simulations is that they do not appear to take into account residential capital, consumers durables, or public capital. These are sources of capital investment as well, and according to BEA estimates, account for 65 percent of the 1989 aggregate stock of fixed capital. Land and inventories are also omitted. Since the authors focus on the optimal allocation of aggregate output between consumption and investment, and use real GDP as their output measure, the omission of these other types of capital seems incorrect. The extension of the authors' analysis to the other types of capital would change both equipment's income share and, depending on where they got their estimate, the size of the Solow residual, and thus potentially alter their conclusions.

To following table may be helpful in summarizing the differences between my study and theirs:

<table>
<thead>
<tr>
<th>Current Paper</th>
<th>September 1992 AER</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aggregate economy</td>
<td>Manufacturing Sector</td>
</tr>
<tr>
<td>Real GDP (value added)</td>
<td>Real gross output, inclusive of intermediate inputs</td>
</tr>
<tr>
<td>Structures and equipment</td>
<td>All capital input</td>
</tr>
<tr>
<td>Deflates investment with consumption prices</td>
<td>Deflates investment with actual BEA-BLS investment price deflators</td>
</tr>
</tbody>
</table>
Finally, I had some problems with the discussion of x, q, and i at the bottom of page 26. But, I would like to reiterate my general support for the aims of the paper, and hope that the authors will take my comments into account in subsequent revisions. I would be happy to communicate with them directly should they wish to pursue my suggestions (or disagree with my assertions).
February 3, 1993

Prof. Jeremy Greenwood
Department of Economics
Harkness Hall
University of Rochester
Rochester, NY 14627-0156

Re: MS 92807 -- "Macroeconomic Implications of Investment-Specific Technical Change"

Dear Jeremy:

This is just to let you know that I have been having a hard time getting people to referee your manuscript. Three people have now turned me down, and I have just sent the paper out to two more referees. I hope there will be no unusual delays from here, but I thought you should know that we are off to a slow start.

Sincerely,

John Y. Campbell
Co-Editor

JYC:bwa
November 2, 1992

Professor Jeremy Greenwood
Department of Economics
Harkness Hall
University of Rochester
Rochester, NY 14627-0156

Re: Ms. 92807

Dear Professor Greenwood:

Thank you for your manuscript entitled "Macroeconomic Implications of Investment-Specific Technological Change," which you recently submitted to the AER.

I edit the Review with the assistance of several co-editors. The co-editor handling your paper is:

Professor John Y. Campbell
Robertson Hall
Woodrow Wilson School
Princeton University
Princeton, NJ 08544-1013

and you will next hear from him regarding its status. Any inquiries should be directed to Professor Campbell.

Sincerely,

Orley Ashenfelter
Editor

OA/kas
cc: Professor Campbell
October 21, 1992

Professor John Y. Campbell, Co-Editor
American Economic Review
Woodrow Wilson School
Princeton University
Princeton, NJ 08544-1013

Dear Professor Campbell:

I would like to submit the paper "The Macroeconomic Implications of Investment-Specific Technological Change" to be considered for publication in the American Economic Review. Thank you in advance for handling this submission.

Sincerely,

Jermy Greenwood

JG/Imw