Professor Zvi Hercowitz  
Department of Economics  
University of Western Ontario  
London, Ontario  
CANADA N6A 5C2

Re: File No. 468

Dear Zvi:

Enclosed is a referee report on your paper, "Investment, Capacity Utilization and the Real Business Cycle," with Jeremy Greenwood. The referee is encouraging about your paper, but feels it is not sufficiently detailed about its predictions to warrant publication.

Given this report, I regret I cannot accept your paper for publication. At the least, I hope the comments of the referee are helpful to you and that the relatively quick turn-around time has not slowed your research down too much.

Thanks for submitting your work to the American Economic Review.

Sincerely,

John B. Taylor  
Professor of Economics

JBT/aah

Enclosure
REPORT ON
INVESTMENT, CAPITAL UTILIZATION AND THE REAL BUSINESS CYCLE
BY
JEREMY GREENWOOD AND ZVI HERCOWITZ

This is an interesting paper. The authors develop a real business cycle model as an alternative to models developed by Kydland and Prescott (1982) or Long and Plosser (1983). These previous models assumed a technology shock which multiplies the production function thereby affecting the marginal products of labor and accumulated capital. In this paper, however, it is assumed that the shock only affects future capital via a shock to investment. That is, the law of motion for capital is given by

\[ k_{t+1} = k_t[1-\delta(h_t)] + i_t(1+c_t), \]

where \( k_t \) is capital, \( h_t \) is the rate of capital utilization, \( i_t \) is investment, and \( e_t \) is a shock to current investment. The authors argue that their model has an advantage over existing real business cycle models in that the shocks driving this model are more reasonable. The authors argue that it is difficult to think of real world examples of the type of technology shocks that Kydland and Prescott assume, while it is much easier to imagine shocks of the type described here.

I think that this argument has merit and that a model with

\[ \text{References can be found in the paper being reviewed.} \]
this type of investment shock should be explored. The problem with assuming this kind of shock in a standard growth model, as the authors point out, is that consumption and labor productivity will be counter-cyclical. This follows because a shock to investment will raise the real interest rate, causing people to work more and consume less. Productivity will also fall due to diminishing marginal productivity of labor. The authors overcome this difficulty by assuming that capital utilization is an endogenous variable which affects the depreciation rate of capital (see equation (1)). In this case, when there is a positive shock to investment, the cost of replacing capital falls, causing firms to utilize capital at a higher rate. This will increase the marginal productivity of labor, causing productivity to be procyclical. In addition, the higher real wage causes people to substitute away from leisure toward consumption. However, there is still the real interest rate effect described above which could dominate and cause consumption to be counter-cyclical.

This leads to the main point I wish to make in this report: The paper, as it now stands, does not adequately demonstrate that this model is one which generates business cycles as we observe them in actual data. Although this model seems to have potential, the authors never get beyond signing derivatives. A business cycle model should be able to account for things like the fact that investment fluctuates five times as much as output or the fact that consumption is very highly contemporaneously
correlated with output while the capital stock is not. These are the kinds of issues the authors should be addressing.

In attempting to address these issues, the authors will undoubtedly run into some other problems. For example, the capital series generated by this model does not correspond to the measured capital series but is instead a sort of "efficiency units of capital" series. A way around this problem is to let \( k_t = k_{1t} + k_{2t} \) where

\[
(2) \quad k_{1t+1} = (1-\delta(h_t))k_{1t} + i_t^1 \quad \text{and} \\
(3) \quad k_{2t+1} = (1-\delta(h_t))k_{2t} + i_t^2.
\]

With this formulation, \( k_{1t} \) corresponds to measured capital and \( k_{2t} \) corresponds to an unmeasured series which augments the measured capital series.

Another problem with trying to empirically implement this model is that it is unclear what the \( h_t \) series is suppose to correspond to. I don’t have any really good ideas about this but the ultimate success of this model depends heavily on solving this problem.

In summary, I believe that the authors have an interesting idea that should be pursued. The main problem I have with the paper is that I don’t think it is a finished product. The paper needs to include an empirical component where series generated by this model are compared in some manner (eg. estimation or calibration) to actual time series. This is what business cycle research is all about.
March 2, 1987

Professor John Taylor, Co-Editor
American Economic Review
Department of Economics
Stanford University
Stanford, CA 94305
USA

Dear Professor Taylor:

Re: File No. 468

I would like to resubmit the paper entitled "Investment, Capacity Utilization, and the Real Business Cycle" (joint with Zvi Hercowitz and Gregory Huffman) to be considered for publication in the American Economic Review. The referee's main objection about the earlier version of the paper was that it did not include an empirical component and therefore he did not feel it was a finished product. He suggested that the theoretical model outlined in the paper should either be estimated, or calibrated and simulated. We have taken this criticism very seriously. An artificial economy has now been constructed and simulated in the paper. Some summary statistics which emerge from the simulation are compared to the actual ones which characterize US aggregate fluctuations for the 1948-85 period. We found the referee's suggestions to be of a very constructive nature and believe as a result that the revised version of the paper is substantially improved. I hope that this new version of the paper will be satisfactory, and that we have been able to do justice to the referee's thoughtful remarks.

Enclosed are four copies of the manuscript and the submission fee.

Sincerely,

Jeremy Greenwood
(519) 661-3489

JG/ew
Encl.
June 18, 1987

Professor John B. Taylor, Co-editor
American Economic Review
Department of Economics
Stanford University
Stanford, CA 94305
USA

Re: File No. 468

Dear Professor Taylor:

I am writing to follow up on the telephone conversation I had on June 12 with your secretary, Alice Halfersten, concerning the paper entitled "Investment, Capacity Utilization and the Real Business Cycle", by Zvi Hercowitz, Gregory Huffman and myself which was resubmitted to the AER. As I mentioned to Ms. Halfersten, Robert King and Charles Plosser have requested that we submit our paper for publication in a special issue of the JME. I should emphasize that this invitation was entirely unsolicited. In spite of this invitation, my coauthors and I would much rather have the paper published if possible in the AER, given its higher profile and stature in the profession, and also because of the fact that we have already taken some of both your's and a referee's time and effort. Naturally, though, we desire to keep things open with the JME as long as possible in the event that our paper is not accepted at the AER, since the proposed issue of the JME seems like an attractive alternative.

I would like to say that the first referee report we received was both speedy and constructive and we recognize that the revised version of the paper has not been at the AER that long. I hope, though, that you will not mind if I contact you soon by telephone (there is a mail strike looming in Canada) to see if you have any further information on the status of our paper so that we can inform both you and the editors of the JME about our final decision.

Thank you very much for your time and attention regarding this matter.

Sincerely,

Jeremy Greenwood
(519) 661-3489
Professor Jeremy Greenwood  
Department of Economics  
Social Science Center  
University of Western Ontario  
London, Ontario  
CANADA N6A 5C2

Re: File No. 468

Dear Professor Greenwood:

Enclosed are two referee reports on your revised paper. One is from the original referee who recommends publication after only a few small changes. The other is from a new referee who feels there are some technical problems, but basically likes your paper.

Given these reports, I would be happy to publish your paper in the American Economic Review if you can handle the new referee's problem. It seems to me that this issue is sufficiently serious that you would want to address it anyway.

I hope you can send me a revised draft as soon as possible.

Sincerely,

[Signature]

John B. Taylor  
Professor of Economics

JBT/aah  
Enclosures
REPORT ON
INVESTMENT, CAPITAL UTILIZATION AND THE REAL BUSINESS CYCLE
BY
JEREMY GREENWOOD, ZVI HERCOWITZ AND GREGORY HUFFMAN

This paper has been substantially revised and now includes a section reporting standard deviations, first order autocorrelations, and correlations with output of various variables using a particular parameterization of the model. Following Kydland and Prescott (1982), these statistics are compared with the same statistics computed from actual data. However, rather than simulating the model as Kydland and Prescott do, the authors actually compute the exact stationary joint distribution function for the state variables and from this compute the desired second moments of output, consumption, etc., which are functions of these state variables.

The paper now represents an important contribution to modern business cycle theory. However, there are some revisions that could be made that will cause the paper to hang together a little better. In the following, I will discuss the paper section by section and give my recommendations.

The introduction does a good job of motivating the model but does a relatively poor job of informing the reader about what the authors plan to do. In fact, the current version of the introduction does not discuss what the paper is about until the fourth
paragraph. Since the average reader will probably not read beyond the introduction (perhaps not beyond the first couple paragraphs), it is important early on to give the reader a very brief description of the model, how it differs from previous models of the business cycle, and a description of the methodology employed in the paper. The fact that an exact probability distribution is computed for the model is significant and should be advertised. (In fact, the introduction misleads the reader by referring to the exercise carried out in section 7 as a "simulation.") In particular, the fact that this methodology enables one to compare the properties of this model with those of the data and models studied by Kydland and Prescott, Hansen, etc. should be stressed. I think it is possible to revise the introduction along these lines and still keep it within three pages by restructuring some of the existing arguments.

Sections 2 and 3 seem clear and well written. The next two sections on the qualitative effects of an investment shock are also well written, but presented in a way that makes the paper appear to be a collection of disjointed sections rather than a coherent whole. These sections are important to the paper because they provide the reader with intuition on how the model works as well as results that are independent of the specific choice of parameter values used in section 7. However, the paper would fit together better if these two sections were combined into one section with a heading like "qualitative effects of a shock to investment" where these results are presented as a
preface to the quantitative results given in section 7. As the paper is currently written, section 7 is sort of stapled on to the end of the paper disjoint from the rest of the argument. The paper would be improved if the rest of paper was written so as to build up to and embrace this section rather than leave it as an appendage. After all, this is probably the most important section of the paper. This change would be minor, but packaging is very important and this would help the paper flow better and the argument seem tighter.

The next section on investment subsidies is interesting, but does not seem to add much to the argument being presented in the paper. Working out the effects of public finance shocks in real business cycle models is an important research topic, but the analysis offered here does not lead to unambiguous results. This exercise seems to be an ideal candidate for the kind of analysis carried out in section 7. Perhaps it would be best to save this for another paper where the effects of investment subsidies can be worked out more fully. In this paper it would be appropriate to include a paragraph or a footnote describing how investment subsidies could be studied in the context of this model, and then reference this as work in progress. However, I see no reason to devote a whole section to this in order to discuss ambiguous comparative statics results.

I think the last section is the most interesting part of the paper. I have two comments. First, the authors have chosen to use annual rather than quarterly data and to use a different
revising their paper.
Referee Report on
"Investment Capacity Utilization and the
Real Business Cycle" by
J. Greenwood, Z. Hercowitz and G. Huffman

The paper analyzes a standard one sector model of aggregate growth that has two special features: "productivity shocks" affect only newly produced capital and the intensity at which capital is utilized is a decision variable. An alternative specification with an investment tax credit is also analyzed.

I find the basic technology extremely appealing. Specifically, the idea that if a "new" generation of capital goods becomes available (a positive productivity shock) then it is optimal to use intensively the existing capital stock even if this causes "accelerated" depreciation, is very interesting.

In particular, I find remarkable that the numerical analysis of the model (Section VII) shows that it 'behaves' reasonably.

Although favorably impressed, I think that there are some serious technical problems that have not been solved. If I am correct, these problems are such that the paper will have to be substantially revised to be publishable. I divide my comments in two sets: General and Specific. The first ones are, in my opinion, more fundamental.

General Comments

(1) Most of the analysis in sections IV, V and VI (actually, parts of section III too) assumes the value function to be concave. No proof of this
is given, instead the authors refer to an unpublished manuscript by Lucas, Prescott and Stokey. At this time I do not have access to that manuscript. I think, however, that the result is not true. Before showing this let me point out that it is true if the technology was such that \((x,k,k')\) [where \(x = (c,e,h)\)] lies in a convex set (this is true in the standard growth model.) The authors, however, study a model with a nonconvexity that is caused by \(h\). This creates problems because the function \(F(kh,\ell)\) is not concave in \((k,h,e)\), and neither is \(k(1-\delta(h))\) in \((k,h)\).

Next, I provide one proof of concavity of the value function. Because the \(\varepsilon\)'s play no role I will omit them.

Consider the problem

\[
\max_{\varepsilon \in \mathcal{F}} \sum_{t=0}^{\infty} \beta^t u(x_t)
\]

subject to

\[(x_{t+1}, k_{t+1}) \in F(k_0) \text{ given.}\]

In the standard growth model

\[x_t = (c_t, \ell_t, i_t)\] and \(F\) is given by

\[F = \{(x,k,k') : x_1 + x_3 \leq F(k,x_2), k' = k(1-\delta) + x_3\}

\[x \geq 0, k \geq 0, k' \geq 0\]

The set \(F\) is convex. Notice that it is not enough to have convexity given \(k\). The argument below requires convexity of \(F\).

To prove concavity let \(T\) be the mapping whose fixed point is the value function, that is
\((*) \) \((Tv)(k) = \max \{u(x) + \beta v(k')\}\)
\[x,k'\]
\[s.t \] \((x,k,k') \in F\]
\[k \text{ given.}\]

The key is to show that \(T\) maps concave functions into concave functions. Of course, assume \(u\) is concave. Given \(k_1\) and \(k_2\), let \((x_1,k'_1)\) and \((x_2,k'_2)\) attain the max on the right hand side of \((*)\). Let \((k_\lambda,x_\lambda,k'_\lambda)\) be given by
\[\lambda(k_1,x_1,k'_1) + (1-\lambda)(k_2,x_2,k'_2)\]

We need to show that
\[(Tv)(k_\lambda) \geq \lambda(Tv)(k_1) + (1-\lambda)Tv(k_2)\]

Fix \(k_\lambda\), by convexity of \(F\) \((x_\lambda,k'_\lambda)\) is feasible (this follows because \((k_1,x_1,k'_1)\in F\) for \(i=1,2\)). Therefore,
\[(Tv)(k_\lambda) \geq u(x_\lambda) + \beta v(k'_\lambda) \geq \lambda[u(x_1) + \beta v(k'_1)] + (1-\lambda)[u(x_2) + \beta v(k'_2)]\]

where the second inequality follows because \(u\) and \(v\) are assumed concave.

This completes the argument. Consider next the \(F\) set of the paper given by
\[F = \{(c,\ell,h,k,k') \geq 0 : c + k' \leq F(kh,e) + k(1-\delta(h))\}\]
\[k' \geq k(1-\delta(h))\]

The right hand side of the first inequality is not a concave function of the components. Then, in general, this set will not be convex.

\(\triangleright\)

It seems to me that \(\emptyset\) may be concave but that will depend on the degree of concavity of \(u(*)\) and \(-\delta(*)\). (The more concave \(u\) and the less concave \(-\delta\) the better.)
Again, I want to point out that I could be wrong. If not, the analysis of the marginal conditions is incorrect. Moreover, assuming that the solution is interior may be incorrect.

(2) Even if (1) does not apply, I do not find the analysis of Sections IV and V very helpful. First I think it is a little bit misleading to say that they concentrate on the iid case just to emphasize the natural channel of persistence. This is so because (12) and (13) would be impossible to derive if $\epsilon$ entered the right hand side of (7) other than by affecting $k_{t+1}$. I do not think that even assuming that higher $\epsilon$ today results in better distributions tomorrow is enough to sign $dk_{t+1}/d\epsilon_t$. Again I may be wrong but I would like to see a proof (not in the paper but in a note the referee can look at).

In Section IV the authors compare their model to the "standard neo-classical model with constant capacity utilization." Is there a reference? They are not referring to the Kydland-Prescott model because in that model a high productivity shock increases both consumption and investment today. I think they refer to a model that is similar to their model but with $h_t = 1$.

This is not the "standard" model, and I do not think most readers are familiar with it. They should make clear what model they have in mind. Why is it that the shocks result in changes in the interest rate (there are no prices in the planner's problem) and finally why Barro-King is applicable. The bottom line is that I think they should be more careful with their explanations and should be more precise in their language. (What are demand and supply shocks in general equilibrium? If I think of (2) as describing
the production function of another sector that uses what is left over of $k_t$ and $i_t$ to product $k_{t+1}$ is $\epsilon_t$ still a "demand" shock?)

(3) Section VI does not seem to add much to the paper. Why not indicate what are the results in a footnote? If they want to present the results in the main body of the paper they should be careful about the argument that shows that the competitive equilibrium allocation of the distorted economy solves some planner's problem. Some comments (e.g., top of page 17) indicate that the authors are confused. They seem to think that $v$ (the value function of the modified planner's problem) coincides with the value function that competitive agents believe they face. This is not true. In the value function for the agent's problem $\eta_t$ is a different argument and cannot be factored out. (You cannot impose equilibrium conditions before agents maximize. They see themselves as being able to do things that the planner knows are impossible. That they choose not to do them is an equilibrium result.)

The bottom line is that my hunch is that the result is correct but the argument is imprecise. As a side comment I believe that R. Becker was the first one to show the result that there is a planner's problem that "solves" the competitive equilibrium. His paper was published in the *Journal of Economic Dynamics and Control* in 1980 or 1981. He should be given credit.

(4) I assume that $v$ is not concave and, hence, that VI-VI are not necessarily correct. What can be done? I suggest concentrating on the numerical analysis. Specific suggestions are

(a) Report $v(k_j;\xi_s)$ and check concavity (say interpolate linearly between $k_j$ and $k_{j+1}$.)
(b) Because the decision rules are computed, the dynamic response of the system can be computed. In particular, it is possible to simulate the model and thus estimate a VAR of the relevant variables. With the moving average representation easily interpretable (\( \epsilon_t \) is the noise), it is possible to describe the effect of a shock (say an innovation to \( \epsilon_t \)). This approach (or something similar) that relies on the numerical results appears, to me, to be more interesting than the theoretical stuff that is now presented.

(c) I find the last part of footnote (12) unacceptable. The simulation exercise is useful because it is a cheap way of understanding the behavior of the model. The idea that a "good" model is one that generates, for some time series second moments that are "close" (whatever that means) to the estimated moments from the U.S. economy is a debatable criterion, but one that I can live with. However, claiming that the more traditional statistical approach has the problem that it is hard to define what 'fit' is or what metric to use is ridiculous. Ignoring the problem does not eliminate it. In fact what the authors are doing is changing, say, mean square error or Maximum likelihood for their "personal" notion of when two covariances are "close". They should concentrate on explaining why is it that what they do is useful, leaving out their remarks about the traditional approach to confronting the model with the data.
Specific Comments

(1) (page 4) No function that is homogeneous of degree one can be strictly concave. Therefore $F_{11}F_{22} - F_{12}^2 = 0$. This, of course, does not change any of the results.

(2) (page 9, fourth line of Section IV) should read "will now be undertaken".

(3) (page 11) The first equation should be $dF_2(\tau)/d\epsilon_t$ instead of $dF_2(\tau)/d\epsilon_t$.

(4) (page 11). The paragraph following the equation mentioned in (3) just summarizes a well known property of homogeneous functions. It should be a footnote. Also there is a typo. On the second line $F_2(\cdot)/1_t$ should be replaced by $F(\cdot)/1_t$.

(5) (page 20) The symbol $\theta$ has been used for the tax rate. I suggest the authors change it.

(6) (page 21) The standard notation in probability is $P[\epsilon' = \epsilon_\tau | e = e_s] = \Pi_{sr}$ and not $\Pi_{rs}$.

(7) (page 21) Explain the role played by the last equation. Some readers may find hard to realize that it is there to "handle" the first order conditions (8) and (9). Alternatively, eliminate the equation and write down the original problem.

(8) (page 23) In the middle of the page the authors say "If the model possesses a unique asymptotic distribution... then such a relationship must describe a contraction mapping". This is incorrect. Consider a mapping $T: X \rightarrow X$. If $x_0 \in X$, $T^n x_0 \rightarrow x^*$ and $Tx^* = x^*$ it does not follow that $T$ is a
contraction. As a counterexample, consider the following Markov Process 
\[ x_{t+1} = x_t^{1/2} \] with state space \([\epsilon, \omega]\), for \(\epsilon > 0\) but small. The sequence of \(\rho^n\) is just the sequence of Dirac measures (point mass measures) on \(x_n\). This sequence converges to \(\rho^* = \delta_1\). The mapping clearly is not a contraction.

(9) (page 25). The discussion about the value of \(\theta\) is very sketchy. Is the model sensitive to different values of \(\theta\)? The authors should report the results for several \(\theta\)'s.

(10) (page 25) I do not quite understand the criterion for choosing \(\omega\). Is this the only restriction imposed by \(\omega\)? My calculations show that if \(\beta = (1+\rho)^{-1}\) then in the (deterministic) steady state

\[ h^* = \left[ \frac{\omega}{\rho} \right]^{1/\omega} \]

If \(\rho = .05\) and \(\omega = 1.44\), \(h^* = .28\). Is it reasonable to calibrate a model that implies that capacity utilization is less than 30%? The authors claim that there are no reasonable empirical counterparts for \(h\), however, I will be surprised if the ones that are available suggest that it is around 30%. This choice needs to be justified on firmer grounds.

(11) (Page 26) I find strange that the authors determine how fine the grid must be in terms of the second moments of the implied stationary distribution of the endogenous variables. Assuming that descriptizing the state space is an appropriate procedure (I conjecture this is correct when the "true" state space is countable, but I do not know what happens when, as in this case, it is uncountable), the grid should be chosen so that the value of functions converge. I suggest making the value functions continuous by using a linear interpolation between the points and then using the sup norm in the space of continuous functions.
Final comment

I want to emphasize that I think the paper has potential. As I see it, it should have very few theoretical results (maybe just intuition and comparison) and the numerical section must be expanded along the lines of my General comment (4) and the Specific Comments pertaining to it.
Professor John B. Taylor, Co-editor
American Economic Review
Department of Economics
Stanford University
Stanford, CA 94305
USA

Dear Professor Taylor:

Re: File No. 468

Enclosed is a revised version of the paper entitled "Investment, Capacity Utilization, and the Real Business Cycle" by Zvi Hercowitz, Gregory Huffman, and myself. Both referees' reports were extremely conscientious and detailed and we have carefully considered all of their comments in preparing the revision. I would like to take the opportunity here to provide a brief overview of our revision.

The major reservation expressed in the reports about the paper was raised by Referee #2. This concerned the concavity of the value function for the dynamic programming problem presented. In the previous version of the paper we did not prove this proposition, instead appealing to a reference [Lucas, Prescott, and Stokey (1985)] providing a standard argument for establishing this. As the referee correctly noticed, though, the introduction of capacity utilization in our model introduces a potential nonconvexity into the production structure so that the typical line of reasoning does not directly apply here. However, a simple (but not self-evident) transformation of the production function converts our problem into a relatively standard one. A proof of the concavity of the value function, along the lines the referee suggested, is now provided in Appendix A.

Some of the referees' other comments are now discussed.

(1) The introduction and other sections have been restructured in the manner suggested by Referee #1, and we feel the present version is considerably more cohesive. Since neither referee felt that the section on investment subsidies played a central role in the paper, it has been dropped.
(2) Referee #2 is quite correct in saying that, "it is a little bit misleading to say that (we) concentrate on the iid case just to emphasize the natural channel of persistence (in the model)." When shocks follow a first-order Markov process displaying positive serial correlation the sign of \( \frac{\text{dk}_{t+1}}{\text{dc}_{t}} \) is indeed ambiguous, as the referee conjectures. We hope that we now have eliminated any misconceptions conveyed in this regard. The comparative static results based on the iid case are noteworthy because they serve to give the reader a clear picture of the salient operating characteristics of our model, and as Referee #1 states "are important to the paper because they provide the reader with intuition on how the model works..." We believe that the fact that a variable rate of capacity utilization allows for consumption to move directly with investment and labor supply may not be self-evident for many readers. Interestingly, Barro and King (1984) state on this point: "In the simplest formulation of the utilization decision, it is impossible to get current investment and the current utilization rate to move in the same direction." Our paper, however, establishes that it is indeed simple and possible to produce such a relationship. Analysis with more general shock structures requires simulation as we in fact performed for the first-order Markov case, where the results carried through for the particular representation of the economy that was chosen.

(3) Relevant to the formulation of our model is the Barro and King (1982) result that in business cycle models with time separable preferences, consumption will co-vary negatively with investment and labor supply in response to prospective future shocks. As Referee #2 remarked, it was not clear in the previous version of the paper why the Barro and King result was applicable to our model with constant capacity utilization. In Appendix B we now demonstrate that in such a framework a shock to the marginal efficiency of investment will cause consumption to move inversely with investment and labor supply. The results in this appendix may also be of interest to readers familiar with the Barro and King paper since, unlike their work, the argument here is not based on aggregate demand and supply functions.

(4) Referee #1 felt we should better emphasize the difference between our empirical implementation of the model and method utilized by Kydland and Prescott. In particular, he felt that "the fact that an exact probability distribution is computed for the model is significant and should be advertised." From this distribution the autocovariance structure of the model could be completely uncovered, if desired. As was suggested, we have mentioned this in the introduction of the paper and have also now included a computer generated diagram which should serve to highlight better what has been done. Also, Referee #2 thought that convergence in second moments might not be a strong enough criteria when choosing the grid for the capital stock. We have now checked for convergence in the distribution function for capital under the usual \( L^1 \) norm. Using this criteria (which implies convergence in all the model's moments) the chosen grid seems to be sufficiently refined. We have also clarified some misleading exposition regarding convergence to the stationary joint distribution governing the capital stock and technology shock. Our discussion is restricted to finite state Markov chains of the type described in the paper. It is now stated that if the model possesses a unique asymptotic joint distribution function for the...
capital stock and the technology shock, then iterations on the mapping $\rho^1 = \rho^{OP}$ must converge. (This issue is of only minor concern to Referee #2, and may be obvious, but we have enclosed a proof in case he is interested.) Additionally, the notation describing the probabilistic structure of the model has been changed so that it accords with the convention used in statistics.

(5) Referee #1 questions why the behavior of capital utilization in the model is not compared with existing measures in the data. The reason, as noted in the paper, is that existing measures are constructed from relative output levels (i.e., the ratio of actual to an index of potential output, the latter being based upon a "trend-through-peak" procedure), and hence exhibit the same basic pattern as a detrended output series. Additionally, Referee #2 notes that in the calibrated deterministic version of the model the steady-state level of capacity utilization is around .28. This number is seemingly too low. The variable $\tilde{h}$, however, should be interpreted as an index of utilization, and not its absolute level (this interpretation is now clarified in page 4). By changing the units of measurement it could in fact be set at any level. For instance, consider replacing $h$ by $\tilde{h}$ everywhere in the paper, where $c$ is a constant and $\tilde{h}$ is another index of capacity utilization. It is easily checked that the optimum solution for $\tilde{h}$ is given by $\tilde{h} = h/c$, with $h$ being determined as in our paper. Thus by choosing $c$ appropriately, $\tilde{h}$ can be given any value. Note that this transformation does not change the calculated value for $\omega$ or any of the standard deviations, autocorrelations, and correlations reported in the paper.

(6) Last, there is no good guide in the empirical literature for choosing a number for the elasticity of labor supply, $1/\theta$; such estimates are a very controversial subject in labor economics. Therefore as Referee #2 suggested we did some sensitivity analysis around the number we chose, the upshot of which is reported in the text of the paper (to avoid the proliferation of tables).

Both referees provided thoughtful and unusually detailed reports. We hope that their comments have been satisfactorily reflected in the new version of the paper.

Sincerely,

Jeremy Greenwood
(519) 661-3489
Proof [Lucas, Prescott, and Stokey (1985)] Referred to in Point 4

Let $s_t$ be a random variable which takes on $m$ possible values in the set $S = \{ \lambda_1, \lambda_2, \ldots, \lambda_m \}$ for any $m \geq 1$. $\Pi = [\pi_{ij}]$ will be defined to be the $m \times m$ Markov probability matrix determined by

$$
\pi_{ij} = \text{Prob}(s_{t+1} = \lambda_j | s_t = \lambda_i).
$$

Let $\Delta^m$ be the unit simplex (with the $\ell_1$ norm), which is then a complete metric space. We are interested in showing that there exists a vector $p^* \in \Delta^m$, s.t.

$$
\lim_{n \to \infty} ||p^n - p^*|| = 0 \quad \forall \ p \in \Delta^m. \tag{1}
$$

($p$ and $p^*$ are to be interpreted as row vectors). It is straightforward to show that (1) holds if and only if

$$
\lim_{n \to \infty} ||e_i \Pi^n - p^*|| = 0 \quad i = 1, 2, \ldots, m \tag{2}
$$

where $e_i$ is a row vector with 1 in the $i^{th}$ position and zero elsewhere.

Lemma 1: Let $\Pi$ be an $m \times m$ Markov matrix, and for each $j$, let $\varepsilon = \min_{i,j} \pi_{ij}$.

If $\sum_{j=1}^{m} \varepsilon_j = \varepsilon > 0$, then the mapping $T: \Delta^m \to \Delta^m$ defined by $Tp = p\Pi$ is a contraction with modulus $(1-\varepsilon)$.

Proof: Let $p, q \in \Delta^m$, then
\[ \| T p - T q \| \leq \| p \Pi - q \Pi \| \]
\[ = \sum_{j=1}^{m} \sum_{i=1}^{m} (p_{ij} - q_{ij}) \nu_{ij} \]
\[ = \sum_{j=1}^{m} \sum_{i=1}^{m} (p_{ij} - q_{ij}) (\nu_{ij} - \epsilon_{ij}) + \sum_{i=1}^{m} (p_{ij} - q_{ij}) \epsilon_{ij} \]
\[ \leq \sum_{j=1}^{m} \sum_{i=1}^{m} |p_{ij} - q_{ij}| (\nu_{ij} - \epsilon_{ij}) + 0 \]
\[ = \sum_{i=1}^{m} (p_{ij} - q_{ij}) \sum_{j=1}^{m} (\nu_{ij} - \epsilon_{ij}) \]
\[ = (1-\epsilon_{ij}) \| p - q \| \quad \text{Q.E.D.} \]

**Lemma 2:** Let \( \Pi \) be an \( m \times m \) transition matrix, and define \( T: \Delta^m \to \Delta^m \) by \( Tp = p \Pi \). Then equation (1) holds if and only if for some \( k \geq 1 \), \( T^k \) is a contraction.

**Proof:** If \( T^k \) is a contraction then, since \( \Delta^m \) is complete, \( T \) has a unique fixed point, say \( p^* \), and (1) holds.

Conversely, suppose (1) holds. From (2) it follows that

\[ \lim_{n \to \infty} \| \varepsilon_i \Pi^n - p^* \| = \lim_{n \to \infty} \| \Pi_{1^{*}}^n - p^* \| = 0 \quad i = 1, 2, \ldots m \]

(Here \( \Pi^n_{1^{*}} \) is the \( i^{th} \) row of \( \Pi^n \)). That is, every row of \( \Pi^n \) converges to \( p^* \) as \( n \to \infty \). Hence for \( k \) sufficiently large, there exists at least one column \( j \) for which \( \Pi^n_{ij} > 0 \), \( \forall \ i \). Then \( \epsilon_j > 0 \) and \( T^k \) is a contraction of modulus \( (1-\epsilon_j) \).

Q.E.D.
Professor Jeremy Greenwood
Department of Economics
Social Science Centre
The University of Western Ontario
London, Canada
N6A 5C2

Re: File No. 468

Dear Professor Greenwood:

Thank you for the revision of your paper, "Investment, Capacity Utilization and the Real Business Cycle." I have sent the revision back to the original referees and will let you know about a final decision as soon as possible.

Sincerely,

John B. Taylor
Professor of Economics

JBT/jbs
REFEREE REPORT ON
INVESTMENT, CAPACITY UTILIZATION AND THE REAL BUSINESS CYCLE

I consider the current version of this paper to be a significant improvement over the previous versions I have seen. The introduction is clear and well written, the results seem to me to be correct, and the paper is much more cohesive than the previous versions.

One suggestion I have is to elaborate on or eliminate the discussion presented in the last paragraph before the conclusion. There it is pointed out that the labor elasticity of output computed using the model in this paper is equal to .96, which is close to the empirical values reported in Lucas (1970) (see references in paper). In Prescott (1986) (page 24-28 in the working paper version that I have) it is reported that this elasticity turns out to be 1.3 and 1.4 for the Kydland-Prescott and Hansen models. It is interesting that this model produces an elasticity that is so much lower than these others.* However, when Prescott calculates this elasticity he ignores capital, presumably because it varies relatively little over the cycle in these models. In the current model, capital varies a lot—in fact it varies more than output. Thus, when the authors compute the labor elasticity of output taking into account the variability in capital, they get a much lower number than if they had ignored capital.** Given that the

* The authors seem to believe that the interesting fact to be explained is the large value of this estimated elasticity relative to labor’s share of income. This was the question that was puzzling to Lucas in 1970, but for those working with real business cycle models, the puzzle seems to be "why is this estimated elasticity so small?"

** If I compute this elasticity the way Prescott does, ignoring the variability in the capital stock, I get a value of 1.6. This number is simply the covariance between output and hours divided by the variance in
large variability of the capital stock predicted by this model is not observed in actual data, it does not seem reasonable to suggest that this is a model that can account for this estimated elasticity.

hours which can be computed using information provided in Table 1.

I will comment briefly on the changes or lack of them.

1. I was wrong about the nonconcavity of the value function. I did not suggest that a proof of concavity be included. I suggest replacing Appendix A by a footnote that says "notice that \( \Gamma(\kappa, l, e) \) as defined in the appendix) is concave. Therefore, standard dynamic programming arguments establish concavity of the value function."

2. I still think that section IV is too long. I suggest the authors simply present the results (with the equations in an appendix) in the same way they discuss the standard neoclassical model, i.e., last paragraph on p. 9 and first on p. 10. It seems that at least a partial justification for this section is that it highlights some special features of the model. Consider, for example, the statement on page 13 regarding the possible procyclical behavior of consumption and investment. It is pointed out that another version of the model—one with fixed capacity utilization—cannot deliver the same behavior. What about more standard models like Kydland-Prescott? What about the authors’ model? Although this is a possibility, it is never explored in the simulations. I suggest that the properties of the model that cannot be explored analytically be studied via simulations.

3. In my general comment (4) I suggested the authors summarize the results in a different way. Specifically, it was suggested that (maybe adding measurement errors because the system is stochastically singular) they present vector autoregressions fitted to data generated by the model. The reason for doing this is that there is enough evidence generated by the economy that would be interesting to match with their model. Why is it that they do not find this a useful way of summarizing data? I think of this approach as a higher dimensional version of Table 1.

4. In my specific comment (8) I suggested that the discussion regarding the invariant measure was not very correct. In response, the authors sent me a result of Lucas, Prescott and Stokey that is of the form "if A then B." Nowhere is A verified and therefore it is not possible to claim that B holds. Let me explain my argument (this is a minor point and I pursue it just to clarify matters):

   (i) I do not know what an asymptotic distribution is.

   (ii) If the authors mean a distribution \( \rho^* \) such that \( \rho^* = P \rho^* \), the statement is false. The counterexample is given by:

   \[
   P = \begin{bmatrix} 0 & 1 \\ 1 & 0 \end{bmatrix}
   \]

   The unique invariant measure for this process is \( \rho^* \) given by \((1/2, 1/2)\). However, if \( p=(0,1) \) or \((1,0)\), \( pt \) oscillates between \((1,0)\) and \((0,1)\), and it never converges. Notice that \( \rho^* \) is the unique invariant measure for \( P \).

   (iii) If the authors define the asymptotic distribution as \( \lim() \), then the statement is that they assume the limit exists and is unique, in which case it must be a fixed point \( F \rho \). This, of course,
\[
\lim_{t \to \infty} \rho^{opt}
\]

then the statement is that they assume the limit exists and is unique, in which case it must be a fixed point of \( P \). This, of course, follows by continuity. Moreover, uniqueness follows by assumption because if there are two fixed points pick \( \rho \) equal to each of them and the limits (assumed unique) are different.

The bottom line is that in their application they can check if \( P \) is a contraction mapping in which case they get uniqueness (there are other simpler conditions).

5. If \( h \) is an index and one follows the strategy suggested by the authors, is it still possible to get the capital/output ratio right? It seems to me that renormalizing may affect other steady state properties of the model.
Professor Jeremy Greenwood  
Department of Economics  
Social Science Centre  
University of Western Ontario  
London, Ontario  
CANADA N6A 5C2

Re: File No. 468

Dear Professor Greenwood:

Enclosed are two reports on the revised version of your paper, "Investment, Capacity Utilization and the Real Business Cycle," with Zvi Hercowitz and Gregory Huffman. Both referees recommend your paper be published. They have a few more comments which seem helpful. Omitting appendix A might be a good idea.

Given these reports, I am happy to accept your paper for publication in the American Economic Review. It is nicely written and makes an important contribution. Please make the changes suggested by the referees that you feel are appropriate. I am enclosing a style sheet with instructions for preparing a final draft.

When I receive the final revision I will send it on to the main AER office for scheduling and processing.

Sincerely,

John B. Taylor  
Professor of Economics

JBT/jbs  
Enclosures
January 25, 1988

Professor John B. Taylor, Co-Editor
Department of Economics
Stanford University
Encina Hall-Room 426
Stanford, CA 94305-6072
USA

Re: File No. 468

Dear Professor Taylor:

Enclosed is the final version of the paper entitled "Investment, Capacity Utilization, and the Real Business Cycle" by Zvi Hercowitz, Gregory Huffman and myself which has been accepted for publication in the American Economic Review. As you suggested, we have omitted Appendix A contained in the previous version of the paper. Also enclosed are two copies of the computer generated diagram contained in the paper. The larger one is the original diagram, while the second is a reduction of the original on standard size paper. We weren't sure which the ARR would prefer.

I'd like to add that we were very impressed with the unusually high quality and speedy refereeing that we received. Thank you very much.

Sincerely,

Jeremy Greenwood

encls.
Professor Jeremy Greenwood
Department of Economics
The University of Western Ontario
Social Science Centre
London, Ontario
CANADA N6A 5C2

Re: File No. 468

Dear Professor Greenwood:


I have sent the paper to the main AER office for processing and scheduling. You will next hear from the main office about proofs and publication date. Please notify my office of any change of address which may occur between now and the time of publication.

Thank you for submitting your work to the American Economic Review.

Sincerely,

John B. Taylor
Professor of Economics

JBT/jbs