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Kydland-Prescott, Long-Plosser, and King-Plosser: development and cross-fertilization, referees and revision*

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Introduction

There are a number of ways to analyze the developmental process characterizing how *seminal papers* in scientific research programs reach *publication* stage. One is based upon *textual analysis* of *variorum* draft manuscripts. A second is analysis of the *interaction* of papers with *complementary* work in the research field. A third is analysis of the impact of professional colleagues, *such as commentators on drafts, journal editors, and referees*.

In previous work (Young, 2008), the early years of the Kydland-Prescott research program was dealt with in detail by focusing specifically upon the process of what Lucas called putting "all the pieces together" (2005, 777)—including optimal and stochastic growth and recursive competitive equilibrium, culminating in "Time to Build" (1982). Textual analysis of variorum drafts was *supplemented by recollections of the authors* and others, so as to comprehend the evolution of the research program itself, the connection between elements in their approach, such as their critique of control theory, and the linkage between their "time inconsistency"(1977a), "competitive theory of fluctuation" (1978a-d, 1980a) and "time to build" (1982) papers.

This paper deals with how "*cross-fertilization*" of ideas in the work of Kydland-Prescott and Long-Plosser occurred. This is done by surveying the reactions of informal and formal commentators regarding the respective contributions in the form of: (i) comments of colleagues on drafts, (ii) editor's and referee's reports on versions submitted to journals, and the resultant amendments to these drafts and submitted versions, and (iii) cross-citation and presentation patterns of the respective papers themselves.

The paper is divided into three parts. The first focuses on the development of Kydland and Prescott' s 1982 paper from its initial form onwards, and cross-fertilization in terms of the written comments of colleagues and correspondence relating to these comments. The second part deals with the cross-fertilization between Kydland-Prescott and Long-Plosser via a "clearinghouse" for ideas in the form of Fischer Black. The final part utilizes referee's comments and author's correspondence with editors to show how Kydland-Prescott (1982) and Long-Plosser (1983) came to be published in the form that they took. In addition, patterns in cross-citation and presentation of the respective papers, including the almost parallel development of King-Plosser, will also be dealt with in this context.

Dotsey and King presented a survey of "rational expectations business cycle models" which discussed what they termed "the basic real business cycle models" of Kydland-Prescott and Long-Plosser vintages (1987; 1988,3).   A decade later, McGrattan also dealt with "real business cycles" (2006), and noted that the term first appeared in Long and Plosser (1983); however, it is nowhere to be found in Kydland-Prescott (1982), nor in the variorum drafts of that paper (Young, 2008). She went on to say that "the term…was often associated with a methodology", and not necessarily with the "original findings" of Kydland-Prescott (1982), continuing that "the methods of their 1982 paper" have been applied "to study many different sources of business cycles, including monetary shocks" (2006, 1). As shown elsewhere (Young, 2008), what can be considered to be the *earliest* draft of "time to build" (1978a), *according to Kydland himself*, dealt with *both* real shocks and the *possibility* of monetary shocks. Indeed, the published version contained a "sting in its tail" (1982, 1369), which pointed towards analysis of the role of money in "a real model of aggregate fluctuations" (Kydland, 1980; 1981; 1987, 3-10; 1989). The Long-Plosser paper, for its part, it is based *solely* on a real business cycle foundation.   It should also be recalled here that there is no "*precedence issue*" involved when dealing with the Kydland-Prescott and Long-Plosser, *as they are not competitors*, but "alternative", that is to say, *complementary* models for dealing with business cycles (Long and Plosser, 1983, 45, note 8).

However, more is involved here than the "real-money" divide. It should be recalled that Kydland-Prescott is based upon a *one-sector model* and its methodology is *aggregative*. Long-Plosser, on the other hand, is a *multi-sector model*. When placed in the perspective of its subsequent development, as manifest in the King-Plosser (and King-Plosser-Rebelo) research program—characterized by *one-sector model*s—this becomes important due to the associated *loss of analytical "richness"* involved in the movement from a *multi-sector* to a *one-sector* model . Up to now, the evolution of Long-Plosser, and King-Plosser have been overlooked, but this is not the place for a detailed study of its development, as it will be done elsewhere (Young, forthcoming). Rather, here the focus is on the *interaction* between the research programs and papers of Kydland-Prescott and Long-Plosser as manifest in the process of "cross-fertilization" that characterized the nature of their development, that is to say, the **interaction** between the authors, commentators, and referees.

Development and Cross-Fertilization

"Time Inconsistency", "Competitive Theory of Fluctuation", and "Time to Build

The "key example" in their seminal 1977 paper, as Kydland put it, was the "investment-tax- credit example". *What is important to realize is that this example encompasses a "two-period time to build approach"* reflecting "the fact that time is required to expand capacity, and investment expenditures occur over the entire time interval" (1977a, 482), as recognized by both Kydland (2005, interview) and Prescott (2006 d). *This is a crucial point in the evolution of the Kydland-Prescott approach, for it illustrates the inherent linkage between their 1977 and 1982 paper.*

In recent correspondence (2006 d), Prescott has also stressed the dynamic general equilibrium nature of the "Rules vs. discretion" paper. After acknowledging that the investment-tax-credit example did "exploit the rental price of capital theory of Jorgenson and of Jorgenson and Hall", he continued on to say "but what is important is that it exploits the theory developed in "Investment under uncertainty" to derive the equilibrium process given the policy rule. Unlike Bob's [Lucas] "Neutrality" paper, there is capital accumulation so it was truly dynamic. Lucas comes up with the mapping from an investment tax policy rule to

the equilibrium process of the economy …Finn and my analysis introduces maximizing households, so we have a dynamic general equilibrium analysis".

The transitional phase in the development of the Kydland-Prescott approach reached its penultimate stage with the presentation of a paper by Kydland and Prescott at the 1978 NBER conference on rational expectations and economic policy. The story surrounding this watershed paper is *enigmatic*, to say the least. This is because those who attended the conference and commented, or commented in correspondence with the authors on the paper, such as Fischer Black, such as did not realize its significance, as will be seen below; although this is not unique, as the same phenomenon occurred when Muth's original Rational Expectations paper was presented at an Econometric Society meeting in December 1959 (see Young and Darity, 2001) Moreover, the evolution of the 1978 conference paper itself, from its *initial* form, through *the draft presented at the conference,* to its *final published version* in the 1980 NBER Conference volume, is a key element in the "Time to Build" story.

In order to understand the importance of this paper in the *ongoing* intellectual process that culminated in the 1982 " Time to Build" paper, however, we must first turn to how Lucas--and Kydland—respectively perceived what occurred at the conference where the Kydland-Prescott paper on " Stabilization Policy" was given. There are two versions of Lucas's recollections regarding the NBER conference held at the Bald Peak Colony Club, New Hampshire, October 1978 (Fischer, 1980). In his "Professional Memoir", Lucas wrote (2001, 28) " At that conference, Ed Prescott presented a model of his and Finn Kydland's that was a kind of mixture of Brock and Mirman's model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bald Peak model, Kydland and Prescott found that monetary shocks were just not pulling their weight: By removing all monetary aspects of the theory, they obtained a far simpler and more comprehensible structure that fit postwar U.S. time series just as well as the original version. Besides introducing an important substantive

refocusing of business cycle research, Kydland and Prescott introduced a new style of comparing theory to evidence that has had an enormous, beneficial effect on empirical work in the field".

Lucas published " Present at the creation: reflections on the 2004 Nobel Prize to Finn Kydland and Edward Prescott" in the *Review of Economic Dynamics* (2005). Lucas wrote (2005, 777) "The first public presentation of "Time to build..." occurred at an Oct. 1978 conference sponsored by the Federal Reserve Bank of Boston. You might picture the scene as something like the New York appearance of King Kong, when the theater curtain is drawn and the 40-foot ape is revealed, struggling with his chains. But it was nothing like that. The paper …was too hard to be read in advance, and Ed's presentation was technical and confusing". He continued (2005, 778) "I should say that this paper was not the version that was published in Econometrica in 1982. The 1978 version had a kind of nominal wage stickiness, related to my 1972 information-based model (Lucas 1972). This feature is now interesting mainly as evidence that Ed and Finn did not start out by attempting to show that business cycles were real in origin or that monetary influences were unimportant. Their substantive aims at the time were pretty standard. But their methods were brand new, and it was only after much experimentation with the model that they were led to the *discovery* [his emphasis] that the real, technology shocks were doing all the work, and the sticky wage part was contributing nothing".

In his Nobel autobiography, Kydland also recalled events surrounding the 1978 NBER conference paper. As he put it (2005, autobiography): "For an NBER conference in 1978, we wrote a paper that was somewhat schizophrenic. It contained a business cycle model, but also evaluated stabilization policy. The main idea behind the latter was that changes in taxes were costly as a way to balance the government budget over the cycle. Instead the "slack" should be picked up by fluctuations in government debt. In the end, we were asked to reduce the length of the paper for the resulting conference volume published by the NBER in 1980, and we had to leave out much of that material".

Detailed comments on the Kydland-Prescott conference paper were made by Feldstein, Hall, and Taylor, and published in the conference volume (1980, 187-194). The general discussion appearing in the volume also cited comments by Blinder and Nelson, among others.

Nelson, for his part, had perhaps the clearest recollections of the NBER conference, while also making significant comments on the Kydland-Prescott paper. As he recalled (2002): " It was a great conference and I remember the general scene vividly (including noticing that Paul Samuelson was reading the St. Louis Fed weekly newsletter on money supply, and it had his name as addressee on it!). I can't really say that I recall the Kydland-Prescott paper making a splash, but we all had our personal reaction. Mine was that sources of lags they mention, particular time to build, did not seem sufficient to account for the very great persistence of business cycle fluctuations as implied by their AR equation on page 171 [of the conference volume]. My comment is directed to the fact that the sum of coefficients is quite close to unity, so in light of the Dickey-Fuller problem of downward bias (which I was working on at the time in connection with the Nelson-Plosser paper), it is not clear that the sum is significantly below unity. The Nelson-Plosser view was that if it is unity than the cycle is not just long-lived but possibly not stationary. Of course, what we argued in our paper was that the unit root--sum equal to one--could not be rejected, so detrending may be entirely artifactual and the trend process may account for variation that the Kydland-Prescott's simple linear detrending attributes to the cycle. Indeed we argued that perhaps all the variance in output is attributable to trend, leaving no transitory "cycle" to explain".

Above, the term *enigmatic* was used to describe the story of the 1978 Kydland-Prescott paper. Close inspection of the comment by Taylor on the paper published in the conference volume reveals the following anomaly. In his comment, Taylor wrote (1980, 193): "Kydland and Prescott build their equilibrium business cycle model upon the assumption of utility maximization. That is, they posit a representative household utility function which depends on consumption, leisure, and *government expenditures*, and they assume that households maximize this utility function subject to budget constraints" [my emphasis]. However, in the

utility function in the version of the paper as published in the conference volume, *government expenditures do not appear* (1980 a, 174,177). When asked about this, Kydland replied (2006): "We wrote a paper for the NBER conference containing a business cycle model (not unlike, as I recall, that in the paper I had written up in preparation for my "job talk" -- that is, converting my one-year visiting position to permanent -- at CMU that same spring) along with an application to public finance. That application would have shown that fluctuation in the desired provision of public goods, combined with cyclical fluctuations otherwise, implied that the fluctuation ought to be picked up primarily by changing government debt and not by changing tax "rates". In other words, the paper had somewhat of a dual focus (often not a good idea), as reflected also in its title. After the conference, the editor (Stan Fischer, as I recall) told us the paper was too long for the volume and had to be cut. So we more or less omitted the portion emphasizing cyclical public finance (with a heavy heart, because we thought the message was really interesting and innovative). Of course, with that emphasis removed, there was no longer any point in keeping government purchases in the utility function".

In fact, in a letter dated 2 November 1978, Stan Fischer wrote to Ed Prescott commenting "on the Bald Peak paper". In this letter, Fischer focused on what he considered as the problem of "exposition" in the version of the paper as delivered by Prescott at the conference. He wrote:

"My comments are largely expositional, and on details. However, there is one overall comment--namely that it is difficult to figure out where a lot of your results are coming from. I checked the 1977 JPE article, and guess that you've expanded on the model of section V of that article. I wonder whether either you could make the present paper more explicit in the text, or else present parts of the earlier paper in an appendix. I'd have a slight preference to doing it in the text if you can conveniently fit it in; you refer to a number of interesting results that one would like to see proved. Please let me know if you think the model would take up too much space, or if there is any other reason it might be better not to include it. Please call me ... if you have any problems with what I've suggested, particularly if you think it

impossible to have a brief exposition of the model on which you're relying for your conclusions".

And indeed, a "brief exposition" of the 1977 Kydland-Prescott model was "expanded" on and included in the revised version of the paper (1978d), with the section on "financing fluctuating government expenditures" (1978c, section 3, 16-19) elided. However, this "expanded " model also included elements of the model presented in their April 1978 "Persistence" paper (1978a), as will be discussed below.

But more is involved than simply the elision of material from the version of the conference paper as presented by Prescott, and commented upon, by Taylor, among others. There are, in fact, *five versions* of the 1978 Kydland-Prescott conference paper, four drafts (1978a, b, c, d), and the published version (1980 a). Finn Kydland has kindly provided the author with some of these drafts. Kydland has also explained--in an extensive interview (2005, interview)-- their significance and relation to the published version of the conference paper , and the relationship of the conference paper to *other work* in the context of what can be called the "time to build research program", all within the framework of the *overarching* Kydland-Prescott research program.

According to Kydland (2005, interview), the "first draft" of the Oct. 1978 NBER paper, that is to say "time to build", as Lucas put it (2005), was a draft paper by Kydland and Prescott entitled "Persistence of unemployment in equilibrium", dated 19 April 1978 (1978 a). This draft was the basis for the " time to build" research program, on Kydland's view (2005, interview), *as it was the first modern real business cycle paper, in that it was quantitative and encompassed models of people and businesses* (1978 a, 5). The catalyst for this paper was that both Kydland and Prescott were "bothered by 'persistence' based upon rigidities and adaptive expectations" (Kydland 2005, interview). The draft included an explicit time to build feature in "the basic model" (1978 a, 5-6) and a quadratic utility function, in addition to the possibility of monetary shocks (1978 a, 9).

According to Kydland (2005, interview), the April 1978 draft is linked to both the 1978 Kydland-Prescott NBER paper and to the paper by Kydland entitled "Analysis and policy in competitive models of economic fluctuations" (1980). A notable feature of the April draft was that according to the title page it was "preliminary and incomplete" and comprised "Background material for GSIA Seminar, April 19th, 1978". According to Kydland (2005, interview), he gave this seminar, as he was being considered at the time for a tenure track position.

Most importantly, however, is the fact that the model presented in this April 1978 draft was the basis for the model that appeared in the *published* version of the 1978 Kydland-Prescott paper, as will be shown below. There is no government expenditure in the utility function in the April 1978 draft, and "the driving terms of the model are productivity and possibly tastes" (1978 a, 11). The model is in a "dynamic competitive equilibrium" (dynamic general equilibrium) framework (1978 a, 11-13). The "draft" entitled "On the possibility and desirability of stabilization policy", actually had *two draft* versions, the first, a *"preliminary"* version (1978 b), dated September 1978, *and an amended version of the same title actually presented at the NBER conference, also dated September 1978* (1978c). According to the title pages, both were "Prepared for the NBER conference on rational expectations and economic policy, October 13th-14, 1978". The *major* differences between the preliminary version (1978b) and that submitted to and presented at the NBER conference by Prescott (1978c) were the inclusion, in the latter, of a number of important illustrative examples, and the addition of two figures. The final version was dated: "October 1978, revised Dec. 1978" (1978d). According to the title page, the title was changed to "A competitive theory of fluctuations and the feasibility and desirability of stabilization policy". This was also the title of the paper *as published* in the conference volume.

There are a number of differences, both formal and substantive, between the amended September 1978 version (1978c), which *was that presented at the conference*--on the basis of John Taylor's comment, Finn Kydland's recollection, and Stan Fischer's letter to Ed Prescott-- and the October-December 1978 revision (1978d). In addition, there is one minor--albeit

significant--difference between the October-December 1978 revision, and the version published in 1980 (1980 a), in the form of an *additional* reference in a note in the text, as will be seen below.

Moreover, the October-December 1978 revision contained an abstract, absent from the Sept. 1978 amended version, which read as follows: " A competitive theory combining elements of Lucas' (1972, 1975) monetary shock theory with a model of equilibrium capital accumulation, under uncertainty is developed. The model assumes that multiple periods are required to build new capital goods. The resulting equilibrium process displays both the observed co-movements of economic aggregates and observed serial correlation of real output from trend. A conclusion is that the tax rates should be constant over the cycle. This does not minimize fluctuations but does minimize the burden of financing government expenditures".

The difference between the references in the September 1978 amended version, October-December 1978 revision, and the 1980 published version include citation, in the *latter versions*, of published papers by Debreu (1954) and Friedman (1948), working papers by Brock (1978) and Prescott and Mehra (1978), and two 1978 Carnegie-Mellon working papers by Kydland and Prescott, including their paper "Persistence of unemployment in equilibrium". Moreover, to the note (1978 d, 10, 5; 1980 a, 175, note 5) referring to the passage "Ours is a competitive theory which combines the Lucas (1972) monetary shock model with a model of capital accumulation in an environment with shocks to technology", the following was added in the 1980 published version: "Black (1978) has argued that real factors can explain aggregate fluctuations"; more about the reason for this below.

But the *crucial* difference between the September 1978 amended version (1978c) and the October-December 1978 revision (1978d) and 1980 published version (1980a) can be seen in the *model* of the latter versions; a model which also emanates from the Kydland-Prescott "Persistence" paper of April 1978 vintage (1978a), that is, in Kydland's view, as cited above, the *first modern real business cycle paper*. Indeed, as Prescott recalled (2008)" I remember the summer of 1978 when Finn and I figured out how to use the neoclassical growth model

and the growth facts to restrict the parameters of the linear-quadratic economies.   That changed everything."

The *cross-fertilization* aspect of this stage in the Kydland-Prescott story is manifest in the comments provided to them in Feb. 1979 by Fischer Black that dealt with the Oct.-Dec.1978 revised version of the conference paper (1978d). His comments were attached to a letter to Kydland and Prescott dated 13 Feb. 1979. Black started by saying "my paper is listed in your bibliography, but there seems to be no reference to it in the paper". He went on to question the nature of the supportive evidence relating to the importance of effects of monetary shocks on real aggregates, and asked: "Does such evidence exist?" His next substantive comment related to the Kydland-Prescott treatment of plant and equipment, the nature of capital and the capital good, and the time period needed to "build a new capital good" . Black said that on one page, Kydland and Prescott had limited the focus of their model to plant and equipment, while on the next page the model involved "a single kind of capital", and asked: "Which is it?" Black then dealt with their presentation of a "time…to build" notion, and wrote: "You talk about more time being required to build a new capital good. I think it would be better to talk about the 'optimal' length of the production period". Finally, Black said that while Kydland and Prescott talked "about the time between starting and finishing a plant", they also talked "about a time that runs into the period during which the plant is used", and asked: "Isn't the latter more relevant in explaining persistence?" In his covering letter, Black wrote "Here are the comments on your paper. I guess my overall reaction to this draft is that it needs a lot of work".

In his reply, dated 26 Feb. 1979, Prescott wrote

"Thank you for your comments…We had planned to, and in the published version will, cite your paper as another general equilibrium analysis emphasizing the importance of real shock as a cause of business fluctuations. I apologize for the goof up".
He went on to say

"The studies of Barro and Sims were the ones we thought "supportive" of the importance of monetary shocks. This is not to say any theory with real shocks only would imply the absence of the correlations they found".

He continued

"You suggest we should have considered a multi-capital good growth model. In subsequent analysis we plan to build in more features of the economy such as the distinction between capital in the household and capital in the corporate sector. With this paper we had a time deadline and a length constraint so we considered only the simplest examples to get order of magnitude effects and I am confident of our conclusions from a single capital model.

 Making the length of the capital good production period endogenous is something we considered. It would have presented a number of technical difficulties and we knew of no evidence that the construction period of new plants varies much over the cycle. Do you know of any evidence that it does?"

Prescott concluded by saying

"I have found our discussions very useful in clarifying issues. My view is that a good theory of fluctuations should make assumptions consistent with empirical findings in the other applied areas such as labor, finance, growth, etc. Otherwise there is not enough discipline and it is too easy to explain any set of observations.

Thanks again for the comments. Finn and I will be discussing the issues you raised at length".

In correspondence with the author, Prescott also provided detailed retrospective assessments of the history and the impact of the 1978 and 1982 " time to build" *papers*. Because of their significance, they are cited at length here. He wrote (2001, 2002) "the 1978 paper did not have much of an impact. In fact Finn and my 1982 paper did not have much of an impact. At the time ...the only person who thought it was important was Bob Lucas. The big break in Finn and my thinking...came [in 1978] when [we] decided to begin with the growth model with the leisure decision endogenous... [and] to use the growth model to study fluctuations. The beauty of growth theory is the connection between it and the system of

national accounts. Restricting our linear-quadratic economy so that it behaved in the same way as the growth model when not to distant from the steady state seems little in retrospect, but was a major [breakthrough]... At the time we...were convinced that monetary shocks were the cause of business cycle fluctuations… Finn and my paper forced to change my mind. Prior to writing the paper, and finding that the productivity shocks were of the right magnitude and persistence, I was certain that monetary shocks were the factor giving rise to business cycle fluctuations and the problem was to find the propagation mechanism for these shocks. We were searching for a propagation mechanism for monetary shocks along lines suggested by ... Frisch many years before. At the time...Black and...Plosser…[were] the only people I know who would argue that real shocks are all important... Finn and I (1982) were surprised when we found that persistent changes in the factors that affect the steady state of the deterministic growth model gave rise to business cycle fluctuations. Finn and I in this paper broke a taboo against general equilibrium in macro".

In order to fully appreciate Prescott's recollections as cited above, the development of the 1982 Kydland-Prescott paper has now to be dealt with. Moreover, as he is specifically mentioned in the recollections of Kydland and Prescott, and cited in the "time to build" papers, the role of Fischer Black in the story also has to be dealt with (that of Plosser and the development of the Long-Plosser approach will also be considered below).

The manuscript of the 1982 Kydland-Prescott paper was received by *Econometrica* in January 1981, and the revision was received a year later, in January 1982 (1982, 1369). Publication of the paper was announced in the list of accepted manuscripts that appeared in the July 1982 issue of *Econometrica* (1982, 1085), and the paper eventually appeared in the November 1982 issue. Much as been written about the impact of this paper in the form of the subsequent Kydland-Prescott Real Business Cycle research program that emanated from it. However, the development of the 1982 paper itself has not been dealt with.

As in the case of their 1978 NBER "time to build" paper and its final version as published in 1980 (1980 a), the development of the 1982 Kydland-Prescott "time to build" paper is characterized by a number of draft versions and the final version, as published in

1982. Now, the link between the April 1978 Kydland-Prescott paper "Persistence of unemployment in equilibrium" and the 1982 "Time to build" paper can be seen in what Kydland considers(2005) to be the *first* "formal draft "of the 1982 paper , that is, the October 1979 revised draft version entitled " Time to build and equilibrium persistence of unemployment". According to Kydland, this draft was just circulated for comment, "probably to Lucas". The term "equilibrium persistence of unemployment" remained in the title in the Oct. 1979 and September 1980 versions. In the December 1980 version, "equilibrium " was elided, the title becoming "time to build and persistence of unemployment". This was changed to "aggregate fluctuations" *only in the Dec. 1981* revision, *after referee's reports on the paper*, as will be seen below.

What is also important about the October 1979 revised version is that it contains handwritten amendments and additions attesting to the interaction between Prescott, who was then at Minnesota and Northwestern, and Kydland at Carnegie Mellon. For example, Prescott change the original term "the relative demand shift "to" productivity shock" (1979, 3); added "adjustment costs" 1979, 7; 1982, 1348), and a footnote regarding   "beginning of period stocks (1979, 9; 1982, 1349). But more important was the utilization in this version, of an *exponential* "constant relative risk aversion utility function" (1979, 12), which is made *quadratic* in the September 1980 version (1980b, 12). According to Kydland (2005, interview), the September 1980 version should be regarded as the first *complete* version; it was issued as a GSIA working paper (No. 28-80-81) and sent to Cornell for a seminar given by Kydland (2005 , Nobel autobiography). In Kydland's opinion (2005, interview), the September 1980 version *also contained the central message* of the 1982 Kydland-Prescott paper in the form of the sentence "*Our approach integrates growth and business cycle theory*" (1980 b, 2; 1982, 1345); this, according to Kydland (2005, interview) and Prescott (2004, 376, note 1) follows from the 1978 and 1980 versions of the Hodrick-Prescott paper (1978, 1980).

Moreover, the Sept. 1980 version of the paper (1980b) was given by Prescott at the University of Chicago Money and Banking Workshop. While Gary Becker did not attend, Prescott sent him a copy of the paper. In a letter to Prescott dated 24 November 1980, Becker said that he found the Kydland-Prescott draft to be "interesting" and "the right way to go", and was "impressed" by the sample calculations in their paper relating to "how much" of business fluctuations could "be generated from delays in the time to build". Becker went on to make a number of cogent points regarding the outcome of "time to build". He wrote

"As a general matter, it would appear that <u>long</u> delays in building time will tend to moderate cycles rather than contribute to them…Even if one is not sure whether a shock is permanent or temporary, one could discontinue investments if the shock turned out to be temporary when it was anticipated to be permanent. The cost of such discontinuation would be smaller, the smaller the outlays required over the first year or two relative to the total eventual outlay."

Becker concluded

"Of course, if the lags are just of the right length, not too long and not too short, they can, it appears from your paper, produce cycles of the kind we have observed. There must be a nonmonetonic function relating the length of lags to the implied cyclical fluctuations, such that fluctuations rise as lags increase from zero to some value, and then begin to fall. Am I right on some of these inferences?"

Finally, as regards the influence of Black on the development of Kydland-Prescott, while Prescott wrote in correspondence (2002) that he "did not influence my thinking on business cycles", Black's November 1979 MIT working paper entitled "General equilibrium and business cycles" *was* cited in *both* the December 1980 and December 1981 revisions of the Kydland-Prescott "time to build paper", *and* in the 1982 published version. An earlier 1978 version of Black's working paper was cited in the drafts and published version of the Kydland-Prescott NBER conference paper, as seen above. There are actually five versions of Black's paper. The original appeared in April 1978 as an MIT Working paper, which was first revised in September 1978 and later revised in November 1979. A 1978 version of

Black's paper was *not* cited in the Kydland-Prescott NBER conference paper draft dated September 1978, but *was* cited in the Kydland-Prescott draft dated Oct. 1978 and revised Dec. 1978, and also appears in the Kydland-Prescott paper as published in the NBER conference volume (1980 a, 196). Interestingly enough, the third version, that is the Nov. 1979 revision of Black's 1978 paper, *was* cited by them as early as in the September and December 1980 revisions of "Time to build", and *also* in the published version of the "Time to build" paper (1982,1369). The fourth version appeared in August 1982, as an NBER Working paper. The fifth and final version was published in his 1987 book. But *much more is involved here* than citation of Black's paper. For as in the case of their Bald Peak conference paper discussed above, Fischer Black was a *"clearinghouse"* to which Kydland and Prescott sent their draft paper entitled "Time to Build and the Persistence of Unemployment" for comments. Moreover, as will now be shown, he was *the* key player in the process of "cross-fertilization" between Kydland and Prescott and Long and Plosser.

Clearinghouse and Cross-Fertilization

 The Kydland-Prescott, Fischer Black, and Long-Plosser Nexus

Despite their *complementary* development, up to now cross-fertilization between the ongoing work of Kydland and Prescott and Long and Plosser *before* publication of their papers has not been established. Now, documentary material provided by Finn Kydland shows that while the models were *independently discovered*, there was some correspondence *between the authors themselves* at the *draft stages* of the papers, and that they *interacted* with Fischer Black, who played the role of both clearinghouse and commentator, bringing together their ideas, and, as will be seen below, even suggesting, at one point, a *combination* of their respective approaches.

In February 1981**,** Fischer Black sent three pages of detailed comments to Ed Prescott on the Kydland-Prescott draft entitled "Time to build and the persistence of unemployment", which dated from December 1980 (1980c). This was also the version they submitted to *Econometrica*, as will be seen below. Black opened his comments by saying that the Kydland-Prescott paper was "full of insights" and "interesting results". He then said that he

believed that there were "many ways to generate the qualitative features of business cycles", such that a model could be described as one of "competitive", "real", or "general equilibrium" type. According to Black, the Kydland-Prescott paper gave not only "one", but "many examples"; and this, as he said "because some parameters" in their model "did not matter much", this providing "an example for each set" of a parameter values. Black then counter-pointed the Kydland-Prescott approach to that he took in the Nov. 1979 revision of his paper "General Equilibrium and Business Cycles" (1979b). He wrote in his comments: "Another way to do it, I believe, is with adjustment costs. The adjustment cost models you reject are not complex enough to give the kind of behavior you are looking for. For example, in the model in my paper, a key feature is that there are many sectors and the shocks to different sectors are somewhat independent. With that feature, I believe you can generate a complex autocorrelation pattern for output."

In fact, the November 1979 revision of his paper not only was comprised of "many sectors", but was "a multisector model with unemployment" (1979b, 2). Moreover, in the 1979 version, Black clearly explained how "shocks to different sectors" occur. He said "Starting with unemployment at its average level, suppose there is a burst of technological change in certain sectors, so that the values of [the state variable] in those sectors goes up sharply. This will increase utility, but the transfer of resources into those sectors will also increase unemployment in this model" (1979b, 8). In a note to this, Black continued "In a more general model, we might assign a different unemployment cost to resource shifts resulting from good shocks and bad shocks. The job search resulting from good shocks is often done while individual remains employed" (1979b, 19 note 14).

In his comments on the Kydland and Prescott draft, Black continued on to make the very interesting suggestion that illustrated the degree to which "cross fertilization" characterized his views, which advocated the combination of the Kydland-Prescott model with his and that of Long and Plosser. In Black's words:

"Actually, though, I think the best model would be one combining "time to build" and "adjustment costs". And other features, such as those in the Long-Plosser paper, as well. There are virtues to simplicity, but the world is not simple. If we want the model to explain micro behavior and macro behavior at the same time, it will have to have a lot of structure, I think".

After discussing the possibility of utilizing Alchian's treatment of adjustment costs, the Kydland-Prescott definition of the price of capital goods, and their discussion of depreciation, Black returned to compare their approach to his own regarding unemployment, as manifest in his Nov. 1979 paper. He wrote "I think it is useful to distinguish two kinds of unemployment: search-type unemployment and layoff-type unemployment. I take my paper as dealing with search-type unemployment, and yours as dealing with layoff-type unemployment. The two are related, of course, both economically and statistically."

Black, however, was somewhat critical of the Kydland-Prescott treatment of "current productivity" and said: "I can't convince myself that you really need ignorance about current productivity to get your results. Any more than I can believe ignorance about the current money supply is needed. If the decay rate for permanent shocks is higher than you have assumed, is the ignorance still needed? Which features of your model is it crucial to?" On the other hand, Black "especially liked" how Kydland-Prescott dealt with "the intertemporal substitution of leisure" and their "concise discussion of policy".

 Black then posed a series of questions, and made a number of observations, regarding the Kydland-Prescott approach. Among these were the implications of combining the related notions of "time to build" and "cost to build" in a model. In this case, Black wrote "If you have a model where "time to build" and "cost to build" are related, I think you may not have to single out inventories as a special kind of capital. You may be able to have a single kind of capital, or, better, to have many kinds of capital that are all factors of production, but without the need to say which is which". He also posed the questions " How would your model be affected if you used a labor-augmenting productivity shock rather than the kind you use? Or if you used both?"

After stating his belief that "wages are procyclical, at least with longer measurement intervals", and that "wages were certainly low in the depression", he went on to ask: "What is the economics of a negative correlation between the capital stock and output? How is the capital stock measured?...If people slept 16 hours a day and worked 8 hours, would you still use the fractions 1/3 and 2/3 in your utility function? "

Black concluded his comments by saying "Anyway, as you can imagine, I am very enthusiastic about the thrust of your paper. There's no monetary policy, and no fooling with countercyclical tax policy. Just pure, optimal, business cycles ".

Black's comments were the start of a significant cross-fertilization of ideas. In a letter dated February 25, 1981, Prescott sent his responses to Black's comments. In the body of letter he wrote: "Thanks for the extensive comments and the kind remarks on Finn Kydland's and my "Time-to-Build and the Persistence of Unemployment" paper. Enclosed are responses to the comments. I found preparing these comments a very useful exercise for clarifying my views". He continued his letter "Enclosed are two papers. One is a copy of Lucas's "Method and Problems in Business Cycle Theory" paper. I interpret his arguments as supporting our methodology. Enclosed also is Hodrick's and my descriptive investigation of post-war U.S. business fluctuations". Prescott's responses consisted of two pages of detailed paragraph by paragraph replies to Black's comments. In response to Black's comment regarding the importance of the multi-sectoral approach, Prescott replied "Possibly multi-sector models, at least if they represent multistage production, could give rise to more complex serial correlation properties. I suspect, by relabeling variables, our model could be viewed as a multi sector model". Regarding the *Long-Plosser* paper, Prescott wrote ***"I have not yet seen a copy of the Long-Plosser paper. Do they have a model that mimics the behavior of the economy and that could be used to predict the effect of various exogenous interventions?"***.

Prescott then turned to the issue of periods of construction. He said "This is just the Austrian business cycle story. I know of no evidence that construction periods are cyclically variable and even if they are I would be surprised if a model incorporating this feature would behave much differently than ours. Neither Finn nor I know how to incorporate this feature

into a model that can be quantitatively analyzed. Computational costs become essentially infinite". He went on to say: "With constant returns to scale the shadow price of capital is the market price of the firm. In fact stock prices probably vary more than our model predicts. We conjecture that having new technology embodied in new capital might resolve this anomaly as well as the model's overly high correlation between productivity and output. That we are able to discuss issues such as these is, I think, a virtue of our methodology".

After replying to Black's point regarding depreciation, Prescott also dealt with the issue of "search equilibria", and wrote "I always felt search equilibria difficult to analyze. Bob Lucas and I failed in our attempt to extend our equilibrium search paper to environments which would in equilibrium display aggregate fluctuations". Prescott then turn to the problems raised by Black regarding the Kydland-Prescott treatment of " ignorance" about "productivity", and the possible introduction of "cost to build" into their model. He wrote: "Crucial to our model is uncertainty as to future productivities. The auto makers in this country were surprised when they found the demand (i.e. value of output) less than expected. I think the stock market is a very good indicator of the expected present value of future returns upon existing capital. There is no reason why this need move with expected future productivity though I expect a model could be constructed for which it would"
He continued: "To introduce the Austrian-Alchian 'cost to build' construct into a general equilibrium framework necessitates the introduction of many more capital goods. Capital in construction must be indexed not only by periods from completion but also by how long it takes to construct using the chosen technology. Problems of corners arise. It's the computation problem again". Prescott that then turned to the observations made by Black. He wrote "with our model, technological change is neutral and, possibly non neutral technological change would also resolve the anomalies mentioned". He went on to say "the real wage does not move over the business cycle much. Some people argue that it is pro-cyclical and other counter. I would not label the depression a business cycle. My expertise on the depression is limited but my impression is that the real, as opposed to nominal wage did not vary that much in the depression. With respect to growth, the real wage is highly

correlated with real output per capita". He continued " If the capital stock is low, the optimal

decision is to accumulate capital rapidly (high investment). Investment is the most pro-

cyclical component of real output varying in percentage terms four or five times as much as

consumption". Finally, Prescott wrote "Becker in coming up with the 2/3 of time allocated to

leisure activities, does not include sleeping time in the endowment of time. Consequently if

leisure was zero, then I would not include leisure as an argument of utility function; i.e. I

would use 1 and 0 in our utility function".

Two weeks later, on March 9, 1981, Black again wrote Prescott and said "Your replies

are so stimulating that I can't resist another round". In answer to Prescott's assertion that,

under Prescott's assumption of different stages of production, Kydland-Prescott could be

considered a multi-sector model, Black wrote "You have one way to link your model and a

multi-sector model. But what if the sectors represent different consumption goods and

services rather than different stages of production? What if the issue is not "time to build", but

rather "time to complete a shift of resources from one sector to another"? Won't that give the

same kinds of behavior that your model gives?"

Black then turned to the Long-Plosser paper and said "Long and Plosser don't talk

much about exogenous interventions. Neither do you. What kinds of interventions do you

think can be analyzed in your model?"

He then returned to the issue of construction periods and wrote "I didn't mean to suggest

that construction periods be made cyclically variable. I think of the Alchian story as

providing an even-more-micro basis for your story. I did not mean that it would give

different results". Black then counter-pointed his model to the Kydland-Prescott approach and

said "If a change in market value of existing capital is due to physical deterioration in your

model, then I can see why you won't have stock prices varying too much. This is a crucial

difference between your model and the model in my head, some of which is in my paper. In

my multi-sector model with adjustment costs, changes in tastes and technology cause

changes in the market value of existing capital".

He then replied to Prescott regarding "search equilibria", and wrote "Perhaps I'm cheating, but I simplifed the job of analyzing search equilibrium by changing the problem to one with adjustment costs. I think of the cost of changing jobs as arising largely from the cost of search. I do not analyze the details of the search process". Black then replied to the issue of "surprise" and wrote: How does the automakers' surprise fit into a model? As a change in productivity? In my model it is a shift in the allocation of demand across sectors (and possibly across time)".

Black then turned to the relationship between output, investment and capital stock and wrote "I thought you said in the paper that output (not investment) was negatively related to the capital stock. Why would that be? Because investment is high when capital stock is low? But isn't investment high when the capital stock (as measured by stock prices) is high?" Black ended this part of his letter, regarding the utility function by saying "I forgot about weekends and holidays and vacations. Possibly because of some work habits I am trying to change".

Black then turned to Lucas's paper "Methods and Problems in Business cycle Theory". He wrote: " First, this, like almost all his work is sharply reasoned and insightful. This paper disproves his apparent assertion that good theoretical work must involve highly abstract models. Second, though he generally avoids reference to "testing" a model, he does use that word in the second paragraph of his paper. I am coming more to the view that testing is a notion from classical statistics that may be useful in physics, but that Bayesian estimation is more useful than "testing" in economics. Experimental data can be used to test a theory. But time-series data ? "

Black went on to provide comments on the Hodrick-Prescott paper and wrote "Does your method give substantially different results from a method that simply starts with percentage differences of growing variables?" He then asked "Do you have reasons to believe that a business cycle involves 'overshooting' of some kind? Your arguments... seem to imply that you expect a positive deviation at one point to be followed by a negative deviation (not just a smaller positive deviation) at a later point". He went on "is labor productivity observable in a world of uncertainty? I think you might say that you are relating conventional

measures of the variables of interest, rather than the true variables". He then wrote"Again, if you measure the capital stock using market values, I think you will find a positive relation with output, especially when you include lagged values of the capital stock". Finally Black said "I regard your paper [Hodrick-Prescott] as presenting the correlations among certain variables of interest without attributing any causal structure to the correlations. That, I think, is the right way to do empirical work!"

A fortnight later, on March 19,1981--after Plosser had presented the Long-Plosser paper at MIT--Black sent a letter to Kydland regarding the difference between the Kydland-Prescott and Long-Plosser approaches to separatibility and substitutability. Black copied this letter to Prescott, Long, and Plosser respectively. In it, he wrote:

"Charlie Plosser and I were discussing the intertemporal substitution of leisure when he was here to give his paper. He and John Long feel that all you need is a high elasticity of substitution between leisure and goods in consumption. I said that I think you need a non-separable utility function, as you and Ed have." A few days later, on 24 March, Prescott sent a letter to Plosser. Because of its importance to our story, the text of letter is cited in its entirety below [spelling corrected and insertions made for clarity]:

"Thank you for the copy of John Long's and your "Real Business Cycles" paper. I thought the basic question of the business cycle was why the consumption of leisure moved <u>counter</u> cyclically yet the real wage (or marginal product of labor) varies little over the cycle. I thought your crucial statement was on page 27,"If, however, producers substitute between inputs (as relative prices change) less readily than consumers substitute between commodity and leisure consumption, then the above analysis suggest...employment... will be positively associated with commodity outputs... "

First I assume you mean commodity consumption and not commodity output for some output is for investment purposes. Second is this consistent with your assumptions concerning preferences and technology (i.e. (3.1), (3.2), (2.3) and (2.4)) ?. In the single consumption good case, Finn and I were unable to get employment to vary enough over the cycle within your framework. We did tie down the θi by requiring the model to have the right

steady state (in the absence of shocks) commodity consumption and labor supply split. Of course making θ_0 large relative to θ_1 would result in larger variations in labor supply (in percentage terms) but then the resulting steady state would be inconsistent with the data.

I think a special version of your technology is essentially the same as our time to build technology...so I anticipate that you will have no trouble accounting for the serial correlation properties of output.

I also like your technology much better than those which assume adjustment costs. With them, more of the variation in output is associated with variations in productivity rather than in the labor input. To repeat, the fundamental puzzle of the business cycle is why the labor supplied varies so much given the small variation in productivity.

I think you find the extension to the government sector difficult. Once the invisible [hand] fails, computing the equilibrium can become more difficult. Finn has made some progress on this problem in a recent paper. Introducing money is even more difficult.

That for your model sectionally independent shocks to production in each sector result in positive serial correlation in output and positive cross-sectional correlations is interesting. Your random walk example does not satisfy assumption (IX) page 9. It has a high degree of serial dependence. This is not to argue that introducing serial dependence this way is not a good procedure. After all, technological inventions have a persistent effect upon the production possibility set."

On 26 March, Prescott sent Black a copy of his letter to Plosser, and also replied to the "hypothetical test" proposed by Black in his letter of 19 March. Two days later, on 28 March, Prescott again wrote Black, with further replies to the points Black raised in their earlier exchange of ideas, and this time, included a response to Black's comments on the Hodrick-Prescott paper. He first took up Black's point regarding exogenous interventions [Black to Prescott, March 9, 1981] and wrote: "One exogenous change might be a change in the process governing the ability of a small country to transform some good that it produces into goods it consumes via trade. Another exogenous change might be a policy of a balanced budget rather than having it balanced on average over the cycle". Prescott then answered Black's query

regarding "surprise" and said: "I was trying to follow the Chicago principle of loading as much as possible into the constraints and as little as possible into preferences. Due to the increase in the relative price of a complementary product, the output of the auto firms valued in terms of the composite consumption good was smaller. The difference here is more semantic than substantive". He then turned to the relationship between capital stock, investment, and output raised by Black and wrote: "Here I was thinking of the capital stock as the number of machines and the market value of a machine as reflecting the expected present values of its net rental. When capital stocks are low, investment, an important component of output, is high. The market value of existing capital is high because returns on capital are expected to be above average until the capital stock is no longer low. This is what Finn and I found though the variability in the value of existing capital for our model was lower than the observed variability in stock prices. Possibly firms are insuring the returns on human capital and in the boom insurance payments are low. This is consistent with the procyclical movement in capital share".

With regard to Black's comments on the Hodrick-Prescott paper, Prescott wrote: "Differencing nicely eliminates the growth component but it forces one to think in terms of rate of changes rather than levels. It is not at all robust to the measurement errors-at least those that are not highly serially correlated. Further, it tends to eliminate too much of the power associated with the business cycle frequency". He continued: "If people are over (or under) accumulating capital, it is rational for them to offset these errors by accumulating capital at a slower (or faster) rate once the error is recognized. If on the other hand there are no errors, then there is no need to offset past errors. Behavior of inventories suggests there are errors. Our argument against λ [being infinite], is the finding of growth theorists that growth rates were not constant over the period. I would be wary in interpreting the reported impulse response functions as supporting overshooting. The analysis does suggest there may be less persistence than commonly assumed". Prescott ended his letter by saying: "Thanks for the encouragement. The reviews on this paper [Hodrick-Prescott] have been mixed...with most

on the negative side". As a result, Hodrick-Prescott was not published until 1997(on this, see Young, 2008).

Referees, editors, revision, cross-citation, and presentation patterns

Referees, editors and revision: the case of "Time to Build"

As noted above, the Kydland-Prescott paper was received by *Econometrica* in January 1981. This version of the paper [the December 1980 draft] was entitled "Time to build and persistence of unemployment". In a letter to Ed Prescott dated June 26, 1981, Chris Sims, then co-editor of *Econometrica* wrote:

"Both referees like your paper with Kydland "Time to build and the persistence of unemployment". One, however, has some suggestions, which are enclosed. He is particularly concerned about what he calls points 3 and 4. Point 3 is that he is made uneasy by your trend-adjustment procedure. It reflects a general difficulty with the paper: it is clear that in some sense you are giving up at the start on the possibility of fitting all aspects of the time series behavior of the data series you use. (These are quarterly, aren't they? The time unit should be explicit.) This may be the reasonable thing to do, but you should prove provide some indication of what your model misses. You should explain how the drift in your model differs from the observed time series drift. You should summarize in what sense you miss the fine structure of the dynamics. This latter could be done by fitting VAR's to the artificial and actual data and displaying the largest observed differences. Or it could also be done by estimating, say, the first two autocorrelation matrices and drawing attention to the largest differences observed. You may think of other or better ways to accomplish the same thing, but some effort in this direction seems essential to the paper's scientific value.

I assume that the data used are exactly as in Hodrick and Prescott and are given source citations there.

I'll look forward to seeing a revision."

The two referees of "Time to Build" were, in fact, Robert Lucas and John Taylor (personal communication, October 21, 2006). In a letter dated 23 Jan. 1981 to Sims that comprised his report, Lucas wrote:

"I am familiar with the Kydland-Prescott paper, so I can give you my reactions quickly. There is no need to protect my anonymity, though you may if you wish. The paper is very original, both in the business cycle theory it contains and in the way it relates theory and observation. The authors estimate as many parameters as they can from simple sample averages (e.g., factor shares) and informally used "prior" information. Then they fiddle rather unsystematically with remaining parameters to obtain a reasonably close match between certain theoretical moments and the corresponding sample moments. The fiddling is done by a very sophisticated numerical simulation.

The virtue of this procedure is that the model itself is very coherent theoretically, and the data are not permitted to throw estimates into ranges where economic interpretation is difficult or impossible.

I found the paper very stimulating, especially in the direction of trying to guess at the nature of the statistical model which might rationalize this procedure.

I would strongly recommend publishing this."

In his report on paper--as Sims noted in his letter to Prescott dated June 26, 1981--Taylor requested revision and clarification of a number of points. Because of its importance for understanding the process of revision of the Kydland-Prescott paper, Taylor's report is cited at length below, in the context of an analysis of its impact on "time to build". But before doing this, whether Sim's suggestions were taken into account in the Kydland-Prescott revision of December 1981—that became the published version—must be dealt with.

In a new opening to Section 5 of their revised paper--the section re-titled to read "Test of the theory"--Kydland and Prescott addressed some of the points raised by Sims. For example, despite his suggestion, they wrote that they "chose not to test" their model against the "less restrictive vector autoregressive model" (1981, 24; 1982, 1360). On the other hand, they *did* deal with Sims' points regarding the shortcomings in their model, and its treatment of "the

fine structure of dynamics". In the revised subsection titled "Results", they said that one "possible explanation" for problems in the results they obtained could have been "oversimplicity" of their model (1981, 31; 1982, 1365). They went on, as Sims put it in his letter to Prescott of June 26, 1981, " to summarize in what sense" they missed " the fine structure of the dynamics" when they wrote in the revised version "Thus, even though the overall fit of the model is very good, it is not surprising, given the level of abstraction, that there are elements of the fine structure of dynamics that it does not capture" (1981, 32; 1982, 1366).

To return to Taylor, he opened his report on "Time to build and persistence of unemployment" (1981) by stating what he saw as its central message. In his words: "This paper presents a systematic attempt to show that an <u>equilibrium</u> growth model with intertemporal substitution of labor and serial correlation in investment due to <u>gestation</u> lags is capable of fitting U.S. business cycle data fairly well "[Taylor's emphases]. After recommending "that it be published in *Econometrica*", he continued on to say that he had "a number of suggestions or questions which I feel the authors should attempt to address before publication". Taylor then made four major points. The first point dealt with the "form", that is to say, the "specification" of the model utilized by Kydland and Prescott. Taylor questioned the nonlinear specification and wrote:

"Rather than exclusively state their model in linear quadratic form as is common in rational expectations models, the authors of this paper develop their model in nonlinear form and calculate the equilibrium through linear approximations about the steady state. This alternative approach involves considerable calculation and approximations and in some respects it is difficult to get a feel for how accurate these approximations are. The authors state that they do not use a Taylor's series approximation because they want their model to fit well for all points and given realizations. But this leaves one with no measure of goodness of fit of the approximation. At the minimum the authors should indicate why they chose the nonlinear specification and subsequent approximation rather than specify the linear quadratic approach at the beginning. While their approach is unusual for rational expectations

modeling, it is not unusual for conventional econometric modeling and the techniques they develop could potentially be useful in other contexts."

In their Dec. 1981 revision, Kydland and Prescott first dealt with the "Taylor's series approximation" question. In the version submitted, they had written that their "method rather than the second order Taylor series expansion was chosen because we wanted the approximation to hold over the entire range..." In the revised version, this text was elided and replaced by a note. In this, they wrote (1981, 19, note 11; 1982, 1357,note 11) "We experimented a little and found that the results were essentially the same when the second order Taylor series approximation was used...".

In his second point, Taylor wrote:

"Section V of the paper is called "Model Estimation." However, rather than estimate the model, the authors obtained information from other sources as to the likely parameter values. Although this is largely a matter of semantics, it seems to me that this procedure has been called "model calibration" in the literature and the authors might consider such a term in their analysis. For example, those who compute general equilibrium models for the purposes of the evaluating taxes use the word "calibration" in obtaining the parameters of their utility functions and production functions. The use of the word "estimation" in this context does not seem appropriate."

In their Dec. 1981 revision, now entitled "Time to build and aggregate fluctuations", they changed the terminology, as per Taylor's suggestion, from "model estimation" to "model calibration". For example, by calling their attention to the usage of the term "calibration" in other areas of economics, Taylor's "Point 2" brought Kydland and Prescott to write (1981,2; 1982, 1346) " Findings in other applied areas of economics are also used to calibrate the model".

Moreover, the title of Section 5 and subsequent subsection titles were also changed. The first change was made by Prescott, from the original "Parameter Estimates" to "Model Estimation" in the Oct 1979 revision (1979, 24). The term "Model Estimation" appeared as the title of Section 5 in the 1980 drafts (1980 b; 1980 c, 26). The title of Section 5 was then

changed in the Dec. 1981 revision to "Test of the Theory", with added text, and the term "Model Estimation" was changed to "Model Calibration". This also become the title of the first subsection of Section 5 in the Dec 1981 revision and the 1982 published versions respectively (1981, 25; 1982, 1360).

In his third point, Taylor wrote: "In evaluating the model beginning on page 30, the authors use an approach developed earlier by Hodrick and Prescott. Although there might be some advantage to this method of analyzing time series, I feel it is a disadvantage in this paper." Taylor went on to explain why he took this position, and suggested that if the authors did nor agree with him, "then at the least they should provide plots of simulations for some of the major variables. These could then be compared visually with the actual observed time service". Despite this, the Hodrick-Prescott approach was kept as the basis for a time series filtering in the Kydland-Prescott paper, and they did *not* provide "plots of simulations".

As noted above, the December 1980 version of the Kydland-Prescott paper (1980c) was submitted to *Econometrica* under the title "Time to build and persistence of unemployment". In the fourth point of his report, Taylor wrote: "The main objective of the paper as indicated in the title is to indicate to what extent the persistence of unemployment, or more generally, the persistence of deviations of output from secular movements, is due to the this gestation lag in the investment process". And indeed, as Kydland recalled, the change in title of the revised Dec. 1981 version sent to *Econometrica* was to make it, in his words, more general" (2005, interview).

But more is involved here than simply a change in the title of the paper. For, as Taylor continued: "However, the model contains a number of sources of persistence in addition to the gestation source. It contains not only the permanent shocks to technology, but it also contains the substitution between work at different points in time. Some attempts should be made to distinguish between these various components in explaining the persistence of unemployment. The results that are thus far reported make it difficult to determine whether for example, the persistence is due to the gestation lags or to the permanent shocks to

technology. One possibility would be to stimulate the model with the same parameter values for technology shocks and for labor leisure tradeoff over time, but moving the gestation lag to a much shorter time point; at the extreme, have no gestation lag. This would enable one to determine to what extent the gestation lag is important in explaining the persistence of output. This seems particularly important given the title of the paper and the time spent in section II of the paper arguing that the gestation lag model is a more accurate way to describe investment behavior than the more traditional cost of adjustment model".

In their Dec. 1981 revision, which was the version published in 1982, Kydland and Prescott dealt with some of Taylor's "point 4" comments. This was done in the extensive addition of text at the beginning of Section 5 (1981, 23-24; 1982, 1359-60), addition of text in the "Results" subsection, and *completely new subsections* entitled "The Smoothed Series", "Sensitivity of Results to Parameter Selection", and "Importance of Time to Build" (1981, 23-34; 1982, 1360-68). Indeed, the tone of these additions in response to Taylor's suggestions can be seen in the text added at the beginning of Section 5, when Kydland and Prescott wrote (1981, 23-24; 1982, 1360) "Quantitatively explaining the co-movements of the deviations is the test of the underlying theory…so the crucial test is whether the variation in cyclical output arises from variations in employment rather than from variations in labor's productivity".

Editorial role expansion and revision: the Long-Plosser case

As shown above, there was constructive interaction between Chris Sims, the editor of *Econometrica,* and the authors in the Kydland-Prescott case. The revision of the Long-Plosser paper, for its part, was also based upon constructive interaction between the authors and Sam Peltzman, the editor of *The Journal of Political Economy*, whose role was actually expanded at their behest, as will be seen below. The process of revision in this case can be seen in letters from Plosser and Long to Peltzman over the period February-June 1982. The paper was sent to *JPE* on February 16, 1982. In a letter dated April 1, 1982, Peltzman sent his comments, and that of the referee.

On April 9, 1982, Long and Plosser replied to Peltzman and wrote:

"We appreciate your comments and those of the reviewer. We are especially pleased with the excellent turn-around time you provided.

We are writing to you now to express our immediate reaction to the comments and to seek some further advice concerning appropriate revision of our paper".

Long and Plosser summarized the points raised by Peltzman and the referee, and wrote:

"As we understand them, the major comments/ suggestions are:

(1) There is an inadequate link between the model...and the simulation...

(2) In particular, the dynamics...seem to be independent of consumer preferences and utility maximization. The notion of business cycle dynamics resulting from maximizing choice should be emphasized.

(3) Given the causal link between the sections... and the apparent simplicity of the basic theoretical idea(that a production lag combined with any reasonable intertemporal preferences imply smoothing the effects of transitory shocks), the theoretical model... is overdone and should be shortened and de-emphasized relative to the simulation..."

Long and Plosser went on to address these points and said: "As a matter of expositional inadequacy, we fully agree with comments (1) and (2). There is, in fact, an intimate and exact link between the model and the simulation, but we clearly did not to make that link obvious." They continued on to say, however, that "The dilemma we face concerns comment (3). We are entirely willing to attempt some shortening... but we believe that if comments (1) and (2) are addressed, <u>substantial</u> shortening will be inappropriate". They then described the connection between their model and the dynamics of the "model economy". As they put it:

"The link between the model on the dynamics analyzed... is this: Section 3 presents an exact "closed-form" solution for equilibrium quantities and relative prices in a particular example of the type of economy generally described in section 2. [This solution is itself unique. To our knowledge, no one else has ever presented such a closed-form solution without either assuming quadratic preferences and linear technology or approximating actual preference/technology with a quadratic-linear forms.] The joint time-series behavior of

outputs expressed in equation 4.1 (page 26) is not an ad hoc specification. Equation 4.1 is the exact formula for output behavior in the model economy of section 3. Is obtained by directly substituting the equilibrium utility maximizing input rules from Section 3 into the Cobb-Douglas production functions assumed in the section 3 economy. The A matrix is the matrix of exponents from those production functions. The constant vector k determines the "steady state" vector of expected outputs. This vector depends on all of the parameters of the utility function assumed in Section 3 (a point that we were neglected to mention, much less emphasize). Without the detailed model of section 3, the dynamics analyzed in section 4 would be, at best, an ad hoc analogy between observed multi-sector output behavior and the behavior of vector autoregressive processes As a purely descriptive conjecture, this analogy is commonplace and not, in itself, very informative with respect to the economic principles that generate such a process. Our section 3, however, both makes the analogy exact and fully specifies the autoregressive parameters in terms of the preferences and technological parameters of the economy " [emphases of Long and Plossser].

Long and Plosser then gave two additional reasons for their objection to cutting back the sections on their "formal model". They wrote: "The first is a matter of emphasizing economic interpretation. Section 4 characterizes the dynamics of our example economy primarily in terms of its probabilistic properties (e.g., covariance structure, impulse response functions, etc.) and in terms of the correspondence between these properties and alleged regularities in actual business cycles. We regard this as an essential part of the paper but, by itself, it is only a summary of the "outcomes" of the underlying economic forces at work in our model economy. The bulk of our analysis of those underlying economic forces is in sections 2 and 3. The present length of those sections is in fact due to our (perhaps inept) attempt to make them as widely accessible as possible (compare the density of mathematics in our presentation with that in Lucas' 1975 JPE article "Equilibrium Business Cycles"). We fear that a substantial shortening of the sections will make them less readable and/or de-emphasize the economic principles that give rise to "business cycle" behavior"

They went on to say: "This fear would be unwarranted, of course, if the economics of the model were so simply straight-forward that it did require any lengthy discussion. We don't believe that this is the case, however, and this is our final reason for questioning the reviewer's suggestion that presentation of the "basic idea" be substantially shortened. It is not true that (in the reviewer's words) "the one-period production lag, combined with any type of utility function which would imply smoothing across time and commodities, will generate the desired result". As we point out in the paper, smoothing behavior that would occur in response to shocks at constant relative prices may not occur in equilibrium when one accounts for the changes in relative prices induces by the shocks. An immediate and extreme example of this is a special case of our example (Sections 3-4) economy in which labor is the only input in production. In that case shocks are not smoothed across either time or commodities even though the opportunity to smooth is available and our representative consumer would smooth at constant relative prices. It takes a rather close examination of the basic "smoothing" idea to usefully characterize the combinations of preferences and technology that lead the kind of fluctuations observed in actual business cycles (see, for example, our conclusions about labor employment, pp. 21-22). We believe that our analysis of this issue in sections 2-3 contains some non-obvious implications of the "basic idea" that are not evident in the discussion of dynamics in section 4" [emphases of Long and Plosser].

Long and Plosser then turned to Peltzman to ask for his assistance in the revision process. They wrote: "We are most anxious to work with you in revising our paper. We would appreciate your reaction to our comments and welcome any additional advice you could offer as to how we might go about preparing a revision that you would find acceptable."

Two months later, on June 15, 1982, Long and Plosser sent Peltzman their revised version of the paper. In the covering letter, they wrote:

"We have done a considerable amount of rewriting and reorganizing in an effort to shorten and improve the manuscript. Overall, we managed to shorten the paper by over 25%. Specifically, with have made a number of changes in response to your comments and those of the referee:

(i) Section 2, which previously discussed the "general model", has been substantially shortened from over 8 pages to 4 pages. As now written Section 2 quickly lays out the major assumptions and notation of the model as a minimal introduction to the example worked out in Sections 3 and 4.

(ii) Section 3 has been shortened by eliminating some redundant and tangential passages...

(iii) The logical link between the utility-maximizing allocation rules (page 11) and the dynamic behavior of outputs has been emphasized... We have also explicitly noted (page 17-18) how and where the utility parameters appear in the stochastic difference equations governing outputs.

(iv) Section 4, which presents the simulation of the example, has not been significantly shortened, but some minor stylistic changes have been made".

They ended their letter by saying: "We believe that the revised version of the paper is a better and more economical exposition of our basic ideas and we hope that you will agree. The suggestions made by you and your referee were most helpful"

As seen above, then, the interactions between authors, referees and editors in both the "Time to build" case, and the "Real Business Cycle" case were instrumental in bringing the papers to the form which they took in publication, and which brought about the respective progressive research programs. The Long-Plosser, case, however deserves special attention due to its "metamorphosis" from a multi-sector to one-sector approaches in the form of the King-Plosser and King-Plosser-Rebelo models; but detailed analysis will be presented elsewhere (Young, forthcoming). Below, we will only survey citation and presentation patterns of "Time to build", "Real Business Cycles" and the King-Plosser paper in its variorum versions.

Kydland-Prescott, Long-Plosser, and King-Plosser: cross-citation , presentation patterns and impact

Citation analysis is usually applied to assess the impact of a paper on the respective field of economic inquiry and on the economics profession as a whole. An analysis of *cross-citation*, for its part, can provide some information regarding the *interaction* between research programs and associated papers. The pertinent case here is that of the relationship between the *published* versions of Kydland-Prescott (1982), and Long-Plosser (1983), as there was indeed *cross-citation--albeit of unpublished Working Papers*. Kydland-Prescott (1982, 1368 note 17; 1370), for its part, refers to a November 1980 University of Rochester Working Paper version of Long-Plosser. Long and Plosser (1983, 44-5; 69), for its part, cites a "September 1981 Carnegie-Mellon Working Paper" version of Kydland-Prescott. Interestingly enough, the ostensible "title" of the Sept. 1981 Kydland-Prescott Working Paper cited by Long-Plosser *was actually that of the 1982 published version of Kydland-Prescott*, that is " Time to build and aggregate fluctuations". But, as shown above, the version of Kydland-Prescott received by *Econometrica* in January 1981 was "Time to build and the persistence of unemployment", which was the title of the December 1980 version of Kydland-Prescott (1980c). The December 1981 *revision* of Kydland-Prescott, received by *Econometrica* in January 1982 (1982, 1369) was submitted with the *revised* title, which appeared in the published version of the paper. *Up to now, this author has been unable to find any Sept. 1981 version of Kydland-Prescott.* In other words, the version of the Kydland-Prescott paper cited in the published version of the Long-Plosser paper, that is to say "Time to build and aggregate fluctuations", is *most probably* the December 1981 revision submitted to *Econometrica*.

Long-Plosser, for its part, was presented at a number of places, including Wisconsin, USC, Stanford, Washington, Carnegie-Mellon and MIT (1983, 39), and elicited the written comments of Fischer Black, as seen above, and the *November 1980* Long-Plosser Working Paper was also prominently cited in versions of Kydland-Prescott and King-Plosser, as will be shown below.

The earliest version of King and Plosser was their 1981 Rochester Working Paper entitled "The Behavior of Money, Credit and Prices in a Real Business Cycle" (Rochester, 81-8). The paper was presented in Spring 1981 at a Seminar at Rochester, and was also presented at Chicago and Penn. In June 1981, the paper was also presented at a meeting at Konstanz. In February 1982, the King-Plosser paper appeared as an NBER *Working Paper* (Number 853). After describing their model as "a stochastic growth model with a single final product" (1982,4), King and Plosser referred to the Nov. 1980 Long-Plosser Working Paper in the following terms (1982, 4): "Stochastic growth models of a more general variety have recently been employed to study business cycles by Long and Plosser (1980)". Their NBER Working Paper also referred to Nelson and Plosser, *JME*, as "forthcoming". The King-Plosser paper was published under the title "Money, Credit and Prices in a Real Business Cycle" in the June 1984 issue of *AER*. In the *AER* version, the published versions of Kydland-Prescott and Long-Plosser *were* cited, but *not* the Working Papers of either.

As for impact of the Working Papers, and their interaction, two examples should suffice. The first involves the relationship between Prescott-Mehra and Long-Plosser. In December 1977, Prescott and Mehra circulated a draft paper entitled "Recursive competitive equilibria and capital asset pricing" (Prescott and Mehra, 1977). In 1978, this paper was revised and given the title "Recursive competitive equilibrium: the case of homogenous households", and circulated as a Columbia University Graduate School of Business working paper. The paper was eventually published in the Sept.1980 issue of *Econometrica*. The 1978 draft of the paper was received in December 1978, and the final corrected version in December 1979 (1980 a, 1378). In their 1977 draft, Prescott and Mehra further developed the recursive competitive equilibrium framework originally presented in Lucas and Prescott (1971). Moreover, they extended it to the analysis of the cases of "many consumers" and "small fluctuations in aggregate output". In the former case, their analysis was of "an economy with many consumer classes, where each class has different preferences, but the same discount factor" (1977, 21). The latter was an analysis of the case where "fluctuations in aggregate

output are but a few percent"(1977, 22). They concluded (1977, 23) "These difficult and important extensions and applications will be the subject of future inquiry within our recursive competitive equilibrium framework".

The former case is an important one, since--based upon the 1977 draft, as cited above--if all agents have the same discount rate, and if conditions satisfy that a competitive equilibrium Pareto Optimum is ensured, then, as Prescott and Mehra later wrote (1980 a, 1365) "equilibrium processes for economic aggregates and prices [for some heterogeneous consumer economy] will be observationally equivalent to those for some homogeneous consumer economy". Moreover, that fluctuations in aggregate output, is mentioned in their 1977 draft is also significant, although there is no mention of this in the 1980 version. The importance of the 1977 and 1978 drafts of Prescott and Mehra lies not only in it being the linkage between Lucas and Prescott (1971) and their 1980 paper, but because of their impact, it would seem, on Long and Plosser's 1983 paper, that is to say, on the *1980 draft* of Long and Plosser, as will be shown below.

In their 1983 *JPE* paper, Long and Plosser wrote (1983, 43 note 4): "The model we employ is quite similar to the model described in Prescott and Mehra (1980). Their remarks (p. 1365) about the identical consumers assumption (i.e. it is not quite as restrictive as it may appear) and their treatment of the optimality of competitive equilibrium are particularly relevant. They do not, however, explicitly consider the business-cycle implications of their models". In correspondence with Mehra (2005 a, b, c), he acknowledged that Long had seen the 1977 version of Prescott-Mehra at a Rochester "job seminar" that took place in "late 1977 or early 1978". The Prescott-Mehra paper was published in Sept. 1980. Now, the earliest citation of what was to become the Long-Plosser paper this author has found dates to 1980, as cited in the *August 1980* draft of the now classic "Two Charlie's paper" (Nelson and Plosser, 1980). This implies that unless either Long or Plosser, or both, were referees of the 1980 Prescott-Mehra paper-- which is doubtful-- it can be surmised that Long and Plosser most probably extended the 1977 and 1978 Prescott and Mehra approach.

As for the second example, *all three* Working Papers—Kydland-Prescott, Long-Plosser, and King-Plosser—were cited in the *December 1981* revision of Nelson and Plosser's Working Paper "Trends and Random Walks in Macroeconomic Time Series: some evidence and implications" ["Two Charlie's Paper"]. The Long-Plosser WP was cited as dating from 1980; the Kydland-Prescott paper was cited as a 1981 Carnegie-Mellon WP with the title "Time to build and the persistence of unemployment"; the King-Plosser paper was cited as a 1981 Rochester WP [81-8] with the title as above, that is to say. "The Behavior…" *And exactly the same citations appeared in the published version* of "Two Charlie's" (1982, 161-62); but the tale of "Two Charlie's" is another story….

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