

**The 2004 Nobel Prize in Economics**

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November 14, 2004

Prepared for the November 14-15, 2004 meeting of the Shadow Open Market Committee.

## **1. Introduction**

As all readers will probably know, the 2004 Nobel prize in economics<sup>1</sup> was recently awarded to Finn E. Kydland and Edward C. Prescott, the official citation being “for their contributions to dynamic macroeconomics : the time consistency of economic policy and the driving force behind business cycles.” The main purpose of the discussion below will be to outline in more detail the nature of their contributions, which are often misunderstood, but before turning to that discussion I would like (admittedly because it pertains to my own school) to comment briefly on their intellectual pedigree.

In particular, I would like to emphasize Kydland and Prescott’s past and present affiliation to Carnegie Mellon University (CMU), known prior to 1967 as the Carnegie Institute of Technology. Both received their Ph.D. degrees from CMU’s Graduate School of Industrial Administration (GSIA),<sup>2</sup> Prescott in 1967 and Kydland in 1973. After completing his doctoral studies, Prescott spent about 5 years at the University of Pennsylvania and then returned to GSIA for nine years (1971-1980) as a prominent member of the faculty.

Kydland, who was a student of Prescott’s, spend about five years in Bergen, Norway, and returned to GSIA in 1978, where he has been a faculty member ever since.

In this regard, it is interesting to note that the most recent Nobel award in macroeconomics (if one classifies Robert Mundell’s 1999 award as being in international economics) went to Robert E. Lucas, Jr., in 1995. This is significant since Lucas, too, was a professor at GSIA during the years in which he conducted the research for which his Nobel prize was awarded. In fact, Lucas was one of Prescott’s teachers—not his dissertation supervisor, but arguably his strongest influence. Before Lucas, only four economists had

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<sup>1</sup> Officially, the Bank of Sweden Prize in Economic Sciences in Honor of Alfred Nobel.

<sup>2</sup> Now the Tepper School of Business.

received Nobel awards for work in macroeconomics,<sup>3</sup> namely, Milton Friedman (1976), James Tobin (1981), Franco Modigliani (1985), and Robert Solow (1987). Remarkably, Modigliani also was a GSIA faculty member during the years in which his Nobel-cited papers were written.<sup>4</sup>

Let us turn now to a discussion of the Kydland-Prescott contributions. These were developed most notably in two famous papers, both of which have been extremely influential: “Rules rather than discretion: the inconsistency of optimal plans” (Journal of Political Economy, 1977) and “Time to build and aggregate fluctuations” (Econometrica, 1982). Both rely heavily on the hypothesis of rational expectations, but in other respects represent quite distinct topics.

In the first of these papers, Kydland and Prescott (K&P) emphasize the desirability for economic performance of arrangements whereby policy makers in a variety of areas conduct policy in a rule-like fashion, rather than via period-by-period reoptimization (which they refer to as “discretion”). The way in which this message is implied by the formal analysis is both subtle and crucial. Formally, what the K&P analysis showed was that in a dynamic context, in which household’s and firm’s expectations about the future are important, it is not desirable for policymakers to choose their policies in a way that is optimal each period, taking existing conditions as given, even when looking into the infinite future. Why? Because when decisions are made anew each period, with no attention paid to the past, they fail to take account of the way in which current conditions have been affected by expectations formed in the past—and fail in this way repeatedly. As described in a previous SOMC paper (McCallum, 2004), better results can be obtained when policy choices are

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<sup>3</sup> If we classify James Meade’s 19?? award as being for international economics.

treated as part of an ongoing process, rather than as a sequence of unconnected decisions (each one apparently “optimal” from the perspective of that period’s conditions). This was a startling result, which has led to an enormous volume of research and much consideration of what the implications are for practical decision making.

In that regard, K&P themselves argued that “the implication of our analysis is that policymakers should follow rules rather than have discretion. The reason ... is not that [policymakers] are stupid or evil but, rather, that discretion implies selecting the decision that is best, given the current situation. [But] such behavior either results in [time-] consistent but suboptimal planning or in economic instability.”

How then should policy be made, according to K&P? In their words, “Our answer is ... that economic theory be used to evaluate alternative policy rules and that one with good operating characteristics be selected.” In addition, “in a democratic society it is probably preferable that selected rules be simple and easily understood, so [that it will be] obvious when a policymaker deviates from the policy. There could be institutional arrangements which make it a difficult and time-consuming process to change the policy rules in all but emergency situations” (1977, p. 487).

In the opinion of many economists, including myself, this is the right practical conclusion. Others have argued, however, that because most decision makers have no way of inalterably committing themselves to future policy reactions, policymakers not constrained by legislation or superior agencies will inevitably move toward the time-consistent but suboptimal form of behavior (that is optimal on a forward-looking period-by-period basis). Policymakers do not even have the option, according to this line of argument, of adhering to

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<sup>4</sup> Two other Nobel awards in economics have gone to persons whose cited work was done at GSIA, namely, Herbert Simon and Merton Miller.

a policy rule because it would not be credible; private agents would know that it would be optimal “today” (given today’s conditions) to revert to the time-consistent but undesirable form of behavior and thus would expect such policy behavior. In my opinion that position is not correct; if the policymaker consistently behaves in a rule-like fashion, private agents will come to understand this and form expectations accordingly. (This is just the hypothesis of rational expectations.) The position that I am disputing is based on reasoning that requires agents’ expectations to be correct immediately after the startup of a new policy regime. That is highly dubious, however; it is only after some time has passed that private agents will understand and believe in a new policy regime. This is one reason for the K&P recommendation that policymakers evaluate alternative policy rules and select one with “good operating characteristics.”

Application of this reasoning to monetary policy has become familiar. The implication is that monetary institutions should be designed so as to encourage a rule-like, process-oriented type of behavior—normally, either by having an independent central bank with a clear monetary rule or by joining a monetary union with such a rule. In addition, there are many applications of the reasoning in entirely different areas of policymaking. Two examples mentioned by K&P involve government policy toward private construction of housing on a flood plain and the protection of patents. Examples in other familiar contexts involve policies for parents’ responses to misbehavior by their children or governments’ responses to hostage-taking by terrorists. Versions are also applicable to many fiscal policy issues.

The second of the seminal K&P papers has been more controversial but is easier to discuss. In it they developed a theoretically coherent general equilibrium model of an economy, at the macroeconomic level, in which prices adjust fully in each period to clear

markets. In many respects the model was similar to the monetary-oriented models of the 1970's by Robert Lucas (1972, 1975). But there were two major differences: the K&P model was quantitatively specified (rather than being qualitative in nature) and it included random “technology” terms that influenced production functions in a favorable or unfavorable manner. The quantitative specification, carefully chosen by K&P by a calibration method to be discussed shortly, turned out to imply that the technology shocks had major effects on fluctuations in real output and labor employment, whereas monetary policy shocks had almost none. Accordingly, they and many other researchers<sup>5</sup> proceeded to eliminate monetary shocks from their models, developing a major body of work that came to be called “real business cycle (RBC) analysis.” The RBC style of macroeconomic modelling became dominant in leading academic circles but was not embraced by most monetary policy analysts since the flexible-price feature implied that central banks could have extremely little influence on real variables (output and employment). Thus, for example, the RBC models would imply that the Fed could not have generated the recession of 1981-83—whereas monetary economists typically believed that the tight monetary policy of 1981 was responsible for generating this recession as a by-product of bringing inflation down from its double-digit level of 1979 to the 4% level of the late 1980s.

Beginning in the mid 1990s, however, economists began to develop models that blended RBC ingredients with slowly-adjusting nominal prices.<sup>6</sup> The resulting models, termed “New Keynesian” by some and “New Neoclassical Synthesis” by others, combined the desirable theoretical rigor of RBC analysis with the additional realism of sticky prices.<sup>7</sup>

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<sup>5</sup> Important early contributions included Long and Plosser (1983) and King, Plosser, and Rebelo (1986).

<sup>6</sup> Important early papers included King and Wolman (1996) and Yun (1996).

<sup>7</sup> Some economists would dispute my judgement that nominal price stickiness is quantitatively important in reality.

They are now the principal vehicles for monetary policy analysis by leading economists both in academia and in central banks around the world. Such models may or may not reflect the RBC hypothesis that technology shocks are the main driving force that generates cyclical fluctuations in output and employment in actual economies.

It remains for me to mention the procedure that K&P developed for giving quantitative dimension to their models. Their approach, which they termed “calibration,” was quite different from the time-series estimation of macroeconomic models that had been employed by most researchers in macroeconomics and monetary economics prior to 1982. With the latter procedure, models would be assigned quantitative parameter values that made them provide a good fit to the ups and downs of quarterly macroeconomic time series. With calibration, by contrast, parameter values were assigned more on the basis of matching long-run average relationships (instead of quarterly fluctuations) and microeconomic findings taken from entirely different types of data sets. The aim was not to match cyclical fluctuations in quarterly time series data, but to develop models that did a reasonably good job of matching realistic values of variances and correlations among a few crucial macro variables. It was recognized that this process would lead to models that fit the time series fluctuations less well, but would be more securely grounded in theoretical relationships and established regularities of a type that could be securely established.

For some years, there was a good deal of methodological warfare between traditional econometricians and proponents of calibration. As time passed, however, many researchers came to appreciate that both approaches are useful; both have their strengths and weaknesses. Today calibration is widely, but not exclusively, used by monetary policy analysts. This is one enduring contribution of the K&P “time to build” paper. The emphasis on macro-

monetary models that are both theoretically well-developed and quantitatively implemented is another. Thus the 1982 paper of Kydland and Prescott, like their 1977 publication, has provided truly major contributions to economic science and policy making.



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