Michael Woodford’s *Interest and Prices: A Review Article*

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1. Introduction

Michael Woodford’s *Interest and Prices* (2003) is an overwhelmingly impressive piece of work. It is enormous in scale, in scope, in its attention to detail, in its command of monetary economics (both theoretical and practical), and in its originality. Does this mean that it will go down as historically significant, as one of the few great treatises in monetary economics? I have no confidence in my ability to predict such a thing—or anyone else’s—but I do believe that Woodford’s book should be regarded as the most important treatise on monetary economics since Patinkin’s *Money, Interest, and Prices* appeared in 1956.

When, at a conference in October 2000, I commented on a preliminary version of Chapter 4, I could not resist quipping that “Woodford seems determined to improve on *Money, Interest, and Prices* by taking out the ‘money’.” That was intended partly as a joke, since his title is presumably a deliberate repeat from Wicksell (1936), which Woodford refers to approvingly and draws upon in numerous places. Also, the analysis is not intended to be applicable only to a non-monetary economy, although it begins with that restriction. The book does quite deliberately de-emphasize the role of money, however, the intention being to express policy behavior in terms of interest rate rules (as would those central bankers who think in terms of rules) and also to construct a body of analysis that would remain valid if technological progress were in the future to eliminate the role of money altogether. In what follows I will refer to this tendency as “anti-monetarist,” so as to have a convenient and evocative term.

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2 To me, incidentally, it seems that there have been very few great treatises in monetary economics. I would regard Patinkin (1956) as one, the relevant portions of Mill (1848) as another, probably Fisher (1912), and perhaps Wicksell (1935), but that is a very short list. Many great contributions have been made, of course, that are not treatises, whereas several ambitious treatises on monetary economics have been unsuccessful.
The main objectives of the work, however, are evidently to develop a framework for monetary policy analysis that is firmly based on dynamic, optimizing, general equilibrium analysis in a stochastic context, while departing from real-business-cycle assumptions by replacing the latter’s presumption of full price flexibility with an optimizing form of nominal price stickiness (i.e., temporary rigidity). Expectational rationality is assumed throughout, with an emphasis on determinacy and some attention to learnability of rational expectations equilibria. The strategy is to develop models that are in principle immune to the Lucas critique, and thus usable for analysis of alternative policy rules, with much concern given to the design of an optimal policy rule—with optimality evaluated in terms of the utility of a typical household. Although the analysis is mostly theoretical, there is considerable attention paid to the conformity of the models’ implications with empirical evidence. Thus the book’s approach is squarely in the spirit of much recent work in monetary policy analysis, represented, for example, by the papers in the NBER conference volume edited by Taylor (1999) and those appearing in the July 1999 issue of the *Journal of Monetary Economics*. The book’s coverage is not, and does not attempt to be, comprehensive. There is, for example, no explicit inclusion of open-economy issues and no discussion of models designed to provide microeconomic foundations for the role of a medium of exchange. Nor is there much attention given to historical matters of either an economic history or history of thought type. The book’s attention to quantitative properties of the various models serves, however, to make it much more than a theoretical exercise.

In what follows, I will begin by attempting to describe the book’s contents on a chapter-by-chapter basis. Then I will discuss at greater length a few specific issues that are raised by Woodford’s analysis; these include the “timeless perspective” approach to policy
rule design, the proper role of determinacy of rational expectations equilibria, Woodford’s innovative view of interest rate smoothing by the central bank, and the book’s emphasis on non-monetary economies. A few brief concluding comments comprise the final section.

2. The Contents of *Interest and Prices*

Woodford’s Chapter 1 is an extensive introductory essay that sets the scene for the seven highly analytical chapters that follow. Its sections emphasize “the importance of price stability,” “the importance of policy commitment,” and the conduct of monetary policy “without control of a monetary aggregate” by means of “interest-rate rules.” These discussions are extremely thoughtful and well argued. Indeed, almost anyone with a serious interest in monetary policy analysis would benefit from a reading of this chapter, I believe, and most would find it enjoyable as well as instructive. Although it includes almost no formal model specification, it develops its arguments in an analytical manner that reflects the presumption (maintained throughout the book) of rational dynamic optimizing behavior by market participants. More will be said about this chapter’s arguments in what follows.

In Chapter 2 Woodford lays out in detail the simplest version of the analytical framework that is employed throughout the book. It involves a general equilibrium analysis of an ongoing, stochastic economy that is inhabited by a large number of households that receive commodity endowments and must make decisions about saving vs. consumption and about which assets to hold, that is, how much money to hold in relation to other financial assets. In the initial exposition, “money” is not a medium of exchange—it provides no transaction-facilitating services to its holders—but is just a liability of the central bank in

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3 An excellent summary of this type is provided in the book itself, on pp. 55-58.
4 I say “almost” because the chapter combines a policy rule with a Fisher identity to create, under the assumption that the real interest rate is exogenous, one very stripped-down model.
5 Later chapters pertain to an economy in which goods are produced, of course.
terms of which prices are quoted. Numerous propositions are developed that pertain to the existence and uniqueness of rational expectations (RE) equilibria under policy rules based on central bank control of a one-period nominal interest rate. In this discussion much emphasis is accorded to “determinacy,” i.e., the uniqueness of RE equilibria. After considerable analysis of the “cashless” economy, Woodford introduces transaction-facilitating money by the device of including (the services of) real balances as an argument of each household’s utility function. He argues that this change does not substantially affect the principal conclusions of the analysis. It does, however, eliminate one feature of the cashless economy with price flexibility, namely, that central bank policy has no effect on the behavior of any variable that contributes to household utility. The presentation in this chapter does not presume knowledge of esoteric mathematical tools, but is highly analytical and detailed. The chapter is 77 pages in length, not counting 30 additional pages of proofs that appear in the appendices. This length is typical for the book; Chapters 2-8 have lengths that average about 80 pages.

Throughout Chapter 2, all prices are taken to be fully flexible—there is no nominal stickiness of any type. But in Woodford’s view nominal stickiness is an important feature of actual economies, so Chapter 3 develops a version of the prominent Calvo (1983) model of staggered price-setting that will be used in the remainder of the book. With only a fraction of all producers able to change their prices in any period, the arrival of any shock that alters the price that would be charged in the absence of nominal stickiness introduces a relative-price misallocation. Each household is assumed to own equal shares of all the firms that produce the many goods present in the economy, and these producers all possess some
monopoly power and set prices optimally in light of that power. Other market arrangements
that yield similar analytical results are discussed. The Calvo model has been extremely
popular in monetary policy analysis during the past 15 years, but in its basic form (which is
entirely forward looking) has implications that seem to be inconsistent with data from
industrial economies. Woodford is sharply aware of this potential weakness, and explores
various extensions of the basic model that go a considerable way toward its mitigation. In
particular, he explores the effects of introducing a delay in the implementation of newly
chosen prices (chosen by the fraction of sellers that reset their prices under the Calvo
hypothesis), which can alternatively be interpreted as having price changes based only on
information available in the past. And, as a second mechanism, he explores the effects of
having those prices that are not re-optimized in a period nevertheless change from the
previous period at a rate governed by either the average inflation rate in the policy regime
under consideration, or else the previous period’s inflation rate.\(^7\)

In a series of calculations
Woodford shows that by incorporating effects such as these, he is able to produce much more
realistic dynamic responses with the amended Calvo model, including responses to monetary
policy shocks (interest rate innovations) that occur with a delay, are hump-shaped, and have
inflation’s peak response occurring later than that for output. Finally, one section of Chapter
3 applies the Calvo staggered-price mechanism to nominal wage-rate adjustments, as well as
prices—as suggested by (e.g.) Erceg, Henderson, and Levin (2000) in a notable study.
Throughout all of this, price adjustments are based on optimizing calculations on the part of
the price setters, subject to specified frictions such as information delays. The results of the

\(^6\) Another form of price stickiness is also discussed, a new-classical specification that features prices set
optimally one period in advance by a fraction of sellers (with the others flexible). It is used mostly as a basis
for comparison and is not drawn upon frequently in subsequent chapters.

\(^7\) The latter modification was emphasized by Christiano, Eichenbaum, and Evans (2001).
various modifications should be considered carefully by anyone who, like myself, has been skeptical about the empirical relevance of the Calvo staggering formulation.

Chapter 4 brings together the staggered price-adjustment specification of Chapter 3 with the general equilibrium approach of Chapter 2 to produce a “Neo-Wicksellian Framework for the Analysis of Monetary Policy.” The basic version can be summarized, as many analysts have noted, in a three-equation model that includes an optimizing IS-type intertemporal relation, an optimizing price adjustment equation, and an interest-rate policy rule to govern the dynamic behavior of inflation, the output gap, and the one-period interest rate. The discussion shows that the non-policy forces affecting output-gap behavior can be summarized in terms of a “natural rate of interest,” the time-varying equilibrium real rate of return that would obtain if prices were fully flexible, thereby reflecting the neo-Wicksellian appellation. If the central bank managed to keep actual short-term nominal interest rates equal to this natural rate of interest there would be no tendency for prices to be adjusted upward or downward, so inflation could be stabilized at a rate of zero. More generally, to a log-linear approximation, the interest rate that the central bank needs to maintain (period by period), to keep inflation each period equal to a low but non-zero target rate, would be that target rate plus the (time-varying) natural rate of interest. This is the same as generating interest rates that keep the discrepancy of output from its flexible-price value equal to zero. Expectations regarding future inflation are relevant, however, so keeping this discrepancy at zero does not suffice to define a satisfactory rule. Woodford accordingly undertakes an extensive exploration of conditions for the determinacy of RE equilibria. A major finding is that some version of the Taylor principle, i.e., that a nominal interest rate policy rule must raise that rate by more than one-for-one in response to any increase in the inflation rate, is
needed for determinacy in a variety of settings. A notable feature of the presentation is that Woodford does not stop with the determinacy analysis, but goes on to examine the (least-squares) learnability of RE equilibria, i.e., whether small departures from a RE equilibrium would be eliminated over time by the behavior of agents who are not miraculously endowed with exact knowledge of the true parameter values describing the economy’s structure, but instead have to learn them by observations on the behavior of the economy’s aggregate variables. This extension will be regarded as important by anyone who, like myself, regards such learnability as a necessary (not sufficient) condition for the plausibility of any RE equilibrium.\footnote{This topic is discussed more fully below, in Section 4.} Next, the Chapter 4 discussion shows how a central bank might achieve inflation stabilization, that is, how to keep inflation equal to a given, constant target value period by period. Although this requires that the interest rate equal the natural rate plus the target inflation value, that equality does not constitute an implementable rule for central bank control of the interest rate. One can be devised, however, and the mechanics are painstakingly worked out. Up to this point Chapter 4 has proceeded in the context of a model in which there is no medium-of-exchange money, so in its third section Woodford extends the model in that respect. As in Chapter 2, but now with sticky prices, he finds little effect of including money balances in agents’ utility functions when the latter are separable in terms of monetary services. Here, moreover, he considers a specification with non-separability, which implies that the IS-type intertemporal optimality condition includes terms pertaining to current and expected real money balances. He attempts a realistic calibration to estimate the magnitude of the effects of this extension of the model, and concludes that such effects are quite small. This conclusion is consistent with the findings of related studies by Ireland (2003) and McCallum (2001). Finally, Chapter 4 includes a section pertaining to
considerations raised by the fiscal theory of price level determination, a topic that I will not attempt to discuss in this review.\textsuperscript{9} It can be said briefly, nevertheless, that most results throughout the book are predicated upon the assumption that fiscal policy is being conducted in a sensible manner, one that seeks to retire (on average) a positive fraction of outstanding government debt in each period.

Chapter 5 introduces several extensions of the basic model of Chapter 4 that are designed to make it more compatible with time series data (specifically, data for the United States). These are not arbitrary, data-fitting extensions but rather complexities in the postulated environment, with agents continuing to behave in an optimizing fashion. One such extension involves an assumption that there exists a time delay such that expenditure decisions must be made in periods prior to their implementation, or the related assumption that expenditure decisions are based only on information from periods in the past. Also investigated theoretically is the hypothesis that consumption decisions involve habit persistence, represented by a utility-function specification in which a household’s consumption in period $t$ is replaced by a function of its consumption in $t$ and also that consumption level in relation to consumption in $t-1$. One section discusses quantitative results for the influential quantitative model of Rotemberg and Woodford (1997, 1999), and also a subsequent study by Amato and Laubach (2003). Another major section extends the basic framework of Chapter 4 by including capital investment explicitly in the optimizing analysis. One significant conclusion of the latter is that the framework without explicit capital formation can be interpreted and calibrated so as to provide a close approximation to more complex structures that do model consumption and capital formation separately (pp.

\textsuperscript{9} My current views, which must be subject to possible future revision, are summarized in McCallum (2003a).
352-372). The purpose of Chapter 5 is not to “offer a fully realistic quantitative model of the monetary transmission mechanism” but rather to provide insights regarding “several modeling techniques that are used in a number of recent examples of estimated models with optimizing foundations” (p. 321). In this respect, the chapter seems largely successful.

The remaining Chapters 6-8 are devoted to numerous aspects of optimal monetary policy, for economies modelled as in Chapters 4-5. Chapter 6 begins by developing the relationship between the individual-household utility criterion, which Woodford uses to discuss policy optimality in the book’s final three chapters, and the central bank’s objective function.

[Section is incomplete]

A few words should be added on the expositional qualities of Interest and Prices. Virtually every chapter begins with an introductory section that explains verbally, without resort to mathematical formulations, the issues with which the chapter will be concerned. Some of these discussions are not easy to understand, but mainly because the topics are inherently difficult. Woodford’s explanations are detailed and in many cases are eloquently argued. Quite generally, they reveal a true mastery of the material. Apart from the chapter introductions—i.e., in the remaining 95% of the book—the treatment is formal and demanding of an ability to keep many symbols in mind at one time. It avoids, nevertheless, reliance on esoteric mathematical results and provides explanations of some frequently-used tools. The book’s arguments are worked out in detail, with most steps being clearly explained. For this reader the going was not easy, however; I found myself frequently lost in blizzards of notation (e.g., $\bar{T}, \bar{T}_i, i, i^d, i^t, i_n, i^*_n, i^m, i^*_m, \hat{x}, \hat{x}^*, x$, $\bar{Y}, Y, \dot{Y}, \dot{Y}_n, x_i = \dot{Y}_i - \dot{Y}_n, x^*, \hat{x}^*, x^{ss}$), and alternative cases. Because of the numerous
concepts, cases, and symbols it is not easy, in my opinion, to dip into the book to read about particular topics without a study of previous sections and chapters. In several ways, the book would have been much easier to read if it had not included so many alternative specifications and cases. But then it would also have been less comprehensive, less encyclopedic, and perhaps less definitive.

3. Policy Optimality from a Timeless Perspective

One interesting and much-discussed aspect of Woodford’s approach to policy analysis concerns the notion of optimality from a “timeless perspective.” This topic is given an authoritative treatment in Chapter 7 of Interest and Prices, but the discussion is so detailed and extensive that some readers may find it difficult to put into perspective. I am myself on record as welcoming the approach but disagreeing slightly with its precise formulation.10 Here I would like to reconsider the matter, for I now believe my previous objections to be inappropriate.

In order to illustrate the issues, let us consider an example based on a simple workhorse model that is essentially the same as the basic model from Woodford’s Chapter 4. Its specification is not satisfactory for all issues, partly because it takes the average inflation target as given, but is useful for exposition of the particular topic at hand.11 Thus we suppose that the monetary authority (i.e., the central bank) seeks at time $t = 1$ to minimize

\[
L_1 = E_1 \sum_{i=1}^{\infty} \beta^{i-1} [ (\pi_t - \pi^*)^2 + \omega x_t^2 ]
\]

in an economy in which inflation $\pi_t$ and the output gap (or real marginal cost) $x_t$ are related by the price adjustment relation

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\( \pi_t = \beta_1 E_t \pi_{t+1} + \kappa x_t + u_t \quad \kappa > 0. \)

Here the output gap is measured as a fractional (or logarithmic) departure from the “natural rate” value of output that would prevail if prices were fully flexible, while \( \pi^* \) is the central bank’s inflation target value. Also, \( u_t \) is a stochastic shock, reflecting some sort of inefficiency that, for simplicity, may be taken to be white noise with mean zero. Note that the private-sector discount factor is denoted as \( \beta_1 \), indicating that it could possibly differ from that of the central bank, which is \( \beta \). Initially, however, we assume that \( \beta_1 = \beta \).

In this linear-quadratic setup, certainty equivalence prevails for conditional optimality, so we can write the Lagrangian expression

\[
L_1 = \sum_{t=1}^{\infty} \beta_t^{-1} \left\{ \left[ (\pi_t - \pi^*)^2 + \omega x_t^2 \right] + \lambda_t [\beta_1 \pi_{t+1} + \kappa x_t + u_t - \pi_t] \right\}
\]

and obtain the following first order conditions:

\[
\begin{align*}
(4) & \quad 2 \omega x_t + \kappa \lambda_t = 0 \quad t = 1, 2, \ldots \\
(5) & \quad 2(\pi_t - \pi^*) + \lambda_{t-1} - \lambda_t = 0 \quad t = 2, 3, \ldots \\
(6) & \quad 2(\pi_t - \pi^*) - \lambda_t = 0 \quad t = 1.
\end{align*}
\]

Then for all periods after the startup is completed, elimination of the Lagrangian multiplier \( \lambda_t \) yields

\[
\begin{align*}
(7) & \quad (\pi_t - \pi^*) + (\omega/\kappa)(x_t - x_{t-1}) = 0 \quad t = 2, 3, \ldots.
\end{align*}
\]

For the startup period, however, (4) and (6) imply

\[
(8) \quad (\pi_t - \pi^*) + (\omega/\kappa)x_t = 0 \quad t = 1.
\]

The difference between (7) and (8) arises because the latter is concerned with the transition

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\[11\text{ We are, in other words, here concerned with the attainment of specified policymaker objectives, not the determination of which objectives would maximize individual welfare. The latter topic is an important one, but the considerations explored here are logically prior—i.e., would continue to apply.} \]
from initial conditions, prevailing at the initiation of this policy regime, toward the stochastic steady state in which the system tends to settle down. The number of periods for which the conditions differ is only 1 in this example because of the model’s specification; in a more complex model it could be a larger number.

Let us now consider four types of policy strategy, which represent different perspectives on the concept of optimal monetary policy. The first of these is full commitment on the basis of existing initial conditions at \( t = 1 \); the relevant optimal rule is given by (7) and (8).\(^\text{12}\) This approach is, however, dynamically inconsistent. Indeed, it might be called strategically incoherent for the following reason: at time \( t = 1 \) it can be seen that condition (7) will be viewed as currently suboptimal if the policy strategy is ever reconsidered in any subsequent period.\(^\text{13}\) Furthermore, the policy maker may have as much of an incentive to reconsider the conditions in each subsequent period as he does to design a new policy in \( t = 1 \); there is no way in which period 1 is inherently different from periods 2, 3, ... This strategic incoherence manifests itself in a set of optimality conditions that are not time invariant—as indicated by equations (7) and (8).

We turn next to the “discretionary” type of optimization, i.e., a fresh calculation in each period constrained only by currently existing conditions. In this case, the startup condition (8) will apply to every period, \( t = 1, 2, ... \). There is no problem of strategic incoherence, because each period’s choice is based on the presumption that the decision

\(^{12}\) In this section I am using the word “rule” to refer to optimality conditions; i.e., to optimal targeting rules in the terminology of Svensson (2003). For partial disagreements with some of Svensson’s terminology and arguments, irrelevant to the issues of this section, see McCallum and Nelson (2004).

\(^{13}\) Many writers would say simply that the strategy is “time inconsistent,” meaning that the policy maker will have an incentive in periods 2, 3, ... to deviate from (7). Woodford uses that term in a quite different way, however, terming as time consistent any strategy that, if its reasoning were applied at a later date, would result in continuation of the policy chosen in \( t = 1 \) (2003, pp. 23, 473). With Woodford’s terminology, therefore, time consistency does not imply the absence of any incentive to renege on a previously chosen policy. It does imply strategic coherence, however, and is marked by time-invariant conditions.
maker will behave the same way again in each future period. The weakness of this strategy, as emphasized by Woodford (1999a, 2003) and others, is that its performance in terms of central bank objectives is in many cases relatively poor. As a comparison of equations (7) and (8) reveals, the strategy specifies, in each period after the startup, a condition that is quite different from one that would prevail under commitment when the economy is in the vicinity of its steady state. [For some illustrative quantitative magnitudes, see Woodford (1999a), McCallum and Nelson (2000), and Giannoni and Woodford, (2003).]

The “timeless perspective” strategy, introduced by Woodford (1999a), seeks to overcome these two problems by relying upon first-order conditions that would have been chosen under a commitment regime if it had been adopted in the distant past, i.e., by implementation of condition (7) in all periods including the startup period. This approach therefore specifies a rule that is time invariant. It is not “consistent,” in the Kydland-Prescott (1977) sense requiring that there exists no incentive for the policymaker to depart from the prescribed condition (7) in any period. Instead, there exists an incentive in each period to apply the discretionary rule (8), rather than (7), since the former would be preferable given prevailing conditions. The timeless perspective (TP) policy strategy [i.e., (7) for all \( t = 1, 2, \ldots \)] is not strategically incoherent as with full commitment, however; applying (7) in any period \( \tau \) after the startup yield a condition that agrees with the condition for period \( \tau \) that this policy strategy specified (or would have specified) in previous periods 1, 2, \ldots, \( \tau-1 \). In terms of performance, the TP policy yields outcomes that are on average superior (in the

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14 These references actually compare discretionary and “timeless perspective” policies, but the differences from an unconditional perspective are the same as for the comparison at hand.

15 Woodford’s strategy is closely related to the approach taken by King and Wolman (1999, pp. 377-380), discussed usefully by Wolman (2001). Dennis (2001) has suggested that there are many timeless perspective strategies, but Woodford (2003) argues that only one is time invariant.

16 In Woodford’s (1999a) language, the TP approach features strategic policy continuity.
example at hand) to discretion for most reasonable parameter values—see McCallum and Nelson (2000)—although Blake (2001) has shown that discretion yields superior outcomes in some extreme cases.

In my comment (2005a) on Svensson and Woodford (2005), I argued that the “fully timeless” (FT) rule of Jensen (2001) and Blake (2001), which optimizes with respect to the unconditional criterion $E[L_t]$, would yield better results on average than Woodford’s TP rule. This is accomplished by adoption of a forth rule, namely,

$$
(\pi_t - \pi^*) + (\omega/\kappa)(x_t - \beta_1 x_{t-1}) = 0 \quad t = 1, 2, 3, \ldots
$$

The idea here is that application of (8) in each period is not fully optimal unless $x_{t-1}$ happens by chance to equal zero in the start-up period, so one should average across different possible start-up conditions by taking the unconditional expectation of $L_t$ and minimizing with respect to that criterion. I have now persuaded myself that this idea is wrong, however, basically because it involves the same type of mistake as would application of the “golden rule” rather than the “modified golden rule” in the area of capital accumulation. That mistake is to restrict the type of equilibria considered before conducting the policy optimization, rather than optimizing with respect to any equilibrium and then (if desired) limiting the discussion to equilibria of a particular type.

Woodford’s way of looking at the distinction between TP and FT rules is to separate the central bank’s loss into two components, one deterministic and the other reflecting the equilibrium responses to shocks in all periods after the startup. Here I suggest that a more comprehensible way to look at this distinction begins by considering the possibility that $\beta$ and $\beta_1$ are not equal. Then the TP rule becomes
of which (7) is a special case. It is apparent, then, that the two rules (7') and (10) differ only in that the latter presumes that the central bank does not discount future outcomes relative to present ones. Note that this difference is relevant for both transitional and steady-state policy behavior. It would seem that for the transition episode the application of discounting would be inappropriate since the start-up conditions will almost certainly not be those for which (7) is fully optimal. That objection cannot, however, be applied to the steady-state situation. And for the steady state analysis it seems that if the central bank’s preferences include discounting of the future, then it would be improper to set $\beta = 1$ in (7'), as is implicitly done in (10).

Another way to view the issue is as follows. In the basic example at hand, application of (7'), both in the start-up period and thereafter, fails to be fully optimal only because the transition from the initial conditions to the stochastic steady state is not optimal. But this difficulty would not be present if the start-up happened to occur when the previous period’s $x_t$ was by chance equal to zero, for then (7') would entail the same behavior as (8). If instead (10) were applied, it would again be true that there would be no start-up or transitional inefficiency [since (10), too, is the same as (8) when $x_0 = 0$]. But in this case the conditions [(7') and (8)] for full optimality would not be met by use of (10), because $\beta \neq 1$, but would be by use of (7').

At present my overall conclusion, then, is that the TP rule (7'), not the fully timeless rule (10), is appropriate for the design of policy when it is desired to have a rule for “normal

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17 Neither Jensen nor Blake was able to derive (10) analytically; but Blake was able to verify using Maple that the condition is correct when $\beta = 1$.  
18 This conclusion was suggested previously by King and Wolman (1999).
policymaking,” as distinct from a transition from one regime to another. Of course there will be times in which such a transition—the move to a low-inflation regime after many years without such a policy, for example—is desired and for this the TP criterion is obviously inapplicable. But for transitions of this sort, the assumption that RE will apply immediately (i.e., during the transition) seems highly questionable. The topic of transitions leads to issues that are not considered in depth by Woodford, and which are clearly outside the scope of this review.

4. Issues Regarding Determinacy of Equilibrium

Another prominent feature of Woodford’s analysis, beginning in Chapter 2, is its emphasis on the importance of “determinacy” (i.e., uniqueness) of rational expectations (RE) equilibria. Thus on p. 77 he states, immediately after introducing a Wicksellian interest-rate rule to be used in conjunction with his introductory (flexible-price, cashless) economy, the following: “I am interested not only in whether a solution to this system of equilibrium conditions exists, but in whether these relations suffice to uniquely determine the equilibrium paths of interest rates and prices. The question of the determinacy of equilibrium is a preliminary, more basic issue, before one can hope to address the question of what factors affect the equilibrium price level and how they affect it.”

In my 2000 comments on a preliminary version of Chapter 4, mentioned above, I argued that its emphasis on determinacy was overdone and suggested that learnability was a more fundamentally important concept. Woodford, who knows more than I do about both concepts, seemed to agree in principle while disputing some of my more specific claims. Now, in its published version, Chapter 4 incorporates a 15-page discussion of learnability

19 While this rule has been developed most fully and effectively by Woodford, the contribution of King and Wolman (1999) was in circulation somewhat earlier.
that constitutes a sophisticated introduction to the subject, one that includes recent extensions
developed by Preston (2002). Elsewhere in the book (except on pp. 128-129) determinacy is the concept utilized.

What is the nature of the argument that, instead of determinacy, the distinct property of (least squares, LS) learnability—equivalent in dynamically stable cases to E-stability as emphasized by Evans and Honkapohja (2001)—is the one that is actually crucial? The argument is based on the notion (to be considered momentarily) that learnability is a necessary condition for any RE equilibrium to be regarded as plausible. If that notion is granted, then in cases of indeterminacy (i.e., multiplicity of RE equilibria) there may nevertheless be only a single equilibrium that is plausible, whereas an equilibrium that is determinate, but not learnable, is not a plausible candidate for an equilibrium that could obtain in actuality.

The position that learnability should be regarded as a necessary condition for the relevance of a RE equilibrium begins with the presumption that individual agents must somehow learn the magnitudes of parameters describing the economy’s law of motion from data generated by the economy; they cannot be endowed with such knowledge by supernatural forces. Of course any particular learning scheme might be incorrect in its depiction of actual learning behavior. But in this regard it is crucial to note that the LS learning process in question assumes that (i) agents are collecting an ever-increasing (unbounded) number of observations on all relevant variables while (ii) the structure is remaining unchanged. Furthermore, (iii) the agents are estimating the relevant unknown parameters (iv) with an appropriate estimator in (v) a properly specified model. Thus if a proposed RE solution is not learnable by the process in question—the one to which the Evans
and Honkapohja results pertain—then it would seem highly implausible that it could prevail in practice.

The foregoing suggests that determinacy and (LS) learnability are distinct concepts and that the latter is the more important. This raises the question of their relationship. In that regard, a recent working paper of mine (McCallum, 2005b) addresses the issue for a very wide class of linear RE models, one that permits any number of lags, leads (i.e., expectations), and lags of leads—essentially the class of models considered by King and Watson (1998) and Klein (2000). My paper’s main result is that, if current values of endogenous variables are known by individual agents when forming (non-rational) expectations in the calendar-time LS learning process, then determinacy implies learnability; every determinate solution is learnable. This leaves open the possibility, however, that some solution would be learnable—perhaps uniquely so—even in cases with indeterminacy.

Also clearly relevant is the concepts’ relationship under the alternative (and defensible) assumption that current values of endogenous variables are not known by agents during the learning process. As far as I can tell, no general results have been developed thus far, but the conditions for learnability [reported by Evans and Honkapohja (2001, p. 245)] are more stringent so it is clear that determinate equilibria can easily fail to be learnable. The hope of this research is that criteria can be found that will delineate model structures that yield learnable equilibria, and that these criteria will permit some intelligible economic interpretation.

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20 Some researchers adopt this assumption when studying learning processes but some assume that only lagged values of endogenous variables can be known, the case to be mentioned shortly.
There is a second matter concerning determinacy over which I have argued with Woodford in the past, namely, whether the distinction between “real” multiple-solution indeterminacy and “purely nominal” indeterminacy is of analytical (as well as historical) significance. There may be no one other than Michael and I (and probably not even him) who is interested in this issue, however, so I will refer the reader to my discussion in McCallum (2003b, pp. 1156-1157) and leave the matter at that.

5. Rationale for Interest Rate Smoothing

A highly innovative aspect of Woodford’s work on monetary policy analysis is his argument that a high degree of interest rate smoothing behavior by the central bank is likely to be socially desirable, rather than the impediment to good policy that many analysts have presumed it to be.²¹

[section incomplete]

6. Emphasis on Non-Monetary Economies

In this section, I wish to discuss two aspects of Woodford’s emphasis, throughout the book, on what I am calling his anti-monetarist approach to monetary policy. The second of these two discussions will be largely terminological—though perhaps of some significance nevertheless—but the first is more substantive. It concerns one of the more notable choices made by Woodford in designing his treatise, namely, the choice to organize the analysis and presentation around the case of a “cashless” economy. What he means by this term is that there are “no monetary frictions whatsoever” so that “central-bank liabilities have no special role to play in the payments system” (p. 31) and neither do any other assets. He does not, however, intend for the substantive analysis to be inapplicable to an economy with a medium of exchange. For he explicitly recognizes that “In all actually existing economies one
observes that positive quantities of base money are held by private parties despite the fact that this asset yields a lower return than other very short-term riskless assets” (p. 102). Thus in Chapters 2, 4, and 6 he continues, after first developing results for a cashless economy, with extensions designed to be applicable to an economy with “monetary frictions.”

Why, then, begin with the cashless case?

In that regard it seems highly probable that one of Woodford’s leading objectives is to feature a framework in which it can be rational for central banks to leave monetary aggregates entirely out of their policy calculations—in part because it is evidently the case that many actual central banks do in fact conduct policy in such a fashion. This is, I believe, a sensible objective. It is possible, in fact, for analysts who might reasonably be termed “monetarists” to agree that there are some benefits from conducting analysis in such a fashion. Furthermore, I believe that almost all analysts would agree to a somewhat weaker proposition, namely, that (whether or not they pay any attention to paths of monetary aggregates) most actual central banks do in fact use short-term interest rates as their principal instrument (or “operating target”) variables—that is, conduct policy via interest rate rules, not monetary aggregate rules. Clearly, Woodford’s choice to begin with a cashless economy leads directly to the accomplishment of this objective; it is easy to avoid use of monetary aggregate magnitudes in an economy with no money.

There is, however, an alternative way of proceeding while still leaving monetary aggregates out of the central bank’s policy calculations. This is to include transaction-

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21 For a pair of examples, consider Goodhart (1996) and Goodfriend (1987).
22 This term is misleading; the frictions under discussion are transaction frictions, which money can help to overcome. Money is not the source of the frictions.
23 Here it is being assumed that the discussion pertains to central banks in advanced economies with functioning financial markets.
facilitating money via the agents’ utility function (or via a resource-cost transaction function) while assuming that this function, in which a real-balance term appears, is separable in the monetary term vis-a-vis terms relating to consumption and any other arguments. In this case, no monetary-aggregate variable will appear in the optimizing intertemporal condition that constitutes one of the three relations in the basic model. With this approach there would be a transaction-oriented demand for real money balances implied by the optimizing analysis. Inclusion of the implied money demand relation in a model for policy analysis would be unnecessary, however, when policy is based on an interest rate policy rule; the money demand function would serve only to indicate the quantity of money needed to support the interest rate rule. Be that as it may, analysis concerning monetary policy behavior could with this specification be developed either in terms of an interest rate policy rule or a rule for periodic adjustment of some controllable monetary aggregate (such as the monetary base), as Woodford notes on pp. 105-108.

So, there are two approaches that can rationalize the omission of monetary aggregates from models designed for policy analysis: to use models with no transaction frictions or to include transaction frictions but assume separability of the transaction function. Since the latter is more general and since actual non-monetary economies do not currently exist, there would seem to be little gain from beginning with the case of a cashless economy, even if the analysis is somewhat simpler, unless it could be shown that the cashless approach provides a good approximation to the results obtained with the other approach. Woodford recognizes this point, however, and accordingly is careful to analyze the extent to which neglect of transaction frictions would matter. In fact, he even considers cases in which there is non-separability of the relevant transaction function. For the case of his “basic economy,” which
includes price-adjustment frictions as well, he shows in Chapter 4, on pp. 299-311, that the quantitative impact on the model’s impulse response functions is quite small, given his calibration of the magnitude of the non-separability.\footnote{This calibration agrees closely with the one developed in McCallum (2001).} And he goes on to describe econometric evidence presented by Peter Ireland (2004) that is consistent with his calibration. So in this regard, as in many others, the rigor and completeness of Woodford’s analysis is exceedingly impressive.

But while I have nothing but admiration for Woodford’s formal analysis, I would suggest that some of the book’s terminology is unhelpful and that some of its anti-monetarist strictures are uncalled for. Consider, for example, a finding presented as Proposition 2.4, on page 82. In this proposition it is shown that if an asset termed “money” is used as the basis of central bank policy, even though it provides no transaction services and no interest payments to its holders while another asset (government bonds) is available in greater nominal quantity, then there exists no rational-expectations equilibrium path for the price level, i.e., the reciprocal of the value of the asset termed money. In this context, Woodford says: “Results of this kind are often taken ... to imply that it is not possible to model the determinants of the exchange value of money despite its low rate of return. Hence monetary frictions are thought to be an essential element of any theory of the effects of monetary policy. But we have seen instead that an equilibrium in which money exchanges for goods is possible even in a cashless economy, as long as the rate of interest paid on money is high enough relative to the growth rate of the money supply” (pp. 82-83).

Well, I will offer myself as one example of someone whose usual terminology would suggest that transaction frictions are an essential element of any theory of the effects of monetary policy. Why? Because, following Wicksell (1935/1915, pp. 6-7), my usual
definition of money involves the requirement that it serves as a medium of exchange and accordingly provides transaction-facilitating services. 25 The central bank liability that Woodford describes in his cashless economy I would not call money, and accordingly I would say that his cashless economy is a non-monetary economy. With a zero rate of interest paid on this liability/asset, there would clearly be no demand for it—there is another asset available to households as a store of value—so its exchange value would be zero. Thus there would obviously be no finite reciprocal of its exchange value—no finite “price level”—in any period. With this terminology, then, it becomes clear that there is nothing whatsoever surprising about the stated result.

Continuing, suppose next that, as in Woodford’s discussion, the central bank pays interest on this liability/asset—let us call them CBs. Then if the rate paid is high enough there would be a demand for CBs. Furthermore, if these CBs serve as the economy’s medium of account, with one CB being the unit of account, we could reasonably refer to the exchange value of goods relative to CBs as the price level. (In this case we cannot use the usual definition, i.e., the inverse of the exchange value of the medium of exchange.) Clearly, if the central bank manages the interest paid on CBs appropriately, it can generate a well-defined (and perhaps even determinate) price level, given the foregoing definition.

But for this measure of the price level to be of any great significance, it needs to be the case that CBs are in fact serving as the unit in which prices are being quoted and to which nominal price stickiness applies. Woodford recognizes this point, stating on p. 37 that “there is perhaps no deep, universal reason why” it should necessarily be that the CB liability/asset serves as the medium of account in terms of which market prices are set. But the point

25 Wicksell (1935, pp. 6-7) states that “… of the three main functions [as a measure of value, as a store of value, and as a medium of exchange] only the last is in a true sense characteristic of money.”
deserves more emphasis, I believe. In the case of a traditional monetary economy, one with a dominant medium of exchange, there is a transaction-cost reason why prices should be quoted (provided that monetary policy is not managed too badly) in terms of the medium of exchange. In the case of the cashless economy, with no medium of exchange, the argument is somewhat different and should be made explicit. It is stated, in *Interest and Prices*, but very briefly. It would be easy to miss this altogether.

In addition to the foregoing points, it should be noted that in the cashless-economy analysis of pp. 74-82, with which Woodford begins, policy is not carried out in a manner close to that currently used in the United States or other major economies. In actuality we have central banks that use interest rate policy rules and pay no attention to monetary aggregates, of course, but in most cases these rules specify periodic adjustment of rates (such as the federal funds rate in the United States) on some very short term asset traded among banks, while keeping fixed rates that are paid by the central bank to holders of its liability that Woodford would call money. In the analysis in question, however, the central bank conducts policy by manipulation of the rate paid on this liability/asset. The overnight rate equals that value in equilibrium, because no rational private entity would hold the useless (in the analysis) central bank liability/asset unless it paid interest at least as great as that in the overnight market while the household’s problem is not well defined unless the overnight rate is as least a high as the central bank rate (Woodford, 2003, pp. 67-70).

Finally, I have one history-of-thought quibble regarding the matter of cashless economies. Specifically, my reading of Wicksell (1935, 1936) indicates that his “pure credit system” does not, unlike Woodford’s cashless economy, necessarily presume that central bank balances provide no useful services to their holders. What Wicksell evidently means by
a pure credit economy is one that makes no use of gold or any other metallic standard, and in which bank notes do not circulate, but in which payments are made by means of checks written on accounts at commercial banks—which, in turn, settle via balances held at a central bank. But I can find nothing in Wicksell’s writing to suggest that the central-bank deposits, and checking-deposit balances in banks, provide no transaction-facilitating services to their holders.

7. Conclusion

There are numerous ways, not adequately brought out above, in which Woodford’s book makes substantial contributions. There are also other specific issues, in addition to those of sections 4-6 above, that could usefully be discussed at length; a few of these are: the depiction of misallocations provided by the Calvo model; the limited degree of robustness provided by “robustly optimal” rules of the type discussed; imperfect credibility, and transition periods following policy rule changes. Several papers have been written on such issues in recent years and many more will be written, one can be confident, over the next several years.

In conclusion, one might ask if Woodford’s treatise is successful in its objective of providing “theoretical foundations for a rule-based approach to monetary policy” (p. 2) that “takes full account of the implications of forward-looking private sector behavior” (p. 55) while using as its ultimate criterion of policy desirability “the expected utility of the representative household” (p. 57). The book recognizes—even stresses—that the “precise content of an optimal policy rule” will depend upon details of the adopted model of the transmission mechanism and thus are likely to be different for different economies (p. 58). It

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26 The absence of bank notes is stipulated in Wicksell (1936, p.) but apparently not in the later volume (1935).
27 See Wicksell (1935, 1936) and comments by, e.g., Patinkin (1956, pp.) and Blaug (1986, p. 647).
aims accordingly to provide a method that analysts in different economies can use in combination with studies of their own economy to develop rules for beneficial policies. Whether or not the exact procedures suggested in the book will be utilized, the issues illuminated by Woodford’s investigations will likely be at the heart of analytical work over the foreseeable future. In the area of monetary policy analysis, Woodford’s *Interest and Prices* is in my opinion the most important book to have been published in decades, and is one that could retain its position for decades to come.
References


