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## Comments on the Symposium on *Interest and Prices*

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## COMMENTS ON THE SYMPOSIUM ON *INTEREST AND PRICES*

BY  
MICHAEL WOODFORD

The papers prepared for this symposium cover a great deal of ground. It is a pleasure to be able to respond to them, as this provides an opportunity to try to clarify some of the aims of my book (Woodford 2003) that may have been insufficiently clear in the original work. But I shall have to restrict my attention to only a few of the most important issues raised in the various papers. I shall consider three classes of issues in particular: the methodology adopted in the book, the role of “cashless” models, and the connection between my work and the Wicksellian tradition.

### I. METHODOLOGY

I shall begin with Kevin Hoover’s (2004) discussion of the methodology of *Interest and Prices*, as it raises the most general issues about the book’s aims and accomplishment. Hoover rightly states that the book’s methodology is squarely within the recent mainstream of macroeconomic theory, and in particular that it adheres to central tenets of a methodology that appeared to be quite radical when first advocated by authors such as Robert Lucas, Edward Prescott, and Thomas Sargent in the 1970s. I do not prefer to call this the “new classical” methodology, as my book aims to show that the methodology in no way requires one to accept the substantive views (also novel and radical) associated with the new classical macroeconomics; and it is also, as Perry Mehrling (2004) notes, the methodology of both real business cycle theory and modern financial economics, among a number of literatures that now use similar methods. In my view, one of the greatest conceptual advantages of this methodology is that it allows the analysis of monetary policy to be integrated with the analysis of fiscal policy, by adopting the methodology that has been standard in the theory of public finance since the 1970s.<sup>1</sup> But Hoover is correct that the methodology is the one introduced into monetary macroeconomics by the new classical literature,

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Department of Economics, Columbia University. This is a revised draft of remarks made at the “Symposium on Woodford’s 2003 *Interest and Prices*” at the Annual Meeting of the History of Economics Society, Victoria University, Toronto, Canada, June 25–28, 2004.

<sup>1</sup>Benigno and Woodford (2003) develop this integrated theory, building upon the theory of monetary policy presented in *Interest and Prices*.

and those who disliked that literature on methodological (as opposed to substantive) grounds will probably be unpersuaded by the analysis in *Interest and Prices*.

I do not think that this is the place to attempt a full defense of that methodology. As Hoover states, there is nothing very unusual about this aspect of the book. However, it may be appropriate to comment briefly on the extent to which this particular book illustrates the various methodological sins Hoover considers to be endemic to modern macroeconomics. In particular, Hoover says that “economists of similar methodological convictions” offer an “eschatological justification” for their models: “the current models are to be believed, not because of what we can demonstrate about their current success, but because they are supposed to be the ancestors of models—not now existing—that, in the fullness of time, will be triumphant.” I would not make the case for the practical importance of my research in quite these terms.

In fact, my own methodological position is quite close to the one that Hoover endorses when he says that “models are models; they must leave things out; they must make simplifying assumptions. We can speculate on which are important and which innocuous. In the end, only data will tell.” This is certainly my own view, for example, of the status of the hypothesis of a representative household. It is obviously not literally correct that an economy such as that of the U.S. is made up of identical households; but no economic model, at least one that fully explains what is supposed to be happening in the model, rather than simply postulating an unexplained statistical relationship, will be literally correct. The question is which simplifications are useful—because they make reasoning with the model much more tractable while only modestly impairing the accuracy of the conclusions reached—and which are fatal mistakes.

My own working hypothesis at present is that the fiction of the representative household falls under the category of useful simplifications. All such hypotheses should be subject to critical scrutiny and ultimately to the test of empirical accuracy. I would not list a preference for models with a representative household as a core methodological commitment in my work (though it has certainly been a useful device); and a great deal of current research by people who share my general methodological commitments does seek to explore the quantitative difference that is made by introducing empirically realistic forms of heterogeneity into dynamic macroeconomic models. My own view is that research of this kind has not yet shown that heterogeneity matters a great deal quantitatively, relative to the reduction in convenience of model analysis that it requires; but my difference with those who choose to devote more effort to this particular issue is not one of methodology.

Hoover states that “serious microeconomic theory . . . makes it clear that there is no reason to believe that macroeconomic aggregates should behave like” the choices of a representative household. But what this means is that there is no compelling *a priori* argument that a representative household model *must* be accurate—it could fail to be true without this discovery challenging the basic choice-theoretic foundations of modern economic theory. But “serious microeconomic theory” provides no presumption that a representative-household model should not be accurate, either. How far off one’s conclusions are when one makes use of that simplification is a quantitative issue.

In fact, the neglect of aggregation issues in the models in *Interest and Prices* is justified by another device that I employ extensively in the book, and that is the

resort to linearizations of model structural equations as a computational device. This is, of course, characterized as merely an approximation that should be valid in the case that the sources of randomness in the economy are sufficiently small, but that may or may not be accurate in practice (another issue that deserves further quantitative investigation). But to the extent that linearization of individual decision rules is taken to be an acceptable approximation, it follows that aggregate choices should be a function of the aggregate values of the variables that determine the individual choices—just as also follows from the assumption of a representative household. If linearization is *not* very accurate in the contexts where it is used in the book, then aggregation is likely to be an important issue as well; but in that case, the neglect of aggregation issues would hardly be the main reason for doubting the accuracy of the conclusions reached in the book.

Hoover also expresses skepticism about the view that relatively simple micro-founded models of the kind presented in *Interest and Prices* are valuable because they point the way toward more empirically adequate models that can be built upon these foundations. “Woodford does not give an indication of how far such models might have to be developed [to be adequate to the needs of central banks]. Many macro-economists pay lip service to the notion of such development, but to me . . . it seems like a fruitless enterprise and not one that has actually engaged much of the profession.”

Instead, in my view the book describes a research program that has already made considerable progress in developing models consistent with detailed aspects of the co-movements of key aggregate time series. The book itself devotes considerable attention to the progressive complication of an initial baseline model, in order to achieve progressively greater degrees of empirical adequacy, while discussing in detail the consequences for policy analysis of each successive refinement of the model. Subsequent work has developed still more complex models that match a larger number of features of a larger number of time series<sup>2</sup> and that fit nearly as well as purely atheoretical models of the data;<sup>3</sup> yet these models build directly upon the ones discussed in *Interest and Prices*, so that many of the insights developed in the book should carry over to analyses of policy in the context of the more realistic models. I do not mean to suggest that further progress is not likely still to occur, or that one can already be sure that the ultimately successful models will not be quite different from the best ones currently available. But it is not fair to suggest that mere “lip service” is being paid to the hope that empirically adequate and practically useful models can be developed along the lines investigated in the book.

## II. MONETARY ANALYSIS IN A CASHLESS ECONOMY

David Laidler (2004) and Perry Mehrling (2004) instead address an aspect of *Interest and Prices* that is fairly distinctive, and that is the suggestion that at least as a first approximation, it is useful to abstract from monetary frictions in analyzing monetary policy.

<sup>2</sup>Notable examples include the models of Christiano, Eichenbaum, and Evans (2005), Altig *et al.* (2005), and Smets and Wouters (2003, 2004).

<sup>3</sup>See, for example, the comparisons of fit in Smets and Wouters (2003) and in Del Negro *et al.* (2004).

Laidler criticizes the emphasis given in my book to the case of a “cashless” economy. He appears to believe that the case for considering cashless models depends on a claim that actual economies are now practically cashless. Instead, he says, while it is now true that in some economies (such as Canada) the central-bank balances required for the operation of the interbank payments system are negligible, it remains the case that a non-trivial quantity of bank deposits and currency are held in order to carry out payments between individuals. Laidler suggests that as a result, the quantity of money is likely still to play an important role in the monetary transmission mechanism.

But I do not argue that the actual economies for which the book is intended to be relevant are already cashless, or nearly so. Rather, I argue that the analysis of monetary policies in the context of a cashless model is a useful simplification, one which allows a simpler and more transparent development of basic insights that are believed also to be relevant to more complex models incorporating empirically realistic monetary frictions. Once again, the question of how useful the shortcut is, and how urgent it is to go beyond it for purposes of practical policy analysis, is a quantitative matter. But the conjecture that taking account of the role of money in transactions would not require a great modification of the analysis is not merely a pious hope; in the book, I consider at several points how the analysis would have to be modified in order to incorporate one simple (and highly conventional) representation of the consequences of monetary frictions (the Sidrauski-Brock model) and find that this would make rather a small difference to my quantitative results.

Nor does this result depend on assuming that the only balances held to facilitate transactions are the relatively small quantity of central-bank balances used to clear payments between banks. In the calculation reported on p. 118, I use a value of 0.25 quarters for  $\bar{m}/\bar{y}$  (the steady-state ratio of money balances to GDP), and obtain a value of  $\chi$  (the parameter that indexes the degree of departure from the predictions of the cashless model) that is at most 0.005. This value for  $\bar{m}/\bar{y}$  is based on the U.S. monetary base, which consists largely of currency (and not just the small quantity of balances held by intermediaries at the Fed). About half of this currency is held outside the U.S., and thus not plausibly facilitating U.S. transactions; hence one could easily argue for an even smaller value of  $\bar{m}/\bar{y}$  if this is understood to represent base money used to facilitate transactions in the U.S. On the other hand, even if one counts all of the outstanding currency, and also all of the rest of M1 (and thus the bank deposits upon which checks are written, to which Laidler refers),<sup>4</sup> the value would be less than twice the size of the one that I assume; and so one would obtain a value of  $\chi$  at most equal to 0.01. In fact, in the figures in *Interest and Prices* that illustrate the quantitative significance of allowing for monetary frictions, I assume a value of  $\chi$  equal to 0.02. This would still seem to me a generous estimate of the plausible size of monetary frictions in the U.S. economy.

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<sup>4</sup>It is not obvious that one should do so, because of the payment of interest on these other components of M1. What matters in the computation of  $\chi$  is actually the aggregate interest lost as a result of holding money balances. If checkable deposits pay an interest rate lower than the rate available on short-term money-market instruments only because of the non-interest-earning vault cash and reserves that must be held in order to provide these deposits, then the aggregate interest lost would only equal the money-market interest rate times the quantity of base money (which would include the vault cash and reserves that back the checkable deposits), which is the calculation proposed in *Interest and Prices*.

Laidler also offers several examples of issues in monetary economics that in his view cannot be adequately addressed in a cashless framework. I do not mean to claim that there are no such issues. For example, Laidler mentions the contribution of *seigniorage* revenues to the government budget. This is indeed something that would not exist in a cashless economy, and to the extent that *seigniorage* revenues are of practical importance, they cannot be analyzed without modeling monetary frictions. However, *seigniorage* does not play an important role in the government finances in most developed economies, nor are the consequences of monetary policy for the level of *seigniorage* revenues a concern in monetary policy debates in those countries. Hence this does not seem to me an important objection to the use of a cashless model to analyze monetary policy choices for countries of that kind (the goal of the framework proposed in *Interest and Prices*).<sup>5</sup>

At certain places and times, however, the pursuit of *seigniorage* has been a major determinant of monetary policy decisions; and in an analysis of inflation dynamics in such cases, the transactions frictions that give rise to a money demand function would play a crucial role. But the framework proposed in *Interest and Prices* can easily be extended to include such frictions; as I have noted, this extension is treated at several points in the book. It is hard then to see this as an important weakness of the framework proposed in the book.

Nor would I agree with Laidler's argument that the role of U.S. fiscal policy in explaining the increase in U.S. inflation in the late 1960s and early 1970s can be understood only in a model that takes account of the demand for money, and hence allows for *seigniorage* revenues. Laidler states that "U.S. fiscal deficits . . . were monetized." But inflation improves the government's finances (and so substitutes for other sources of revenue) even in a cashless model, as long as there exists nominal government debt, the real value of which is reduced by inflation. Hence one can explain why the deficits of that period would have increased the attractiveness of inflationary monetary policy, even without taking account of the modest contribution to government revenue from *seigniorage*.

Another issue that Laidler says cannot be addressed well with a cashless model is the conduct of monetary policy in a deflationary environment of the sort recently experienced in Japan. I am even less willing to concede this case. Although the problems created by the zero lower bound in such a case are given little attention in *Interest and Prices*, they are treated in detail elsewhere. Eggertsson and Woodford (2003) analyze what can be achieved by expanding the monetary base through purchases of a wide variety of types of assets from the private sector, once the zero lower bound on short-term nominal interest rates has been reached, in the context of a model that does *not* abstract from the transactions role of money and incorporates a perfectly orthodox model of money demand.

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<sup>5</sup>This does not mean that one might not wish to take into account the fiscal consequences of monetary policy decisions in such countries. In my view, the more important fiscal consequences of monetary policy in these economies relate to the effects of inflation and interest rates on the dynamics of the public debt; Benigno and Woodford (2003) provide an analysis of monetary-fiscal interactions within a cashless framework. Debt dynamics are also the more important source of interrelation between monetary and fiscal policy choices in some developing countries as well. See Giavazzi *et al.* (2005) for a recent example.

While the central bank's policy with regard to short-term nominal interest rates does not suffice to determine how much base money it may supply in this case, we show that alternative degrees of expansion of the monetary base have no effect, regardless of the type of assets purchased by the central bank, if they imply no change in the way in which interest rates are expected to be determined in the future. This is true even though long-term interest rates remain positive. Thus we find that a genuine "liquidity trap" exists, and that the scope for monetary policy to ameliorate the deflation and slump is exactly the same in a model that explicitly models the role of money as in a cashless model—namely, it can be effective only to the extent that expectations about the future conduct of interest-rate policy are changed.

Recent experience in Japan seems to me to accord well with such a model. As Laidler notes, the monetary base was dramatically expanded in Japan after the spring of 2001, yet there has been no corresponding surge in nominal spending. Deflation in Japan has not yet been curtailed; there has been some recovery in real activity, but the strength of the recovery remains uncertain and the growth in output has been nowhere nearly proportional to the expansion (by more than fifty percent) in base money. What has occurred is very much like what a simple model would predict, in which a decline of the overnight nominal interest rate to zero indicates satiation in money balances, so that further increases in the monetary base do not facilitate any additional transactions and thus do nothing to stimulate spending.

Laidler suggests that standard models are inadequate as sources of policy advice for Japan because they neglect the possibility of a Hawtreyan "credit deadlock." While it is true that the model of Eggertsson and Woodford (2003) does not model credit market imperfections, in my view a credit deadlock is one example of the kind of situation that could cause the Wicksellian natural rate of interest to become negative for a time. The analysis in our paper does assume that the natural rate of interest is temporarily negative for reasons that are taken as exogenous and are not further modeled. Nor do I believe that the diagnosis of Japan's problem as a credit deadlock provides any reason to suppose that the key to breaking the impasse must be an expansion of the money supply, so that a model that abstracts from the transactions role of money will fail to identify the policies that would work.

Of course, in a model that *does* include a standard money-demand function (like the model of Eggertsson and Woodford), breaking the credit deadlock will be associated with an increase in the money supply, because additional spending will imply greater money demand, which would have to be accommodated in order for interest rates not to rise. But this does not mean that the means required to break the deadlock (which involve changing firms' perceptions of the risks associated with investment spending) are essentially dependent on increasing transactions balances. In a cashless model, one could analyze the determinants of the demand for credit without the distraction of having to also model the demand for transactions balances.

Perry Mehrling instead welcomes the fact that the baseline model in *Interest and Prices* is a cashless one, insofar as this allows me to address the question of what, if anything, central banks would be able to do in a world of highly efficient financial markets of the kind posited in the writings of Fischer Black, and toward which we are arguably headed. He is unconvinced, however, that the book's analysis succeeds in establishing that central banks would be able to maintain price stability in such a world through adherence to policies such as a Wicksellian interest-rate rule.

Mehrling's first important objection is that what I call "the basic neo-Wicksellian model" is one in which there exist no capital goods (or for that matter, durable consumer goods) that can be used as real stores of value. He argues that this omission results in the absence, in the book's analysis, of certain forces of "arbitrage" that would tend, in actual economies to force an equality between the natural and market rates of interest. But in chapter 5 the book does discuss an extension of the basic framework to a model with staggered pricing and endogenous capital accumulation. In the extended model, inflation determination can still be understood in terms of the gap that is allowed to develop between the natural rate of interest (a function of exogenous real factors) and the intercept term in the central-bank reaction function (see p. 376 and following). Hence the basic analysis of inflation determination presented in chapter 2 (for a flexible-price endowment economy) and then in chapter 4 (for a sticky-price model without endogenous capital) continues to apply in the more complete model.

I don't explicitly discuss the question of whether the market and natural rates of interest are different in equilibrium in that model, because in the book as a whole I give more attention to the role of the gap between the natural rate and the intercept term of a Taylor rule as the explanation of inflation variations.<sup>6</sup> But it is easily seen that the market real rate of interest can depart from the natural rate, and that the extent to which it does so will depend on monetary policy. For example, Figure 5.8 shows the market real rate of interest being affected by a purely monetary disturbance, while the natural rate of interest is unaffected. (In the figure, a rise in the market real rate of interest relative to the natural rate results in reduced inflation.) There is no "arbitrage" opportunity created by a gap between the two, since the first-order condition for optimal investment spending is satisfied. The fact that it is possible for the market real interest rate to differ from the natural rate, even when investment demand is optimal given the market real interest rate, amounts to the observation (in the familiar Hicksian formulation) that the real interest rate varies as one moves down the "IS curve" (to levels of output that differ from the natural rate of output). This is true as long as investment demand is not *infinitely* interest-elastic.

Mehrling's second important objection is to the book's explanation of why a central bank should still be able to control overnight nominal interest rates in a cashless economy.<sup>7</sup> (It is on this point that I explicitly reject the assertions of Fischer Black (1987) about the scope for monetary policy in a frictionless world.) Mehrling slightly mischaracterizes my argument on this point. He states that I assume that the government has the "legal authority simply to state what is the unit of account." What I actually say is that the central bank defines the meaning of the currency unit (for example,

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<sup>6</sup>One advantage of this reformulation of Wicksellian doctrine is that it applies even to a flexible-price/full-information model, in which the natural rate and the actual real rate of interest can never differ in equilibrium. Another advantage is that under rational expectations, there is often not a determinate equilibrium associated with a specified expected path for the nominal interest rate or even for the real interest rate, but there may well be a determinate equilibrium corresponding to a particular expected path for the reaction-function intercept. (This requires that the response coefficients of the central bank reaction satisfy certain inequalities—referred to as the "Taylor principle" in the case of the baseline neo-Wicksellian model.)

<sup>7</sup>Boianovsky and Trautwein (2004) express skepticism about the same point.



the meaning of “a U.S. dollar,” in the case of the Federal Reserve); someone who promises to pay a certain amount in U.S. dollars is promising to deliver something with properties that are defined purely by the policies of the Fed. Whether this will actually be “the unit of account” in the U.S. or elsewhere is another matter. I do not assume that this can be legislated, though legal tender statutes can help to make it convenient for private parties to quote prices in terms of dollars. In the models expounded in the book, I assume it to be an institutional fact that the sticky prices are quoted in units of the national currency, a certain kind of liability of the central bank; and it is because of this that the pursuit of price stability by the central bank matters for private welfare. But the claim that the central bank can control *nominal interest rates in units of the national currency*, and by so doing control *the purchasing power of the national currency*, does not depend on the number of suppliers who choose to set prices in those units; the latter question matters only for the real consequences of stability or instability in the value of the U.S. dollar.

Mehrling objects to the account given of interest-rate control in a cashless economy by suggesting that it is based on an unpersuasive analysis of how overnight interest rates are controlled at present in “channel systems” like that of Canada. I argue that the central bank’s ability to set the overnight interest rate on balances held at the central bank follows from the fact that these liabilities of the central bank are by definition equal in value to the national currency; whereas the market value of a Treasury bill can vary depending on the market’s view of the correct interest rate, a million dollars of overnight balances at the central bank can never be worth anything other than exactly one million dollars.<sup>8</sup> This then puts a floor on overnight interest rates in the interbank market. In the case that central-bank balances perform a useful role in the payments system, the market overnight rate can be *higher* than the rate paid by the central bank on these balances. But the interest rate at which the central bank is willing to freely lend overnight establishes a ceiling for the market rate. (In a cashless economy, there would be no need for the lending facility, and the market rate would simply equal the rate paid on central-bank balances.)

Mehrling proposes instead that under a channel system, the central bank’s standing facilities are able to bound the market interest rate only because the central bank stands ready to borrow or lend unbounded amounts at the interest rates that it announces. Since this capacity to borrow or lend unbounded amounts at fixed rates is unlike the behavior of typical dealers in securities markets, it must depend on a special role of the central bank under current arrangements that it would not obviously retain in a frictionless world. I think this reasoning misses something important. Mehrling asks why central banks with channel systems “are apparently not worried about being left holding large losing positions,” as private securities dealers would be if they offered to trade unlimited accounts at their posted bid and ask prices. In the case of a central bank’s lending facility, the answer is that insofar as borrowers take it up on its offer to lend funds overnight, additional central-bank balances are created *that must be deposited overnight with the central bank* by someone (not

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<sup>8</sup>This is why the observation of Boianovsky and Trautwein that in a frictionless economy “other riskless nominal assets are perfect substitutes for money, so [that] the law of one price rules,” does not imply the conclusion that they draw, namely that “the central bank is then a price-taker, not a price-setter.”

necessarily the borrower); insofar as the interest rate on the borrowing facility exceeds that on the deposit facility, the central bank necessarily profits, and by more, the more financial institutions choose to borrow from it. There is similarly no possibility of the central bank attracting an unexpectedly large quantity of deposits if the rate offered on its deposit facility is “too high.” The total quantity of deposits in existence cannot be increased except if someone borrows from the central bank’s lending facility,<sup>9</sup> in which case an even higher rate is paid for those funds by the borrower, so that the central bank gains, rather than suffering large losses. One observes that a central bank can operate standing facilities without any risk of large losses, and that this depends in no way on the “institutional apparatus of the banking system” that I have omitted from my model and that would no longer be present in a cashless economy.

### III. IS IT REALLY WICKSELLIAN?

Finally, both Mauro Boianovsky and Hans-Michael Trautwein (2004) on the one hand, and David Laidler on the other, question how faithful my own *Interest and Prices* has been to the elements that they liked best in Wicksell (1898). Laidler protests that Wicksell was not really as radical a critic of the quantity theory as I am. Boianovsky and Trautwein instead agree that the emphasis that I give to monetary analysis in a cashless framework is an aspect of my project with echoes of Wicksell, but they nonetheless find that my analysis omits much of the richness of the Wicksellian tradition.

I shall not make the mistake of quarreling with such attentive readers of Wicksell about the content of Wicksellian doctrine. In fact, my aim in the book has been to develop a framework for monetary policy analysis that is at once logically coherent, empirically plausible, and practically useful under current circumstances in countries like the U.S. The goal of expounding Wicksellian theory for modern readers is entirely subordinated to that other, more important project. I have taken some inspiration from the Wicksellian tradition and have commented on that in the book, but if forced to choose between being more Wicksellian and hewing more closely to current best practice in monetary policy analysis, I have also chosen the latter.

It is perhaps worth commenting on why I have chosen to call my framework “neo-Wicksellian,” rather than calling it a “New Keynesian” approach, as many of my readers apparently do, including several reviewers of the book (perhaps following Clarida, Gali, and Gertler 1999). The approach developed in *Interest and Prices* builds upon a number of important insights from the “new Keynesian macroeconomics” of the 1980s,<sup>10</sup> but it also departs from that literature in some fundamental

<sup>9</sup>Additional central-bank balances can also be obtained by returning currency to the central bank. But the amount of currency that is held is always held despite the fact that a higher interest rate is available on overnight deposits at the central bank, and so is held because of some convenience yield attaching to currency balances. In a cashless economy, no currency would be held, and the monetary base would consist solely of interest-earning central-bank balances.

<sup>10</sup>These include the insight that the postulate of rational expectations can be usefully combined with the hypothesis of wage and/or price stickiness, the use of models with monopolistic competition together with prices chosen in advance as a way of introducing price stickiness into models with optimizing foundations; and the emphasis given to “real rigidities” as a source of sluggish adjustment of the general level of prices to aggregate conditions.

respects, rather than simply offering an updated treatment of the same themes. Notably, I emphasize the use of a short-term nominal interest rate as the instrument of monetary policy and accordingly devote considerable attention to the connection between interest rates and aggregate demand, while the “new Keynesian” literature identified monetary policy with changes in the money supply, and tended to connect this policy instrument with aggregate demand by postulating a simple quantity relation (possibly motivated as reflecting a cash-in-advance constraint). I also emphasize the usefulness of the Wicksellian concept of the natural rate of interest in understanding how a given kind of interest-rate policy will affect inflation determination, and accordingly stress the role of real disturbances—resulting in variations over time in the natural rate of interest that are not sufficiently offset by adjustments of central-bank interest-rate policy—in giving rise both to fluctuations in inflation and to departures of aggregate output from potential. The “New Keynesian” literature has instead more often stressed fluctuations in money growth as the source of business cycles.

There are undoubtedly many differences between the theory of monetary policy that I propose and that of Wicksell and his early followers. In my view, it is interesting to observe that conclusions of a Wicksellian flavor can be obtained *without* having to base them on the specific assumptions made in Wicksell’s own writings; the case for the importance of the basic insights is only made stronger by observing that they can also be defended on alternative grounds. For example, in the early Wicksellian literature, the distortions resulting from inflation or deflation are often associated with redistribution between workers and firms. I instead work with representative-household models in which redistribution of income has no consequences. Nonetheless, it is not the case that a surprise change in the real value of the nominal wage agreed upon at an earlier date owing to a change in the price level has no consequences for the allocation of resources. In my book, I base my analysis of the welfare consequences of inflation and deflation entirely on the misallocation of resources that results from the incorrect *incentives* provided by misaligned wages and/or prices; but one still obtains conclusions about the appropriate goals of monetary stabilization policy similar to those of authors such as Lindahl.

A similar comment can be made about the use of the postulate of rational expectations in most of the analysis of inflation determination in my book. This is hardly faithful to the method of Wicksell or the early Wicksellians, who instead assumed myopic or adaptive expectations, and the logic of the analysis through which I demonstrate that an interest-rate policy with insufficient feedback from inflation and/or output to the interest-rate operating target can result in instability due to self-fulfilling fluctuations is quite different from Wicksell’s analysis of the “cumulative process.” Nonetheless, I think there is actually an interesting parallel between the conclusions that can be obtained on the basis of a rational-expectations analysis and those that would be obtained under the assumption of adaptive learning (as discussed in chapter 4). In the rational-expectations analysis, too great a sensitivity of actual inflation to inflation expectations can result in a large multiplicity of possible rational-expectations equilibria; in the adaptive-learning analysis (much closer in spirit to the old Wicksellian literature), instead, too great a sensitivity of actual inflation to inflation expectations makes it possible for small initial perturbations of inflation expectations to propagate explosively. The nature of the dependence

of actual inflation on inflation expectations that results in vulnerability of the economy to self-fulfilling expectations is quite similar under the two cases, and the types of interest-rate policy needed to prevent the problem from arising are equally similar under both analyses. The derivation of conclusions for monetary policy with much the same flavor as those obtained from the traditional Wicksellian analysis, even without the hypothesis that it is possible for beliefs to depart systematically from actual outcomes for a prolonged period, only strengthens the case for the practical relevance of those conclusions.

Some may find it odd to seek to connect a modern intertemporal equilibrium analysis of monetary phenomena with Wicksell's writings of a century ago. I have instead been struck by how modern the concerns of the Wicksellian literature still appear, especially by comparison with much writing about monetary issues that came later. But if there is a point to continuing to think about the meaning of the Wicksellian notions such as the natural rate of interest, it will have to be because they can be recast in ways that make them useful in addressing the problems of today. I have offered a proposal as to how that might be done. Its success will depend, crucially, on how useful the framework proposed in my book proves to be in illuminating the problems of central banking in the twenty-first century.

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