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A NEOWICKSELLIAN IN A NEW CLASSICAL WORLD: THE METHODOLOGY OF MICHAEL WOODFORD'S *INTEREST AND PRICES*

BY
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Michael Woodford's *Interest and Prices: Foundations of a Theory of Monetary Policy* (2003) is an important book. Woodford's title is, of course, a conscious revival of Wicksell's own famous work and it points to an effort to recast the analysis of monetary policy as centered on interest rates. I believe that Woodford's theoretical orientation is essentially correct. In repairing to Wicksell, he places the monetary aggregates into a more reasonable perspective, correcting the distortions of the monetarist and Keynesian diversions with respect to money. My money is, so to speak, where my mouth is: My own textbook-in-progress is also based around an IS/interest-rate rule/AS model, in which financial markets cleared by price rather than the LM curve are emphasized.[1] Such an approach, as Woodford notes, has become standard in central banks, but has not yet captured either core undergraduate or graduate textbooks and instruction. My task here, however, was not to praise Woodford's economics nor to trace or evaluate its Wicksellian routes, but to consider *Interest and Prices* from a methodological point of view.

Let me begin with some flavor of the book. Woodford writes:

> The present study seeks to provide theoretical foundations for a rule-based approach to monetary policy ... [in which] more emphasis is given to explicit commitments regarding desired economic outcomes, such as a target rate of inflation, than to particular technical indicators that the central bank may find it useful to monitor in achieving that outcome ... The development of such a theory is an urgent task, for rule-based monetary policy in the spirit that I have described is possible only when central banks can develop a conscious and articulate account of what they are doing. It is necessary in order for them to know how to act systematically in a way that serves their objectives, which are now defined in terms of variables that are much further away from their direct control. It is also necessary in order for them to be able to communicate the nature of their systematic commitment to the public (*Interest and Prices*, pp. 2-3).

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[1] *Applied Intermediate Macroeconomics*, under development with Addison-Wesley.

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It is worth holding in mind that Woodford sees his objectives as both urgent and practical.

Economically, *Interest and Prices* is a highly innovative work. Methodologically, it could hardly be closer to the mainstream of modern American macroeconomics: as my title suggests, it is at home in the new classical world. Indeed, the major trope in Woodford's methodological rhetoric is that his new wine is served in old bottles: The neo-Wicksellian IS/interest-rate-rule/AS model is derived "from explicit optimizing foundations. In this way it is established that a nonmonetarist analysis of the effects of monetary policy does not involve any theoretical inconsistency or departure from neoclassical orthodoxy" (*Interest and Prices*, p. 238).

Woodford's economic innovations include making the case for inflation targets over the alternatives: unemployment or GDP targets. His reasoning is, first, that real quantities may differ from trends in ways that are optimal given circumstances, so that simple measures of the output gap, for instance, may prove to be misleading. On the one hand, the problem might be solved through a more appropriate, more economic measure of potential output than a simple statistical trend. Woodford makes the case for such a measurement. But more importantly, Woodford assumes that individual prices are sticky. This is, in itself, hardly a theoretical innovation—even the founders of the new classical macroeconomics, such as Lucas and Sargent, have come to see that the assumption of sticky prices is essential if models have any hope of capturing observed economic behavior. But Woodford focuses on the fact that some prices are more flexible than others, so that generally rising prices are also bound to be associated with efficiency-sapping relative price changes. Low, steady, predictable inflation is, therefore, likely to be economically efficient. And the inflation rate is likely to be a good measure of the inefficiency induced by monetary and fiscal policy.

Interest and Prices takes the methodological presuppositions of the new classical macroeconomics for granted. The principal problems faced by macroeconomic policy analysis are the Lucas critique and the problem of the dynamic (or intertemporal) consistency of policy actions. Woodford, like Lucas, Sargent, Kydland, and Prescott before him, locates the solution to these problems in microfoundations—a:

> model of the monetary transmission mechanism with clear foundations in individual optimization ... allows us to evaluate alternative monetary policies in a way that avoids the flaw in policy evaluation exercises using traditional Keynesian macroeconometric models stressed by Lucas (1976); and the outcomes resulting from alternative policies can be evaluated in terms of the preferences of private individuals that are reflected in the structural relations of one's model (*Interest and Prices*, pp. 10, 11).

Woodford does not employ the economist's favorite slur, *ad hoc*, but he does note the superiority of his (and new classical) models in not appealing to "mechanical" descriptions of wage and price formation. Most of the machinery employed in Woodford's analysis of models and policy rules has been standard since the early days of the new classical revolution. This includes *inter alia* the evaluation of policy in terms of loss functions expressed as discounted quadratic forms in deviations of inflation from target and output from potential, and the imperfectly worked out, casually applied marriage of the vector autoregression and calibration methods as a way of bringing data to models.

Within this framework he provides a searching and synoptic account of policy analysis. Ambitious in design, it draws together many threads from his own and others' work. Among other issues, he deals with finding a middle ground between dynamically inconsistent discretionary policies and the complete, once-and-for-all, state-contingent policies of Kydland and Prescott; integrating imperfect competition and learning into policy models; Ricardian and non-Ricardian fiscal policies (including the fiscal theory of the price level); and introducing some rigor into the *quantitative* analysis of policy.

Ambitious in design, Woodford is nonetheless modest in his claimed achievements. As he rightly points out, it is not enough that a model be derived from an optimization problem for it to be correct. But recognition that the models in *Interest and Prices* are too simple to be realistic or to give direct policy advice to central banks is, he argues, no objection to his project. Rather, it suggests the need for careful research. His book can then be seen as an intensive exploration of the model space. It is a thick book, because he rings the changes of a large set of modeling elements on the assumption that the lessons learned will form the basis for the kind of research that will construct the realistic models that central banks so urgently require. It is not an end, but a beginning.

Now, however, I would like to inject a skeptical note about Woodford's methodological orientation—particularly about the project of microfoundations for macroeconomics. I know from long experience that within the circles in which Woodford normally travels, the idea that one might question the need or desirability of microfoundations or even the broad outlines of the type of theory that is offered as providing microfoundations will often be met with stares of blank incomprehension or a dismissive wave: "no use in listening to fools." But as the old saying goes, "I don't know who discovered water, but it damned sure wasn't a fish." The methodological presuppositions of modern, American macroeconomics are so strong that it is hard to notice them at all much less to question them. Once one tests the waters and finds them pleasing, once one accepts the methodological premises, then *Interest and Prices* is a *tour d' force*. Let me, however, take the view from the water's edge.

Woodford's strategy is explore the current theoretical toolkit with the promise that, once understood, these tools will be useful in future in building practical useful policy models. He adopts what is, essentially, a representative-agent framework. I say "essentially" because, though there is a measure of stylized heterogeneity in the modeling (for example, in the appeal to monopolistic competition), there is no agent-by-agent modeling of the sort that would really qualify as microeconomics. That much is obvious. Why then mimic the forms of microeconomics? Why postulate a representative agent who takes the GDP of the whole economy as an argument of his utility function? There appear to be at least two possible justifications.

First, hope. Woodford is explicit in saying that the models in the book are inadequate to practical policy analysis because they are insufficiently realistic. One might hope that pursuing ever greater realism of models handled according to standard microeconomic principles will eventually end up in models that are adequate to the needs of central banks. Woodford does not give an indication of how far such models might have to be developed or even whether he expects them to be developed along the margin of increasingly individuating individual agents. Many macroeconomists pay lip service to the notion of such development, but to me—as I will argue more presently—it seems like a fruitless enterprise and not one that has actually engaged much of the profession.

To understand the second justification, recall that Woodford sees his project as both urgent and practical. If a central banker asks for advice today, would Woodford give it? And on what basis? If the practical payoff of his research is only in its utility for some future analysis, then he should decline to give advice on the basis of *Interest and Prices* and related research. I will not do him the injustice of accusing him of adopting a position that contradicts his professions; I have no basis for that. But I have heard it not infrequently expressed by economists of similar methodological convictions that the representative-agent model is but the starting point for a series of fuller and richer models that eventually will provide the basis for an adequate macromodel, and that, therefore, the current generation of models is entitled to credence. I call this *eschatological justification*: 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. To state this argument is enough, in my mind, to dismiss it. But I do not want to belabor this point. Instead, I am more concerned why either justification appears to be persuasive.

The temptation to adopt representative-agent microfoundations comes in part from a misapprehension of the Lucas critique. Woodford, in common with many economists, sees Lucas's message primarily as one about how models go wrong when they inadequately capture expectations. The standard solution, rational-expectations, requires a detailed understanding of the mechanisms generating economic outcomes. Now, almost everyone will agree that systematic and persistent deviations of realized and expected outcomes, whether individual or aggregate, suggest missed economic opportunities. Modeling policy on the basis of exploiting such opportunities is unlovely and disrespectful towards people's economic capacities. (Notice the tension in Woodford's standard assumption of rational expectations—to a first approximation, people act as if they know the model—and his advocacy of transparency about policy and policy rules on the basis that this prevents misunderstandings and helps people to form accurate expectations.)

But the real source of the Lucas critique is more general than expectations. As Marschak (1953), one of many economists who anticipated Lucas observed, people who face a previously experienced range of policies in stable economic contexts may behave in ways that we can predict without (or with limited) structural understanding of the economy. Lucas provides just one type of example of what happens when either the range of policies is expanded or the environment is unstable.

Woodford repeats the Lucasian mantra of models based in individual preferences and technological constants. Yet, why should we think that the representative-agent reaches this bedrock? In the nineteenth century, Quetelet tried to describe the dominant social tendencies in terms of the *l'homme moyen* ("the average man"). As an analytical construct, the average man was seriously defective. He was, for example, not a man, but a transsexual with fractional (and also transsexual) children. The representative agent is rather like that himself: he has neither modal nor median properties, but properties that no agent could have. This is proof that he is not an agent who could legitimately participate in a microeconomic optimization exercise. Keynesians were stigmatized for dealing only in aggregates, but the representative-agent is nothing else but an aggregate in microeconomic drag.

Serious microeconomic theory (*inter alia* in the work of Gorman (1953), Debreu (1974), Sonnenschein (1972, 1973), Mantel (1974), Kirman (1992), and Felipe and Fisher (2003)) makes it clear that there is no reason to believe that macroeconomic aggregates should behave like microeconomic quantities. Individual, well-behaved demand or production functions do not, except under very special and very implausible conditions, aggregate up to analogous functions at the macroeconomic level. Well-behaved macroeconomic functions do not imply that the individual behaviors that support it are analogous. Individual Leontieff production functions might aggregate to Cobb-Douglas or Cobb-Douglas to Leontieff. In such circumstances the representative agent is not a useful mechanism for getting at individual behaviors, if that is what one thinks is needful.

In any case, many economists seem to read the Lucas critique as if it implies that we can protect against non-invariance simply by applying microeconomic theory. But, of course, what it really implies is that we are safe if we can truly model the underlying economic reactions to policy. Are we confident enough in the highly stylized microeconomics of the textbooks to find the promise of security in the theory itself, absent convincing empirical evidence of its detailed applicability to the problem at hand? I think not. But there is an alternative, pragmatic approach.

Within the constrained world of the completely specified optimizing model, the Lucas critique is a theorem. But like any mathematical theorem, it is applicable to empirical reality only to the degree that reality fulfills its premises. The admission that models are not yet realistic enough acknowledges that it is unlikely that the premises of any of our simple representative-agent models are well supported empirically. The world is complex; the models are simple. Models might nonetheless be used to identify the kind of considerations that could lead to non-invariance. Then, theoretical and empirical models might be pursued that allow for these considerations without the pretense (and it is important to remember that it is always a pretense) of getting down to individual optimization. This is not a particularly radical suggestion. The earliest new classical models that merely grafted rational expectations onto IS/LM models were very much in this spirit. The proof of the pudding is in the empirical eating. The Lucas critique itself has been subject to empirical test, and frequently found to be less of a worry in practice than in new classical theory (see, for instance, papers by Favero and Hendry (1992), Ericsson and Irons (1995), and Estrella and Fuhrer (2000)).

One reaction to this kind of suggestion is to reject it as *ad hoc*. But *ad hoc* merely means "for the purpose," and generally a purpose-built tool, while less flexible, will do a better job than a less specialized one. In any case, the charge of adhockery is leveled so selectively as to strain credibility. A model that assumes a mechanical rule for price dynamics or that fails to posit a representative-agent optimization problem is stigmatized as *ad hoc*, while one that posits a representative-agent (despite the lessons of aggregation theory) or perfect competition or stylized monopolistic competition with identical competitors, or fixed schedules of price-setting (all assumptions of the type used in Woodford's models) are assumed to be principled implementations of secure microeconomic theory. Models are models; they must leave things out; they must make simplifying assumptions. We can speculate on which are important and which are innocuous. In the end, only data will tell.

Another justification for the strategy of *Interest and Prices* is the desire to evaluate policy rules through welfare analysis grounded in individual preferences. This raises a set of issues closely related to those associated with the Lucas critique. I must confess that it is a puzzle to me how Paretian welfare economics ever survived the 1950s and why the lessons of that decade were lost on later generations of economists. The first lesson comes from Arrow's (1951) impossibility theorem: under reasonable assumptions preferences do not aggregate. As a result, the representative-agent's utility function cannot be thought of as ranking the outcomes of policy in a manner that deeply reflects those of individual agents.

Second, if Arrow were the only problem, we might still appeal to the weak notion of Pareto efficiency. General equilibria can be shown to be Pareto efficient only under rather strict conditions (e.g. perfect competition) that do not well describe our own economy. Lipsey and Lancaster's (1956/57) general theory of the second best demonstrates the deeply pessimistic (for an advocate of Paretian welfare economics) proposition that when we depart from these strict conditions (i.e., almost always) we are rudderless and cannot guide the economy to an efficient outcome.

The upshot of these considerations is that it makes little sense to regard welfare analysis of monetary policy rules as relating to the direct preferences of individuals absent some empirical evidence about those preferences. Any utility or loss function used in such an exercise is the utility or loss function of the policymaker and not of the private sector or any of the individuals it comprises. If it reflects the desires of the individuals, it does so through the paternalistic eyes of the policymaker. This is not necessarily a bad thing. Policymakers may make better policy by conducting surveys or gathering other evidence about individual preferences, working out quantitative consequences, and trying to maximize favorable outcomes. How they aggregate those preferences and what weights they give them are political decisions. They cannot be made easier or more neutral by the pretense that the utility function faithfully represents the preferences of the private sector.

I have naturally concentrated on those issues on which I feel Woodford and I disagree. At the conclusion, I would reiterate that, in spite of those disagreements, which are hardly particular to Woodford but apply equally to most of modern, American macroeconomics, *Interest and Prices* is a valuable book and should be read by every serious monetary economist. In the end, our differences are ones of outlook. At least with respect to monetary policy, Woodford sees the primary problem as one of providing a theory rich enough to build models that are needed for policy analysis. In contrast, I see the problem as one of having a rich enough characterization of the data. For me, microeconomics will never be more than suggestive for macroeconomics. There is more to be gained from embracing macroeconomics and rigorous data analysis than from a pursuit of the chimera of microfoundations.

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