

Whither Macro?

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I.

When I first began serious study of macroeconomics, twenty years ago at the London School of Economics, the field was in serious disarray. (Me being who I was, that was part of the attraction.) The Keynesian consensus of the 1960s was long gone, eroded equally by pressure from inside and from outside the economics profession. Within academic circles, the New Classical alternative of Robert E. Lucas and his collaborators had already replaced Milton Friedman's loyal monetarist opposition as the most significant challenger, and the Real Business Cycle initiative of Ed Prescott and others was already in the air. Outside academic circles, the dismal economic performance of the 1970s had eroded confidence that economists knew how the economy worked, much less how to employ available policy tools to make it work better. Unthinkably drastic measures were being undertaken (Paul Volcker's tight money disinflation in the United States, and Margaret Thatcher's attempt to dismantle the welfare state in Britain) and no one knew how it would all turn out.

Twenty years later, the field has been put back in some order. There is, we are told, a central "core of usable macroeconomics" (Solow 1997), a version of the Hicksian IS-LM as a model of demand driven fluctuation, most firmly established among those academics who have the most regular interactions with the worlds of government and business. The Handbook of Macroeconomics, produced for a more narrowly academic target audience, is more cautious but still optimistic: "the area of common ground is considerable, though we cannot yet announce a 'new synthesis' that could be endorsed by most scholars working in the field" (Taylor and

Woodford 1999, xi).¹ One suspects that the current practical consensus owes more than a little to the 1990s expansion, much as the previous dissension owed a lot to the 1970s slump, but the contrast is nonetheless striking.

Some have gone so far as to claim that the emerging new consensus is just a more analytically solid version of the old (Mankiw and Romer 1991, 15), but this goes too far. The shift from fiscal policy to monetary policy as the preferred tool for economic stabilization is one measure of the distance we have traveled. Another measure is the shift from optimism to pessimism about the degree of stabilization we can reasonably hope to achieve. But even these measures underestimate. What is most remarkable about the current consensus is not how different it is from the consensus during the heyday of Keynesianism, but rather how similar it is to the pre-Keynesian central banker's view of the world. The core of modern usable macroeconomics would, I think, have been recognizable to a man like Ralph Hawtrey, whose Currency and Credit (1919) sought to theorize the role of monetary policy for his own times.

This is a strong claim, and not very obviously supportable, if only because Hawtrey was writing about a gold standard world very different from our own. But was Hawtrey's world actually so different? Certainly so if our standard of comparison is the immediate postwar period, a time of separate nation states, largely unintegrated into any larger world commercial system, still suffering the legacy of Depression and World War. But Hawtrey's world is not so obviously different if our standard of comparison is the current period, especially if we project into the future the evident trends toward globalization and market integration. Nor is Hawtrey's economics so obviously different from current consensus, once we look at the details. Let's look at the details.

¹A technically gentler introduction to the range of views in modern macroeconomics can be

Current practical consensus has converged on “inflation targeting” as the appropriate long run goal for monetary policy, and on the Federal Funds rate of interest (not the money supply) as the appropriate instrument for achieving that and other short run policy goals (McCallum 1999, Bernanke 1999). The consensus is organized around the empirical success of the Taylor Rule equation for fitting aggregate data:

$$R_t = a + b(o_t - o^*) + c(y_t - y^*),$$

where R_t is the rate of interest, o_t inflation, o^* the inflation target, y_t aggregate income, and y^* the income target. Whatever economists may say, the Taylor Rule is what central bankers apparently do, so macroeconomists who seek to influence policy increasingly phrase their arguments within this frame. Note how the Taylor Rule framework permits a clear distinction between long run goals (moving o^* nearer to zero) and short run goals (selecting parameters b and c in some optimal fashion). There is room enough here for everyone.

Increasingly the Taylor Rule is the framework of monetary policy discussion not only in the United States but also around the world, so we can speak of an emerging “inflation targeting” monetary system. The important point is that an inflation-targeting world, in which every country targets inflation and reports its target publicly, is quite similar to a gold standard world. To see this, express purchasing power parity in rates of change as $o_t = \Delta e/e + o_t^*$; domestic inflation equals foreign inflation plus exchange rate appreciation. Take long term expectations and substitute in the inflation targets for long term inflation expectations to get $o^* = E(\Delta e/e) + o^*$; long run expected exchange rate appreciation is just the difference between domestic and

obtained from Snowden and Vane (1999) and Ibanez (1999).

foreign inflation targets. Unlike the gold standard, exchange rates are not fixed, and of course targets are not the same thing as firm commitments. But like the gold standard there is a firm basis for expectations about long run exchange rates, a basis for stabilizing rather than destabilizing speculation.

It will be objected that, unlike the gold standard system with its fixation on short run stabilization of the exchange rate, the inflation targeting framework leaves room for monetary policy to address short run instability of both realized inflation and realized income. But this too is arguably more a difference of degree than of kind. Under the gold standard, central bankers could and did choose whether to respond to a specific gold drain by raising bank rate in order to reverse the drain, or by expanding central bank credit in order to accommodate it. They had, in effect, their own implicit Taylor Rule, albeit without benefit of our modern statistical capability to calculate price indices and measure aggregate income.

One could go even further, to make the case that the literature of central banking is the true origin of macroeconomics as a distinct field of inquiry, and to interpret Keynes' 1936 General Theory of Employment, Interest, and Money as an adaptation of that traditional literature for the unusual conditions of his own time. But this would be a longer argument, and also unnecessary for the issue at hand. For our purposes, it is enough simply to point out that modern macroeconomics has in a sense come full circle back to pre-Keynesian discourse, and that the fundamental reason for that recurrence has to do with the fact that the macroeconomy has also in a sense come full circle.

We are not however right back where we started. First, the economic role of the state has in the meanwhile tremendously enlarged, now encompassing between one quarter and one third of GDP in most developed countries. Obviously, the fiscal consequences of what such an entity

decides to do remain important for macroeconomic aggregates, even if its decisions are not directed by stabilization goals. Second, the integration of financial markets has also advanced tremendously. This is partly because of technology (computers and telecommunications), but also partly because of new financial theory that suggests efficiency gains (a free lunch) from the integration of risk bearing. The modern nation state is large, but the modern market is larger still. These are the significant changes in material fact with which a modern day Hawtrey must grapple.

From this point of view, the current practical consensus strikes me as living too much in the past. To be sure, the enlarged role of the nation state is obvious to everyone. The decreased attention to fiscal stabilization policy has therefore been matched by increased attention to other aggregative consequences of state spending. The literature on the macroeconomics of Social Security is one example, and I predict we will soon see similar literatures on the macroeconomics of our health care and education systems. (That's where the money goes.) The burgeoning literature on endogenous growth is another example of an attempt to grapple with the macroeconomic consequences of the enlarged role for government.

What is missing is adequate engagement with the second defining fact of our age, the worldwide integration of markets in general and of financial markets in particular. The dialogue of macro with finance has so far been mainly a dialogue with corporate finance, one directed mainly toward improving our understanding of the determinants of corporate investment spending, and that's about it. For the most part, macroeconomists' stories about the monetary transmission mechanism (credit channel, net worth channel) continue to theorize a world of quantitatively significant inefficient credit rationing that the financial industry has undertaken to overcome. Macroeconomists' stories about central bankers who set monetary policy in order to

maximize some domestic welfare function largely abstract from the large sums of money dedicated to profiting from artificial interest rate differentials between different countries.

Living and teaching in New York City, as I do, it is entirely possible that I am oversensitive to the importance of financial developments for understanding the modern macroeconomy. If I lived in Europe, likely I would be placing much more emphasis on the challenge rather than the reality of market integration, and focusing more attention on labor markets than on financial markets. But if a New York address blinds one to some things, it opens one's eyes to others.

Most important, one begins to suspect that the foundational presumption of postwar macroeconomics, that the nation state can control its own macroeconomic destiny by using available aggregative policy tools, may seriously misjudge the nature of the world in which we now live. The United States being the largest and most powerful of nation states, that postwar presumption has persisted here longer than elsewhere. And the United States being the center of the world economics profession, the same presumption has persisted in academia longer than in government and business circles. Such academic discourse strikes me as significantly behind the curve of world developments.

II.

One reason academic discourse has fallen behind the curve is that the world has changed. Another reason is that the language we use to understand our current world remains the language we developed to understand the rather different world of yesterday. One way of stating the problem is that our principal theoretical framework for system-level thinking, general equilibrium theory in the mode of Arrow and Debreu, has no place in it for money (Hahn 1965).

This was perhaps not such a problem when macroeconomics was about fiscal policy, and when general equilibrium was considered an idealization of little practical interest for problems of the disequilibrium short run. It is much more of a problem when macroeconomics is (once again) about monetary policy, and when general equilibrium is being promoted as a model of short run fluctuation by proponents of Real Business Cycles. Just when we need it most, we find ourselves without much in the way of fundamental monetary theory to guide us.

For example, given practical consensus on the Taylor Rule with inflation targeting, the question naturally arises whether this set of practical policy rules has any deeper theoretical basis. Woodford (2001) is perhaps the most successful attempt so far to provide such a basis, but there remains a long way to go. His reliance on the ad hoc assumption of a cash-in-advance constraint to produce a demand for money does little to address the fundamental theoretical issue at stake. In his defense, he is only following the theoretical strategy that characterizes the work of all New Keynesians, a strategy that seeks essentially to support and engage the “core of usable macroeconomics”, by fundamental models if possible but also by ad hoc models if necessary. Given the Hahn problem, ad hoc models are all we can hope for if we insist on staying within the framework of general equilibrium theory.

Roger Farmer (1993, 189) frames the problem of monetary economics as it is seen by most economists (see also Wallace 2001):

There are two puzzles that form the core of monetary theory. The first is the question of why a piece of paper that has no intrinsic value can come to be exchanged for commodities that yield utility. The second is why apparently identical pieces of paper that have identical risk characteristics can trade at

different prices. Otherwise stated, the first is the question of why the price of money in terms of commodities is positive, and the second is the question of why the money rate of interest is positive.

This formulation is so familiar to modern economists that it is hard to imagine monetary theory being organized in any different way, but nonetheless that is just what I want to suggest lies in our future. Observe that neither of Farmer's puzzles is at all puzzling in a gold standard world. In such a world, the price of money in terms of commodities is positive because money is convertible into gold which has a positive price. Further, in such a world, the money rate of interest is not driven to zero by intermediaries issuing zero-yield money liabilities against Treasury bill assets, because such money liabilities would also have to be convertible in order to be money. Put another way, no one finds it puzzling that, in a gold standard world, people are willing simultaneously to hold zero-yielding gold and positive-yielding inside credit. Farmer's puzzles are puzzles only in a world of fiat money, not in a world where money is a promise to pay gold.

I have already argued that the emerging inflation targeting monetary system has many of the properties of a gold standard system. One of the consequences for monetary theory is that we need to start thinking of money as a promise to pay. Forty years ago, Gurley and Shaw (1960) drew our attention to the distinction between outside and inside money. For whatever reasons (maybe the Depression era collapse of the gold standard, maybe postwar generalization from special war time practice), postwar economics got into the habit of thinking about government-issued currency as outside money. Gurley and Shaw did not deviate from this aspect of postwar orthodoxy, but their book did attempt to shift emphasis to the inside credit

character of all other forms of money. What I propose for the future of monetary economics is that we go beyond the program of Gurley and Shaw by starting to think of government-issued currency as a promise to pay.

But what is it that money promises to pay? It must be admitted that the answer is not so clear, but the inflation targeting framework does suggest at least the outlines of a possible answer. Since long run inflation targets imply long run exchange rate targets, in effect an inflation-targeting central bank commits to defend the international value of domestic currency in terms of other currencies. In this sense domestic currency is a promise to pay international reserves. The value of such a promise depends on the ability and willingness of the promisor (the central bank) to attract such reserves by influencing the pattern of trade on the current and capital accounts. (The literature on central bank “credibility” is relevant here, even though it typically conflates ability with willingness.)

One reason that economists have historically been reluctant to view currency as a promise to pay is that we feel the need to distinguish government issue from bank deposits. In the older literature, the former is called money and the latter is called credit. In the postwar literature, the former is high-powered (fiat) money and the latter is (inside credit) money. If we begin to treat government money as a promise to pay, don't we risk losing analytical sight of the apparent fact about the world that government money is better than bank money? Quite the contrary. Indeed, by embracing the credit character of government money as well as private money, we are forced to confront the question why government money is better, rather than treating that question as solved by definition. This is the puzzle, if you will, around which a future monetary economics is likely to be organized.

The similarity of this puzzle to Farmer's second may suggest that the answer is similar as well, namely legal restrictions that give special status to government money. No. There are of course many such legal restrictions, but I tend to think they are not the cause of money's special status but rather merely the codification of that status. Put another way, I am sufficiently impressed by the ability of bankers and financiers to evade legal restrictions in search of profit that I would not want to build a monetary theory on the assumption that attempts to evade are always unsuccessful.

Instead, embracing the credit character of government money forces us to confront the question why monetary systems are organized hierarchically. We see this hierarchy in a correspondent banking system, where the deposit liabilities of money center banks are better money than the deposit liabilities of correspondents. The superiority of central bank liabilities over member bank liabilities seems to be much the same kind of phenomenon, and by extension so too the superiority of government issue over private issue. What kind of theory can explain such a phenomenon?

We need, I think, a model of an inside credit payment system in which patterns of payment, across heterogeneous agents and across time, give rise endogenously to hierarchy as an efficient institutional arrangement. The key agents in such a model will be banks, conceived as payment intermediaries using their own balance sheets to span the gap between payer and payee, and conceived also as market makers in their own monetary liabilities. It is in this latter role that banks play a crucial role knitting together adjacent layers in the hierarchy of credit.

The kind of thing I have in mind is similar to Hicks' sketch in his last book A Market Theory of Money (1989). The conception of banks as market makers also suggests a link with the finance literature on "market microstructure". The big difference, of course, is that in the

stock market you cannot sell what you do not own, at least not in the cash market (derivatives markets are another matter). Banks, by contrast, make markets in their own liabilities precisely by expanding and contracting their balance sheets. In the language of finance, bank deposits are nothing more than the open interest in cash.

The payoff from such an effort of theory construction will not only be a more solid basis for monetary theory, and hence also for monetary policy, than that currently provided by the ad hoc cash-in-advance constraint models. The payoff will also be a true integration of macroeconomics with finance that will bring our theoretical constructs into line with the significant facts of our day, and so bring academic discourse back into contact with business discourse.

III.

So far I have told a story in which the development of macroeconomics is driven by the development of the macroeconomy, but with a lag. And I have used that lag to forecast the future of macroeconomics as being about the integration of finance and macroeconomics in order to produce a new more adequate theoretical foundation for monetary economics. That is what I think will happen, and it is also what I think should happen, but it is probably going to take a long time.

In the short run, we are likely to see development following a more internal and professionally driven logic. Existing models will be refined, expanded, confronted with data, and all the usual business of normal science. We will continue living, as we have done these last forty years, in the shadow of the Keynesian consensus of the 1960s. In the past, much of the energy of macroeconomists has been absorbed either attacking IS-LM for its presumed

theoretical inadequacy or coming to its defense with one ad hoc theoretical justification after another. This past is likely to be our future as well, at least in the short run, if only because (as noted above) the practical consensus around a core of usable macroeconomics has been cast in IS-LM terms. It is worth revisiting some of that past history, if only to avoid being condemned to repeat it.

Because IS-LM was understood as a model of fix-price equilibrium, much of its defense involved explanations of why prices may fail to be sufficiently flexible to clear markets. And so we got the spectacle of academic economists jumping through hoops to explain rigidities at the very time that the real economy was becoming ever more flexible and competitive, union membership was on the decline, and American companies were facing ever more competitive international markets. Whatever one might think about the laissez faire policy implications of New Classical or Real Business Cycle models, their insistence on price flexibility and competitive behavior does seem more in line with essential features of the real world, while the New Keynesians seem to be stuck explaining the microeconomic rigidities of a bygone age.

Put another way, the main positive contribution of New Keynesians seems to have been to our understanding of information problems in individual markets (efficiency wages, menu costs, and the like). This explains why New Keynesians, despite their outdated emphasis on rigidities, are able to continue marching behind the flag of “Superior Empirical Relevance” while the New Classicals and Real Business Cycle schools march behind “Superior Analytical Rigor”. But it is by no means clear that these microeconomic problems add up to a convincing story of macroeconomic fluctuation, at least not for the United States. (Europe may be a different story, and it is significant that European macro places much more emphasis on labor market rigidities.)

For macroeconomics, these microeconomic stories play the largely defensive and conservative role of justifying the assumption of sticky prices.

Probably this pattern of discourse will continue as it has in the past, with the main immediate positive result being to increase sophistication of technique both theoretical and econometric. Already the real business cycle program has evolved to embrace multiple equilibria, sunspot equilibria, and complex dynamics. The impact on econometric practice has been equally transformative. Looking forward, agent heterogeneity seems likely to be the next big technical hurdle, its importance mooted by the theoretical dissonance between representative agent modeling and assumptions of market imperfection. The embrace of heterogeneity looks like being an important hurdle from the standpoint of the longer run development of macroeconomic theory as well. Simply put, heterogeneity is of the essence for understanding credit and payments systems. The problem is to find a mathematical structure that is both general and tractable. (See Bewley 1983, and Mehrling 1995, 1998 for one possibility).

IV.

The world will not of course wait for economists, and practical macroeconomics is therefore likely to continue following its own course without much immediate influence on or influence of theoretical developments. We will likely see continued refinement and elaboration of the Taylor Rule, with associated ad hoc theoretical justifications, but there may be room in the practical literature for other things as well. If I am right that the current period represents a return to the pre-Keynesian era of central bank dominance, then it stands to reason that the literature of that earlier era may be worth revisiting. Already the Taylor Rule discourse has

revived the Wicksellian distinction between the natural rate of interest and the money rate of interest.

Because the pre-Keynesian literature proceeds within the framework of the quantity theory of money, before that theory came to be understood in Keynesian portfolio preference terms as a theory of money demand, we may well see a resurgence of quantity theory talk in practical discourse.

Even more, I expect the practical macroeconomics discussion to continue broadening out to include a wider audience. Currently the Taylor Rule discourse involves largely academics and central bank economists. There is room to expand that discourse to include also larger banking and finance circles, and also students whose career goals will take them into banking and finance circles. To engage in that wider discourse, economists will have to learn more about how the practical world of banking and finance actually works, and that will be a good thing for everyone. To get the conversation started, we badly need a modern day analogue to that venerable nineteenth century bestseller Walter Bagehot's Lombard Street: a description of the money market (1873). The closest thing available is Marcia Stigum's The Money Market (1990), unfortunately now somewhat outdated but still the most useful desk reference on how the markets actually work.

Three years ago I started using Stigum as the main textbook for my undergraduate class on the Economics of Money and Banking. The students found it tough going, and so did I since it forced me to produce entirely new lecture material. Ultimately however they thought the experience was worthwhile, and so did I. I came out of the course even more convinced that the future of macroeconomics depends on engaging with the facts about how developments in financial markets have transformed our world.

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