

Whatever became of the Monetary Aggregates?

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By Charles Goodhart

My title intentionally harks back to Maurice Peston's slim, but excellent, 1980 book, entitled Whatever happened to Macro-economics. In this book, a compilation of three lectures, Maurice asks how much then remained of traditional Keynesian macro-economics in the aftermath of the Monetarist counter-revolution, and of the development of Lucasian rational expectations. Maurice was much more impressed by the new contributions of the rational expectations school than he was of those by the more traditional Monetarists.

Indeed, time has appeared to prove Maurice to be correct in this appreciation. After all, the monetary aggregates, the money supply in one, or other, of its various guises, should presumably play a major role in any monetarist scheme of affairs. As Mike Woodford has recorded, in a recent paper,

“[N]owadays monetary aggregates play little role in monetary policy deliberations at most central banks.”

In contrast, a detailed treatment of expectations, and how these may be generated, lies at the heart of the current Neo-Keynesian analysis.

My own thesis is that this downgrading of the role of the monetary aggregates in current models, and in forecasting future inflation, has gone too far. At this point a couple of personal caveats may be in order. I am far from being a card-carrying monetarist. Not only did I strenuously oppose Friedman's monetary base control mechanism and his K% rule for monetary growth, but I have been credited, though without much justification, for example in The Times obituary of Milton Friedman, (November 17, 2006), with having undermined monetarism by pointing out the likely instability of demand for money functions when turned into targets. That said, I was shocked when successive Conservative governments in the 1980s and 1990s could pass almost seamlessly from the view that control over broad money was the essential centrepiece of macro policy, the Medium Term Financial Strategy, to subsequently paying little, or no attention to monetary developments in later years. By the same token I am concerned that some of the key features of a monetary, and of a truly Keynesian, economy are ignored in the neo-classical (neo-Keynesian) consensus.

A second caveat is that Woodford, and his supporters at the recent ECB Conference in Frankfurt (9/10 November, 2006) on 'The Role of Money', such as Uhlig and Gali had, I believe, two separate purposes. The first was to deny the benefit of having a separate monetary analysis, the famous two pillars of the ECB. In so far as monetary effects were important in preparing forecasts, and deciding policy, they should be integrated into a single, overall analysis of the prospects for the economy. I have no quarrel with this. It is the further second line of argument, that we can in practice virtually ignore developments in the monetary aggregates in the conduct of monetary

policy, focussing instead solely on a key policy determined interest rate, that I do want to question.

The starting point for Woodford (2006), as also for a somewhat similar, earlier article by B. McCallum, (2001), ‘Monetary Policy Analysis in Models without Money’, is the basic, now widely accepted, new Keynesian model of three equations, consisting of an IS type aggregate expenditure function, a Phillips-curve type aggregate supply function, and a Taylor-type Central Bank reaction function, showing how Central Banks set interest rates.

In both the IS and the Phillips supply curve, expectations play a leading role; and whether they are forwards or backwards looking, rational or bounded, forms the core of a huge literature, but one which is not relevant to my thesis here. So I shall more simply write these as:-

$$\tilde{y} = y_t - y^* = f(E\tilde{y}, R - E\pi) + u_t \quad 1 \text{ (IS)}$$

$$\pi_t = f(E\pi, \tilde{y}) + v_t \quad 2 \text{ (AS)}$$

where \tilde{y} is the output gap, y is current real output, y^* is the natural, or equilibrium, or sustainable level of output, R is the nominal interest rate, E is the expectations operator, π is the rate of inflation, and u and v are error terms.

This is complemented by the Taylor type reaction function:-

$$R_t = a + b_1(\pi_t - \pi^*) + b_2\tilde{y} \quad (3)$$

Where π^* is the target inflation rate.

Let me make two peripheral brief comments on this. First, it seems odd that the private sector is shown as responding to future expectations, but in equation 3 the Central Bank appears to be reacting only to current events. This latter is surely wrong. All inflation targeting Central Banks make, and respond to, inflation forecasts. I have tried elsewhere to show that a proper forward-looking specification of reaction functions can make a large difference to the estimated coefficients, (Goodhart, 2005).

Second, this three equation model may only work satisfactorily during relatively calm periods of stable economic developments, a 'fair weather' model. The zero bound to nominal interest rates suggests that this model may have limited usefulness during periods of deflationary pressures. Moreover, during turbulent periods, whether of severe deflation or inflation, expectations will not be anchored, will differ quite markedly from person to person, and be subject to potentially rapid and sharp revision. Under these circumstances one cannot assess what real rates of interest may be, so growth rates of the monetary aggregates may well be a better guide to the effects of monetary policy on the economy than either nominal, or an estimate of real, interest rates.

Be that as it may, this three equation model determines the level of interest rates, (real and nominal), the output gap, and both inflation and the price level, (given the inflation target and the starting point). The system has a determinate equilibrium, so long as the Central Bank reacts sufficiently strongly to inflation in adjusting nominal interest rates. There appears to be no need to look at what is happening to money in

this system to achieve the main macro-economic variables of importance to social welfare.

In practice, however, money can be, and generally is, present in this model, since a demand-for-money function is fully consistent with the above three equations, as in equation 4:-

$$M_t/P_t = f(y, R, \pi) + w_t \quad (4)$$

where P_t is the price level and w_t another error term. Note, however, that so long as the Central Bank sets interest rates, as is the generality, the money stock is a dependent, endogenous variable. This is exactly what the heterodox, Post-Keynesians, from Kaldor, through Vicky Chick, and on through Basil Moore and Randy Wray, have been correctly claiming for decades, and I have been in their party on this. Certainly if the demand for money function fits perfectly, and if its arguments are correctly set out in equation (4), then you learn nothing more from looking at money, than you already knew from looking at inflation, output and interest rates.

There is a minor caveat to this, which is that money stock data, or elements of it, may come out earlier, or be less subject to (initial) measurement errors than data on output. If so, M could act as an early indicator variable for y , or even for P_y . There have been a few instances of such indicator relationships; for example fluctuations in cash holdings (and $M0$), at least for a time, seemed to precede movements in personal consumption in the UK. I would not, myself however, want to put much weight on any such leading indicator properties.

But much the same is true of output, or of inflation; they too are endogenous, dependent variables. If their functional relationships held perfectly, then you would learn nothing more from looking at output, or inflation, outcomes than one already knew from a knowledge of its functional (structural) relationship. In reality, however, a great deal of time and effort is spent analysing whether the deviation of output/inflation from forecast is due to one kind of shock, or another, e.g. transient or permanent, demand or supply shock. Why do we not care as much to shocks, to deviations of the money stock, from its expected value?

One great advantage of Central Banks running monetary policy by setting interest rates, rather than via the monetary base, is that demand-side shocks to desired money holdings are automatically accommodated. So if one should assume that all monetary shocks are demand-side, then movements in the money supply indeed tell one nothing. Note, however, that demand-side shocks to the real economy are also relatively easy to handle, in theory at least.

But it is not true that all shocks to money are demand-side. The bulk of money is in the form of commercial bank liabilities, and banks can behave very differently over time. The form of their liabilities, their capital base, their confidence, their risk appetite can and does alter over time, both cyclically and more permanently. The whole question of whether certain segments of the economy can access funds beyond their current income depends crucially on the behaviour of the banks. If there is a supply shock to money, with certain groups now getting more, or less, access to funding, for example when banks provide mortgages to a wider group of households

on easier terms, will this not feed back into the IS curve? Of course it will. I shall revert to this in a deeper format later on.

But, first, I want to turn to two other issues. The first of these relates to the much closer long-run, low frequency, relationship between money and prices than is observed at higher frequency, shorter run periods, when the relationship is obscured by a variety of shocks. Michael Woodford accepts in his Frankfurt paper, slightly grudgingly for my taste, this longer term relationship, but argues that it is an inherent consequence of having a reasonably stable long-run demand-for-money function.

Thus, if we first difference equation (4),

$$\mu_t - \pi_t = b_1 dy_t + b_2 dR_t + dw_t \quad (5)$$

(see his equation 3.3), it can be easily shown “that every term on the right-hand side of (3.3) is stationary, so that $\mu_t - \pi_t$ is predicted to be stationary.” So the growth rates of the money stock and of prices must, in the medium and longer run, move in tandem.

That is surely correct, but Woodford then goes on to argue that looking at inflationary outcomes by themselves is just as good, or better, than looking at (longer term) monetary outcomes by themselves, or even better jointly. This latter is not demonstrated by his model. Thus he attacks the Gerlach (2003, 2004) two pillar Phillips curve estimate of inflation by arguing that the cointegration of inflation and money means that money growth must be correlated in the longer run with inflation; but the real question is whether attention to monetary trends adds anything beyond what is also already visible in inflationary trends.

If two variables are cointegrated, then if they begin to deviate, there must be some forces, or factors, restoring the relationship. But these forces, or factors, may impinge primarily, or even perhaps solely, on one, or other, of the two variables. One reason for the emphasis that monetarists place on the stylized fact that this long-run relationship between monetary growth and price inflation continues to hold despite there having been many differing money supply regimes is that it makes it much more difficult to believe that the error correction mechanism is solely, or overwhelmingly, from money holdings adjusting passively to pricing developments, rather than vice versa. Of course, this is not a completely knock-down argument. Even under the gold standard there were numerous forces leading monetary growth to adjust to transaction requirements, for example via incentives to find more gold and/or financial substitutes for gold when monetary conditions had become tight. Even so, the likelihood that all such adjustment has been via passive monetary accommodation, without any adjustment of inflation to monetary forces, especially in hyperinflationary circumstances, seems highly improbable.

A subsidiary argument is that it is really inflation, and not the statistics on the rate of growth of M3 or M4, that we really care about. So, even if there should be some effect of excess money balances, (in the sense of money balances well above that consistent with desired low inflation and sustainable output), on subsequent output and inflation, we can still wait to see if it does actually feed through into higher inflation, and then take countervailing action.

But even if one should agree with this 'wait and see' argument, the faster past growth of money should at least be a warning that the future monetary policy decisions of the

Central Bank might need to be more restrictive, in the sense of higher interest rates held for longer, than might otherwise be the case. Furthermore the ‘wait and see’ position depends on a number of arguable propositions, for example,

1. that demand shocks to money holdings are much more prevalent (and longer lasting?) than supply shocks;
2. that lags between monetary policy action and its effect on inflation are short enough that one can afford to wait until inflation actually appears in the data; and/or that the extent of monetary action necessary to stabilize inflation (once it has started – after a lag – to move away from target) will not then destabilize the financial system and/or the real economy;
3. that we can, and do, measure inflation correctly.

Unlike most other economists, I do have doubts whether we measure inflation correctly. A question arises how far, if at all, asset price increases, especially of housing prices, ought to enter the index from which inflation is measured. For a variety of reasons, monetary expansion may be more closely related to asset price inflation, than to the inflation of goods and services prices. If inflation is (incorrectly) measured, to exclude all such asset price inflation, then the links between money growth and (true) inflation may be understated.

Moreover, in the longer run there should be cointegration between wealth and income/expenditures. In so far as monetary fluctuations are closely associated with those in wealth-holdings, then the resultant disturbance to the wealth/income ratio is likely to have consequent effects on income flows and expenditures, and in due course on goods and services inflation.

At this point I begin to move away from the simple three/four neo-Keynesian equation system with which we began. The first amendment, naturally, from what I have been saying already, is to put asset prices/wealth into the model. For obvious reasons, the wealth/income ratio should be an argument in the expenditure function, and wealth should probably be the scale variable in the demand for money function. Finally there should be asset demand and supply functions, where the demand for assets may in turn be a function of shocks to the supply of money, as well as to expectations of future earnings, of future monetary policies and of future asset prices themselves.

That said, I doubt if anyone connected with monetary policy would deny the effect of the housing market, and of the equity market, on the forecast future for output and inflation. But why should we go any further than that, and link the process back to monetary growth specifically. This also raises the much debated issue of whether Central Banks, or anyone else, should be concerned about asset prices, except as they impinge on forecast values of real output and inflation.

Amongst the main arguments against using monetary policy to offset asset price fluctuations are:-

- I. that asset prices do not all move together in lock-step, and
- II. that a member of an MPC is never in a strong position, *ex ante*, to claim that any particular asset price is out of line with fundamentals (a bubble).

But so long as there is a reasonably close relationship between monetary growth and asset prices in general, at least over some periodicities, these objections can be side-

stepped. The authorities are then responding to an excessively fast rate of growth of monetary balances in general, and not to any particular set of asset prices, for example to M3 growth in the eurozone rather than to housing prices in Spain, a point that Otmar Issing has made several times (2002 and 2005).

Let me turn, finally, to my main point. This is that the so-called neo-Keynesian basic model is based on inter-temporal utility maximisation by a representative agent, based on the assumption that all debts are ultimately paid in full, otherwise known in the jargon as the transversality condition. But this means that everyone is perfectly credit worthy. Anybody's IOU can, and would, be accepted in exchange. There is no need for commercial banks, and there are none in Woodford's iconic book, Interest & Prices. Indeed it is hard to see why there should be any need for a specific monetary asset, since everybody's IOUs can be used for exchange purposes. All fixed-interest financial assets are effectively identical, and there is one single interest rate in any period, though it may shift over time as borrowing and savings propensities alter. Moreover nobody, and no firm, is liquidity constrained, ever. Indeed the conditions necessary for a no-default system to operate, either complete financial markets for every possible contingency or perfect information, are, I believe, identical to those that will allow a full Arrow-Debreu-Hahn Walrasian equilibrium to operate. As we know, money is not necessary in such a system.

Thus, by basing their model on the transversality condition, the Neo-Keynesians are turning their model into an essentially non-monetary model. So it is no surprise that monetary variables are inessential in it. In reality, many agents in the economy, both persons and smaller companies, cannot sell assets, since they do not have sufficient

saleable assets, or borrow, except at exorbitant interest rates. They are effectively liquidity constrained, with their expenditures limited to their current income and their few current assets. As Maurice Peston write in his 1980 book (Chapter 1),

“In Keynes’s General Theory consumption is determined by income to a very considerable extent because the latter constrains the former. The poor household has no liquid or marketable assets and can hardly borrow. It can only spend its income.”

It is that constraint that modern Neo-Keynesian theory assumes away. Perhaps as we all become richer, and come to own more assets, such constraints will in practice bind less and less, and then money and commercial banks – and traditional Keynesian analysis – will indeed become less important. But I do not believe that that time has yet come. For a recent excellent empirical article on this, see Nier and Zicchino, (2006). For the time being, the degree to which the current income, plus liquid asset, constraint bears on current expenditures depends to a considerable extent on the willingness of, and the terms on which, banks will lend to the private sector. This is a key reason why I believe that the rate of growth of bank lending to the private sector is as, or a more, important monetary aggregate than broad money by itself. Obviously it makes no real difference whether an established company sells a bond to, or raises a loan from, a bank, but a small company, or person, can usually only borrow from a bank, and then only in loan form.

So, shifts in bank willingness to extend such loans, as banks become more, or less, risk averse, will have the effect of shifting the constraints affecting the economy. In

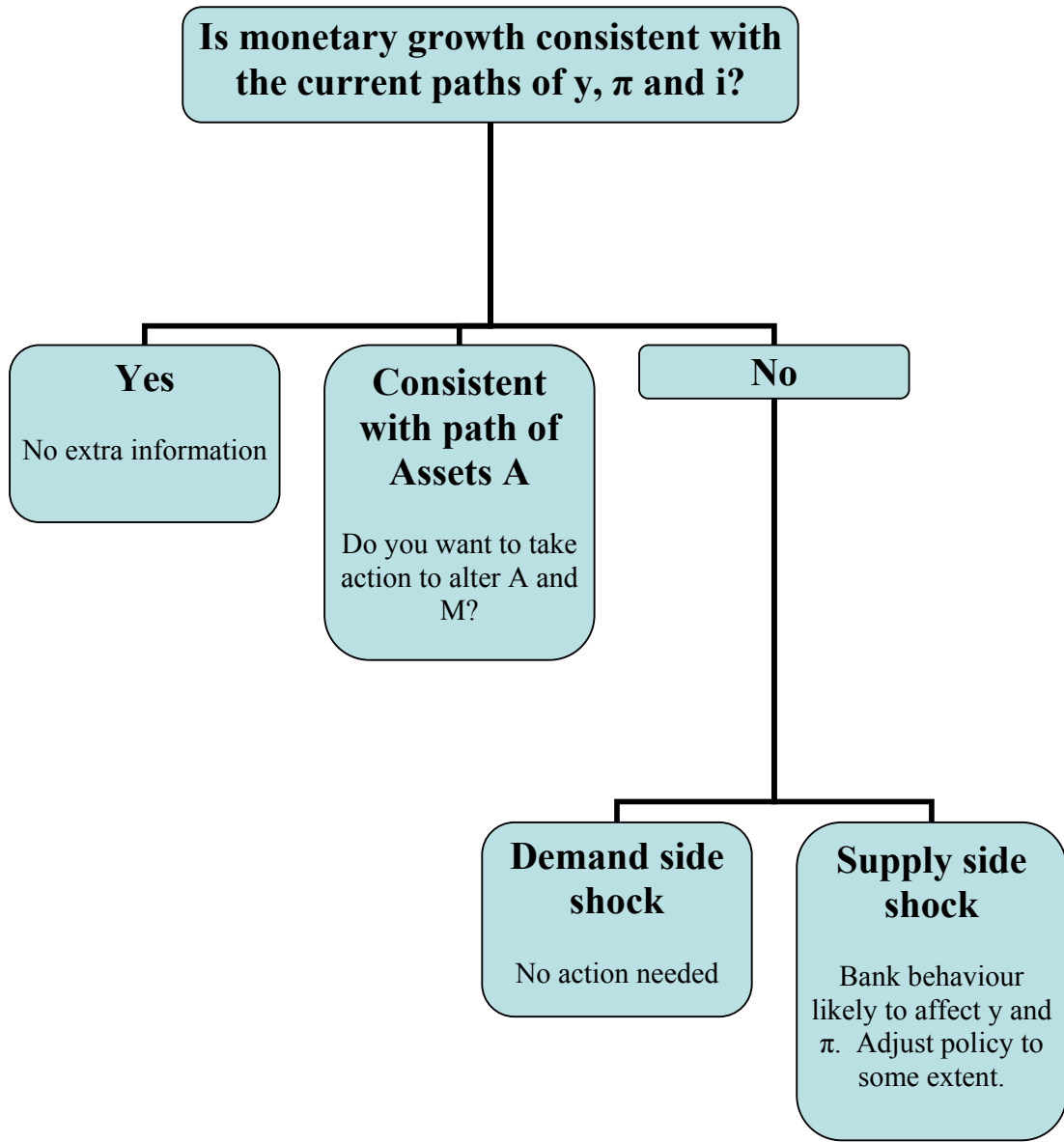
particular, when the growth rate of the money stock is declining, whole segments of the economy that were previously not income constrained may suddenly become so, and at a time when income is probably also dropping.

Furthermore, when default becomes possible, risk premia come into play. There ceases to be one single interest rate, as in the basic Neo-Keynesian model, but a whole schedule of interest rates, depending on the perceived riskiness of the borrower. Generally in depressions interest rates on safe, liquid government debt instruments decline, but risk premia rise. It can then be ambiguous whether, overall, interest rates have risen, or fallen. The reverse is true in booms; the official policy rate may rise, but risk premia may fall. Against this background it would be short-sighted not to cross-check for the combined effect that a combination of official policy measures and changing risk aversion may have by looking carefully at the time path of the monetary aggregates.

As Lord Peston asked (ibid), again in Chapter 1, “How far was the problem of [achieving and maintaining] full employment one of dealing with or failing to cope with risk and uncertainty.” I agree, but a measure of the willingness to face such risk and uncertainty is given by evidence of the growth rates of the money and credit aggregates. Keynesian economics emphasised income constraints and risk and uncertainty. I have argued here that evidence on how the economy is coping with these factors can be given by examining the growth rate of the money and credit aggregates. In my view anyone who believes that default, risk aversion and income constraints matter, whatever brand of Keynesian or Monetarist, ought to concern themselves with the messages emanating from the monetary aggregates. To be sure

such messages are often garbled by noise, especially from short-run demand shocks, so that such interpretation will be an art. Nevertheless it is an art worth attempting.

Let me now conclude with trying to set out a decision-tree on how to respond to monetary data. By now the diagram should be easy enough to follow. The harder part, no doubt, is to know at any time exactly in which box we find ourselves.



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