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WHY DO AGENTS HOLD MONEY, AND WHY DOES IT MATTER?*

by

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SOME PRELIMINARIES

Let me begin this paper with a confession of academic incorrectness, followed by a plea of mitigating circumstances based on unoriginal methodological considerations. The confession: I attach less importance than do most monetary economists to that latest manifestation of methodological individualism, namely 'sound microeconomic foundations', for monetary theory. I am not against deducing conclusions about macroeconomic phenomena from the maximisation postulate; and I would treat with extreme scepticism any macroeconomic hypothesis that could be shown to be inconsistent with economic rationality. Even so, maximising premises are not the be all and end all of economics. They are a commonly used and often useful starting point for generating testable hypotheses, but they are not the only valid starting point.

It is testability, and success or failure when tested, that matter for an economic hypothesis, not its conformity or lack thereof to 'first principles'. For the sake of argument, entertain the possibility that there do indeed exist first principles of economic analysis guaranteed to yield true conclusions about all the phenomena that might interest us, but allow that knowledge of them has not been genetically programmed into us. Suppose also that, by some happy chance, we had nevertheless stumbled upon them. How would we know this? The short answer is that we wouldn't, and couldn't. No matter how many times our predictions succeeded, it would always be possible that they would go wrong tomorrow. We are simply not able to distinguish between ultimate scientific success and a situation in which a set of flawed theories just happen to be helping us to get things right for the time being. That being so, it is appropriate always to allow for the chance that the second possibility is the case, to treat our hypotheses with a touch of scepticism, and keep on comparing them to new data as and when these are generated.

This view of things does not rule out the use of micro foundations to generate hypotheses. It
does, however, require us to take seriously hypotheses generated by other means, including creative conjecture (sometimes called ad hoc guess-work, by those discomfited by the means in question) and it also prevents those who derive their hypotheses from micro foundations claiming that they are, by virtue of having done so, exempt from the discipline of empirical testing. It should also go without saying that a methodological principle whose very foundation is the proposition that, even if we have the truth, we can never know that we have it, offers no guarantee of scientific progress. The best that it offers is the prospect of weeding out a few errors.

THE DEMAND FOR MONEY

The untidy attitude towards generating economic understanding which I have just sketched out underlay the large body of empirical work on the aggregate demand for money function which the 1960s and 70s produced. Though such work ceased to be fashionable with the rise of New-classical economics and its pre-occupation with the deep parameters which allegedly define an economy's underlying structure, it did not come to a halt. Indeed it continued to make modest advances, and those advances cast considerable doubt upon the soundness of the micro foundations which were simultaneously being touted as the sine qua non of respectable macroeconomics.

Modern empirical work on the demand for money started in earnest with Milton Friedman's (1956, 1959) papers. Friedman changed the question people usually asked about the subject. Instead of 'why do agents hold money', the issues became 'what variables affect the demand for money, and are the parameters of the function stable over time?'. Instead of a priori reasoning - 'money is a means of exchange, and therefore it bears a stable relationship to nominal income', or 'money is a liquid asset, and therefore its demand is highly interest sensitive and sometimes
unstable' - the preferred investigative tools became statistical.

The micro-theoretic foundations of Friedman's work were reduced to a bare minimum. The demand for real money balances was modelled 'as if' that for a consumer durable good whose services as 'a temporary abode of purchasing power' yielded a stream of utility to the agents holding it. That was enough to suggest that a constraint variable - wealth measured in some way - and one or more opportunity cost variables - real interest rates and expected inflation - belonged in the demand for money function. To go further was, in the words of one commentator '...like analysing the "ice cube" and "cold milk" motives for owning a refrigerator.' (Laidler 1969, p. 57, fn.). Whatever one may think of such casual theorising - and there is no sin against sound micro-foundations more grave than putting money in the utility function - from the point of view of enabling empirical questions to be formulated, it was well up to the task. What we would now term the 'long run' empirical relationship that emerged from Friedman's own work, and those papers which were an immediate response to it - eg. Meltzer (1963), Laidler (1966) - has, as Lucas (1988) showed, held up remarkably well in the face of a quarter century of new data.

Friedman's work did not put an end to questions about why agents hold money, but it did change the way in which they were addressed. Once we had, as it were, a benchmark demand for money function, we could ask whether the analysis of specific motives for holding money enabled us to formulate alternative, perhaps more precise, versions of the relationship with better empirical characteristics. That was surely a large step forward from the a priori speculation about the empirical nature of the demand for money function that had, up till then, dominated the textbooks: - the 'classical' range, and the 'Keynesian' range, of the relationship, the liquidity trap, and all that. In rather short order, the liquidity trap hypothesis was tested and found wanting (Bronfenbrenner and Mayer 1960, Laidler 1966). It was also noted that a pure asset demand for money was difficult
to reconcile with large holdings of non-interest bearing cash and demand deposits which were rate of return dominated by such essentially risk free assets as, for example, Savings and Loan Association shares.²

At the end of the 1960s it seemed that those hypotheses about the nature of the demand for money function which were to be had from explicit analysis of the motives which might affect money holding were either wrong in the case of the speculative motive, or essentially indistinguishable from Friedman’s general postulates in the case of transactions and precautionary motives; and Patinkin (1965, ch. V) had demonstrated that one could derive a money-in-the-utility-function model from a precautionary demand model. It was for such reasons that, in (1969, p.112), I argued that explicit analysis of the motives prompting the holding of money had

... not produced a model of the demand for money that has made any correct predictions which were not also made on the basis of a simpler approach treating the demand for money in the same way as the demand for any other durable good might be treated.

I am relieved to report that I explicitly labelled this conclusion ‘tentative’ (p.113), because it was, with benefit of hindsight, almost surely wrong.

The mid 1970s saw the collapse of most economists’ faith in the stability of the demand for money function. I have argued at length in the 1993 edition of Laidler (1969) (henceforth Laidler 1969/93, see pp. 172-175) that this collapse was overdone, just as the previous wave of enthusiasm for the relationship had also been overdone. Suffice it here to note that new empirical techniques
based on co-integration analysis, with a little help from taking explicit account of institutional change, have largely rehabilitated the long run demand for money function. The apparent demise of the relationship nevertheless helped to create an intellectual vacuum in monetary economics which was quickly filled by New-classical analysis, but that work on the demand for money which continued not only rehabilitated old empirical results, but refined them too. In the process, it also provided a good deal of evidence consistent with the view that money is held because it is a means of exchange.

The well known Baumol (1952), Tobin (1956) inventory-theoretic model of the transactions demand for money antedates and was widely discussed in the empirical literature on the demand for money to which I have referred above. There was, however, a good deal of scepticism about its empirical value, some of it well based, and some of it not. In particular, the economies of scale in money holding that it was widely believed to predict seemed to fly in the face of Friedman’s evidence that the permanent income elasticity of demand for real money balances was well above unity.

Here, as it turned out, we were on shaky ground, both empirically and theoretically. To begin with, Friedman’s initial estimate turned out to be biased upward. When a proper role was accorded to the interest rate in the relationship, the income elasticity fell to about unity. (Meltzer 1963, Laidler 1966). And when allowance was made for the spread of monetary exchange, particularly in the economies of the late 19th century, the elasticity estimate in question was pushed down yet again. (Bordo and Jonung, eg. 1987). Furthermore, as Brunner and Meltzer (1967) pointed out, the famous ‘square root rule’ for the demand for money in fact only arises as a limiting special case in the Baumol-Tobin model. Its general prediction for the income elasticity of demand for money is a range between 0.5 and 1.0. Thus, though it predicts economies of scale except at
the upper limit, these need not be particularly pronounced. And to this we may add an observation which originated with Joel Fried (1973). He pointed out: first that the ‘brokerage fee’ variable which appears in the Baumol-Tobin model is plausibly interpreted as a time and trouble variable for which the real wage, as a measure of the value of time, would be a natural proxy; and second that in time series data, real income (or permanent income, or wealth) is positively correlated with the real wage. Fried concluded that if the real wage were omitted from a time series regression, then the income-elasticity estimate yielded by that regression would be upward biased.

Now none of this would matter very much if the Baumol-Tobin model was the only one which focused on money’s means of exchange role, the only one which predicted that there should be economies of scale in money holding, and the only one which suggested that some real wage measure of the value of time might belong in the function, because there remains one compelling argument against this model. It has agents receiving income in money at the beginning of some period, and exhausting that income by the period’s end. Even if agents never found it worthwhile to convert cash into bonds and then back again at sub-intervals within the income period, the Baumol-Tobin analysis makes it hard to see why an agent receiving income monthly would ever hold more than two weeks income in the form of cash. And yet the United States economy currently holds close to two months income in M1 balances, and well over six months income in M2. It is hard to sustain claims, such as that recently made by Mankiw (1992), that this is still the best available model of the demand for money in the face of evidence like this.

But the model in question is in other respects representative of a far broader class whose members make the same predictions about economies of scale and the value of time without placing unrealistic upper limits on the size of money holdings. Thus, precautionary demand models, which hark back to Edgeworth (1887) and Wicksell (1898), can easily find room for brokerage fees and
also predict economies of scale; while more general 'value of time' models (McCallum and Goodfriend 1987, Dowd 1990) though they are not explicit about economies of scale, naturally lead to the inclusion of a wage rate variable in the function. They do, when all is said and done, start from the premise that to hold real balances enables agents to economise on time spent transacting and hence to devote more of it either to working and/or leisure.

By now the role of wage rates in the demand for money function has been investigated in a number of studies (see Laidler (1969/93) pp. 168 for references) and it is a fair generalisation to say that, where their influence has been tested for it has been found to be statistically significant. Unless this result is a product of publication biases - in the sense that negative evidence on this phenomenon has not been written up, or if written up has failed to find an outlet - it shows that explicit analysis of money's means of exchange role has enabled us to single out an extra variable for inclusion in the demand for money function, a variable which Friedman's approach led us to overlook. ³ And to this, we may add another consideration, so obvious once it is pointed out that its neglect must be a cause of some embarrassment. There are, as Faig (1989) and Sumner (1990) have recently noted, well established seasonal variations in the demand for money, associated with Christmas in particular. It is hard to see how this could be unrelated to the seasonal surge in retail transactions, and hence to money's means of exchange function. And the fact that there were well documented seasonal fluctuations in the demand for currency in the 19th century associated with the harvest (See Jevons 1866, and Sprague 1910) points in exactly the same direction.

In short, my 1969 conclusion about the empirical fruitfulness of analysing the motives for holding money was probably wrong. It does after all seem worthwhile to analyse the demand for money as a demand for a means of exchange. To do so enables us to make predictions which we
would not otherwise have made, and a number of these do have empirical content. The relevance of this conclusion in the context of theorising about the demand for money is obvious enough, but as I shall now go on to argue, it is potentially relevant in a broader context too. It suggests that those who attach high value to micro-foundations have been looking for them in the wrong place when they have turned to Walrasian general equilibrium analysis.

MONEY AND MICRO-FOUNDATIONS

It is instructive to consider the properties of the two most widely used methods of introducing money into the economic models which currently dominate the ‘micro-foundations’ tradition, namely to resort to an overlapping generations framework, or to impose a ‘cash in advance’ constraint on an otherwise Walrasian economy.

The former approach involves modelling the demand for money as the demand for a pure store of value, and it should not therefore be surprising that it yields some predictions that look remarkably like those of Keynes’s model of the speculative demand for money. If money is a store of value pure and simple, then it is almost by definition a close, and in some cases perfect, substitute for other assets that perform the same function. Hence, if it is rate of return dominated it will not be held. But empirical evidence, even of the most casual kind, makes it hard to take this prediction seriously. Defenders of this approach are aware of the problem, and therefore resort to stories about money’s ‘backing’ - ie. its prospect of being redeemed in the form of utility yielding goods or direct claims thereon at sometime in the future - or about ‘legal restrictions’ that force agents to hold rate of return dominated cash.

The key prediction of the ‘backing’ hypothesis, as developed say by Bruce Smith (1985), is
that the demand for money will be highly elastic with respect to the expected opportunity cost of holding it, so that changes in the nominal quantity of well-backed money will be absorbed in cash balances rather than affect the price level. This prediction is hard to reconcile with an enormous body of evidence about the rather low interest elasticity of demand for money found in modern economies, not to mention the absence of a liquidity trap. More to the point, the evidence which Smith produces to suggest that the hypothesis had content in 18th century North America has been severely, and in my view convincingly, criticised by Michener (1987) and McCallum (1992) for neglecting the presence of components other than colonial government paper in the money supplies of the economies studied.

As to ‘legal restrictions’, these are and have been often in place, and undoubtedly affect the demand for money. It is uncontroversial that many of the institutional changes which have underlain recent disturbances to empirical demand for money functions have been direct and obvious consequences of changes in the regulatory environment. But this observation stops far short of supporting the claim that legal restrictions per se explain the very existence of a demand for money. As Carl Menger (1892) noted long ago, legal tender laws and the like have usually been introduced in order to codify already existing customs in the monetary sector, rather than to establish new and previously non-existent norms of behaviour. Neil Wallace (1988) was surely right, therefore, when he characterised the legal restrictions idea as a means of ‘oversimplifying the theory of money’.

What then, of the cash in advance constraint? Though much used by exponents of ‘sound micro-foundations’, they do not defend it as having any. Rather they treat it as a convenient but admittedly ad hoc device which enables them to get on with their monetary economics. They are right to do so, and as should already be apparent, I do not think that this alone should be grounds for condemnation. The trouble is empirical: the constraint yields what amounts to the degenerate
special case of the Baumol-Tobin model discussed above, a case in which agents receive income in cash and then spend it, without resort to trips to the bank in the interim; and actual money holdings are far too large to be explained by it. If one is going to abandon explicit micro-foundations in order to get money into one's model, why stop with the cash in advance constraint when the literature is full of empirically more satisfactory models which are, at least, no less ad hoc?

It must certainly be admitted that models of the demand for money, based upon explicit consideration of its means of exchange role, cannot be reconciled with a strict application of the tenets of methodological individualism as they are conventionally applied in economics. These tenets would have us start with the economy's primitive characteristics - the tastes of individuals, their endowments, and available technology - and proceed from there, by way of the maximising choices of the individuals in question to predictions about market outcomes when those individuals interact in accordance with well defined rules of market behaviour. If these injunctions are taken seriously, it simply will not do to put money in the utility function, either directly, or indirectly as a creator of time. This can be seen most clearly by considering that special case economy inhabited by Robinson Crusoe.

One might put all manner of arguments into Crusoe's utility function, but a means of exchange would not be one of them. Nor would the appearance of Man Friday change matters. As we all know, it takes at least three agents and three goods before it becomes physically possible to move away from barter. The phenomenon of monetary exchange, from which money holding's capacity to generate utility derives in value of time models, is an inherently social phenomenon. If we model the demand for money in this way, we must presume that a market experiment is already in progress before we analyse the individual experiment. That individual experiment cannot be treated
as having logical priority.

Something must give way: either that body of theorising about the demand for money which has kept reasonably close contact with empirical evidence and leads us to conclude that the demand for money stems from its means of exchange role, or some rather widely held notions of what constitute sound theoretical procedure. Obviously, I would opt for giving up the latter. To argue that some social phenomena cannot be reduced to the behaviour of individuals, but also seem to derive from relationships among individuals, might disturb those who believe that scientific knowledge must be derived from a set of true and strictly individualistic first principles, but it is an innocuous position to take for someone who is content to believe that our most primitive premises can never be more than a set of tentative working hypotheses that are always likely to be superseded. In short, if methodological individualism doesn’t permit us to cope with monetary phenomena, then so much the worse for methodological individualism.

From this point of view, much of the fuss that is made about the impropriety of putting money in the utility function seems to arise from looking for the micro-foundations of the demand for money in the wrong place. If we are permitted to treat the ‘rules of the market game’ as a starting point for economic theorising, and monetary exchange, like property rights, as a component of those ‘rules’, then it makes perfectly good sense to think of agents receiving a flow of services from the asset which permits them to participate in an already ongoing market game.

It is worth noting that economic theorists have not entirely neglected questions about the origins of monetary exchange. Menger (1892), Brunner and Meltzer (1971), Jones (1976) and Kiyotaki and Wright (1993), among others, have all provided analyses, characterised by varying degrees of mathematical rigour, of why monetary exchange exists, and how it might evolve. The fact that the end point of such analysis - typically the emergence of some pre-existing commodity
as the economy's means of exchange - stops short of enabling us to move on directly to deal with the more tradition questions addressed by monetary theory - the determination of the price level, the influence of monetary policy on output and employment, etc. - tells us not that it is unhelpful, or that the models which do enable us to deal with these issues are inherently and fatally flawed, but only that there currently exists a gap in the that body of knowledge which we call monetary theory.

MONEY AS A BUFFER STOCK

In the last few pages, I have argued that the theoretical foundations of models of the demand for money which emphasise its means of exchange function, and sometimes take the short-cut of placing real balances in the utility function, are not quite as devoid of theoretical foundations as is sometimes suggested. If we don't insist on deriving everything from the analysis of the maximising individual, if we are willing instead to treat the facts of monetary exchange as standing in the same logical relationship to individual behaviour as those characterising the structure of property rights, that is as describing the pre-existing rules of the market game which we are analysing, then the 'value of time' approach does provide an intellectually respectable basis for theorising about the demand for money, and, as I have already argued, an empirically fruitful basis too.

Indeed, I would go further than this. The approach in question, and particularly that variation of it that stresses precautionary behaviour, also yields new insights about monetary analysis which have both theoretical and empirical implications. I have written about these matters elsewhere (eg. Laidler 1990, chs 1 and 3), under the label 'Buffer Stock Approach'.² It will suffice therefore to summarise these insights here, while highlighting their subversive implications for much recent
macroeconomic analysis.

To begin with, if it really is the case that the demand for real money balances which we observe in any actual economy has a strong precautionary element to it, then this has immediate implications for the way in which we treat both expectations formation and price flexibility. For the last two decades, the advocates of sound microeconomic foundations for macroeconomics have usually insisted upon modelling agents’ behaviour on the assumption that they make ‘full’ use of ‘all available’ information in formulating their market strategies. They have also - albeit with less insistence lately - preferred to model the macroeconomy ‘as if’ markets are continuously cleared by flexible nominal prices. In each case, the relevant postulate has been defended as the only one consistent with basic maximising assumptions. A moment’s consideration, however, of a typical precautionary demand for money model leads one to suspect that this need not be the case, that both nominal price stickiness and expectations that, while ‘sensible’ fall short of being ‘rational’, can be compatible with maximising behaviour.

To generate a precautionary demand for money on the part of the individual agent, one must begin by postulating some random element in the pattern of payments and receipts. Money is then held as a buffer against such fluctuations, in order to reduce the costs that they impose. The point is, though, that the degree of randomness in this pattern is usually under the agent’s control to some extent. Resources can be devoted to gathering and processing information - to forming expectations - and in the case of a price setting agent, to responding to that information by altering prices in order to influence flows of revenue and expenditure. But if data processing is subject to rising marginal cost, if it is costly to vary prices, and, crucially, if holding precautionary balances reduces the costs of encountering imbalances in payments and receipts, then a maximising agent will balance these costs on the margin. It is thus possible that, in an economy where monetary exchange
is one of the ‘rules of the market game’, this very fact might give rise not just to precautionary inventories of cash, but also to nominal price stickiness and less-than-rational expectations.

This possibility is surely intriguing. It is now ten years since Boschen and Grossman (1982) showed that contemporaneously observable changes in the money supply seemed to have systematic consequences for real variables. It is hard to reconcile this observation with the idea that agents make full use of all available information, and operate in markets where prices are free to vary in response to new data as and when they appear. But if maximising agents do not use all available information, and do not continuously adjust nominal prices, because it is uneconomic to do so, there is nothing here to puzzle us, and certainly nothing that should make us despair of the predictive power of the basic postulate of maximising behaviour.  

This particular insight, namely that rigidities of prices and expectations are not necessarily signs of irrationality on the part of agents, is also valuable in another area of applied macroeconomic analysis, namely that which goes under the label ‘the short-run demand for money function.’ It is beyond question that theorising of the type described in the second section of this paper, though it has a great deal of empirical content, still leaves much to be explained. It yields a ‘long-run’ relationship which, when confronted with time-series data, generates residuals whose complex and varying from sample to sample pattern of serial correlation require the skilful deployment of distributed lags of one sort or another to cope with them.

Laidler (1982, ch.2) and Lane (1990) have shown, however, that the most usually advanced economic rationalisation of such distributed lags, namely the existence of private portfolio adjustment costs that create a distinct and essentially Marshallian ‘short-run’ demand function, is not satisfactory in the presence of price flexibility. Such flexibility enables the market to adjust real balances to a new equilibrium value without individual agents ever encountering adjustment costs,
and its existence would ensure that the economy is always on its long run demand for money function. In that case, unless we have the wrong long run model of the demand for money, we will not encounter serially correlated residuals.

On the other hand, price stickiness and expectations which, though 'sensible', nevertheless are sometimes systematically in error, can easily be deployed to generate a pattern of money holding that deviates systematically from that predicted by a correctly specified long-run demand for money function. However, if we interpret matters this way, we must be careful to note that the parameters which describe short run fluctuations in money holding do not characterise structural characteristics of any demand for money function. Rather they appear to be unidentified quasi-reduced forms of the structure which links variations in the money supply to the arguments of the long run demand for money function, of what is generally described as the transmission mechanism of monetary policy.

Now a claim that the above interpretation of the evidence is well established cannot be defended. One of its earliest formulations, that of Carr and Darby (1981), suggested that the so-called 'Goldfeld equation' - see Goldfeld (1973) - which transformed the long-run demand for money function into a short run one by adding a lagged dependent variable, was simply a price level adjustment equation in disguise. As MacKinnon and Milbourne (1988) showed, this interpretation does not withstand careful scrutiny. But other, more sophisticated formulations of the same 'buffer stock' approach to the short-run dynamics of money holding behaviour - for example, those of Cuthbertson and Taylor (See 1990 for an example which contains extensive references to related work) or Davidson (1987) - seem to be more robust. And this approach also helps to explain the apparent instability of the demand for money function whose appearance in the 1970s did so much to render unfashionable the work I have been dealing with in this paper. Ought we not to
expect a relationship which describes the long and variable lag linking monetary changes to real income and prices to display instability?

CONCLUSIONS

The title of this paper consists of two questions, and we are now in a position to answer both of them. Why do agents hold money? - probably because money is a means of exchange, and because holding a precautionary stock of money enables them to economise on the time and trouble it takes to transact in markets. Why does it matter? - because, if the foregoing answer turns out to be valid, first, it forces us to confront the fact of monetary exchange as an alternative to the Walrasian market as a means of co-ordinating economic activity, second, it seems to undermine the methodological individualism upon which the currently fashionable quest for ‘sound’ microeconomic foundations for monetary economics is based, and third, it seems to point us towards some interesting and potentially fruitful lines of enquiry, both theoretical and empirical.
Footnotes

1. See Mayer (1993) for an eloquent and even tempered defence of essentially the same position as I take here.

2. And these results have, in my view, held up. See Laidler (1969/93) pp. 150-152 for a discussion.

3. The fact that demand functions in the tradition of Friedman's and Meltzer's work continue to hold up in tests such as those of Lucas (1988), even though they omit a real wage variable, surely stems from the close correlation between this variable and permanent-income and wealth variables in long runs of time series data.

4. Note that in Laidler (1990) pp. 13, 21, 47-48, I have argued that a precautionary approach to modelling the demand for money predicts that there are externalities in money holding: - if agent A holds more cash, he is more likely to be able to pay a debt when called upon to do so; therefore his creditors, B, C, etc. need to hold less money to achieve a given degree of security. This argument implies that the cross section permanent-income elasticity of demand for money should exceed the time-series elasticity. This prediction remains to be tested, though the results of Mulligan and Sala-i-Martin (1992) showing an unusually high income elasticity of demand for money arising from cross section estimates on data drawn from U. S. states are intriguing.

6. I do not claim to have originated the 'buffer-stock' terminology. The earliest usage of which I am aware is that of Friedman and Schwartz (1963).

7. In a review of Laidler (1990), Peter Hartley (1992) has suggested that we need not take Boschen and Grossman's results seriously, because the data upon which they are based may not represent the true variables to which new-classical predictions apply. Testing data by comparing their characteristics with the predictions of theory seems to me to put things exactly the wrong way round!

8. Once again, the reader is referred to Laidler (1969/93) for further discussion. See pp. 175-178, 186-188.
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