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Commentary

WE ARE INDEBTED TO K. Alec Chrystal and Ronald MacDonald (1994) for assembling a valuable body of evidence on the relative explanatory power of simple-sum and Divisia versions of the money supply aggregates across a range of countries. This contribution comes at a time when the usefulness of money supply measures is called into question by economists across the policy spectrum. I am dismayed at the wide agreement among macroeconomists ranging from Alan Blinder to Robert Rasche that the money to income relationship is broken and that our conventional understanding of money demand is at a loss to explain the decline in velocity that has occurred during the past decade. Has the quickening pace of financial innovation rendered old relationships obsolete, as many are suggesting? Somehow this all sounds too familiar. Those of you who were around in the 1970s may recall "The Case of the Missing Money." Then it was a puzzling rise in velocity, and one explanation put forward was the quickening pace of financial innovation (see: Enzler, and others, 1976; Goldfeld, 1976; and Hamburger, 1977). Economists, nevertheless, continued to think that monetary aggregates were important, enough so that they were disappointed again a decade later when their models seemed to go off track.

Even the "monetarists" are in disarray among themselves on the issue of which aggregate to watch. At a Federal Reserve Bank of San Francisco conference last spring, the Bank's president, Robert Parry, quipped that Milton Friedman had told him that M2 was growing much too slowly while Allan Meltzer had told him that M1 was growing much too fast, so he figured that monetary policy must be just about right. In the face of that kind of disagreement, it becomes difficult at best to explain to skeptical colleagues or the public why they should take any monetary aggregate seriously as an indicator of monetary policy. But as one of the few teachers of introductory macroeconomics (perhaps the only one) who bases their course on the Quantity Theory of Money rather than the Keynesian Expenditure Model, I can't afford to take such a pessimistic view.

The first and perhaps primary set of results presented in this paper uses regressions of the growth rate of nominal income on the growth rate of a money aggregate, the growth rate of federal spending on goods and services, and, in the case of the United States, the change in the yield on Treasury bills. Lags of zero to four quarters are included. A second set of results adds four lags of the dependent variable to the regression. Test statistics compare each simple-sum (*SS*) aggregate with its Divisia (*D*) counterpart. The Akaike Information Criterion (AIC) statistic compares the likelihoods of the two regressions, and two other tests each produce a pair of *t*-statistics, one that can reject *SS* in favor of *D* and another that can reject *D* in favor of *SS*. Finally, *F*-statistics for the exclusion of all the money terms in each regression address the question, "Does money matter at all?"

In thinking about these regressions, I found it useful to keep in mind the distinction between a money demand equation and a reduced form equation by Leonall C. Andersen and Jerry L. Jordan (1968) and Leonall C. Andersen and Keith M. Carlson (1970) in their landmark papers. A simple money demand equation might be of the form:

$$(1)\ M = K(i)\ Y\ e^{\varepsilon},$$

where Y is nominal income and $K(i)$ a function of the nominal interest rate i. Taking logs, denoted by lower case letters, and rearranging we have:

$$(2)\ y = m - k(i) - \varepsilon.$$

This is a structural equation and to get to the reduced form we need a model for the interest rate, something like:

$$(3)\ i = p^e + r(gov,\ real\ shocks),$$

where p^e is expected inflation and $r(gov, real\ shocks)$ is the real rate as a function of government fiscal variables denoted gov, say the deficit as a fraction of GDP, and real shocks which may not be directly observed. In forming p^e, economic agents will presumably use information in the past history of m and *gov*. Substituting for p^e and then i, we have the reduced form equation:

$$(4)\ y = y(m\ with\ lags,\ gov\ with\ lags) + E(\varepsilon,\ real\ shocks),$$

which is akin to the equation estimated by Andersen and Carlson. The U.S. regressions run by Chrystal and MacDonald, which include an interest rate, are structural and therefore have no obvious role for government spending, while regressions without the interest rate for other countries are in reduced form. In the latter case, I would expect the deficit rather than spending on goods and services to be the more appropriate government fiscal variable.

I want to mention in passing that there is nothing here that says that velocity must be constant, or deterministically trended, or stationary, or even cointegrated with the interest rate for there to be a useful and predictable relationship between money and income. The error processes ε and E may be integrated processes like random walks and indeed coefficients may also be stochastic processes without destroying our ability to estimate models and make predictions, although the kinds of processes involved will affect the accuracy of predictions and the deterioration of accuracy with forecast horizon. Certainly the fact that velocity did not continue to move along its upward trend of the 1970s does not in itself imply that money-income models are invalid, as some people seem to be saying. Twenty years ago, John P. Gould and I (1974) noted that velocity has experienced trend reversals in the past, behaving much like a random walk. Neither the characterization of velocity as a random walk, nor its link to nominal interest rates should have lead us to expect the velocity trend of the 1970s to continue indefinitely.

Turning to the results of the U.S. regressions, I am struck by how weak the evidence is for using Divisia in comparisons with the simple-sum aggregates. I expected that the superiority of the D versions would increase with aggregation since the idea is to extract the transactions part of the aggregate. Indeed, according to the Akaike Information Criterion which looks at the difference in log likelihoods, SSM1 is favored over DM1; DM2 has a slight edge over SSM2; and DM3 is strongly favored over SSM3, although the progression fails with L. One reason that I am surprised how small the AIC statistics are for D aggregates is that in a sense they are already fitted to the data. It would be interesting to see a comparison between SSM1 and DM2 to see whether most of the benefits of purging SSM2 are captured by SSM1, which is more readily available to most of us in real time. Similarly, it would be interesting to see direct comparisons between the relatively simple Currency Equivalent (CE) aggregate proposed at this conference last year by Rotemberg (1993) and the D aggregates. It would be helpful to the reader to have goodness-of-fit measures and log likelihoods reported for all the regressions so that other comparisons could be made easily. The *t*-tests give very puzzling results, frequently giving inconclusive results in which each version is rejected, in turn, in favor of the other. The character of the results does not change when lags of the dependent variable are included.

Does money matter? Does it matter if money matters? Perhaps less than I might have thought in the context of structural regressions which include an interest rate. Certainly, variation in velocity, proxied by the interest rate, may account for a considerable variation in nominal income, so money is not the only monetary variable in the model. Indeed, it is not surprising then that in the results for the U.S. reported in Table 1 SSM2 matters, in the sense of the F-test for inclusion of that variable, much more than SSM1, since the velocity of SSM1 varies much more than the velocity of SSM2. What puzzles me is that DM1 matters so much less than SSM1, in fact not at all, and results are even worse for DM1A.

Has the money-to-income relationship broken down since the early 1980s? It might be interesting to see if the regressions using the D aggregates are more stable than those for SS aggregates.

For the remaining countries, the regression does not include the interest rate, so for the non-U.S. countries, we are looking at a reduced form. As explained above, however, I might have expected the fiscal variable to be the budget deficit rather than spending on goods and services. The message I get from these countries overall is that DM2 works better than SSM2, but it is not important to use the D version of M1. Japan, of course, is a special case. I say "of course" because Japan seems to be different in many economic studies, a fact often pointed out with pride in my experience by Japanese economists. In the case of money aggregates, not only does Divisia not matter, but nothing about the aggregates matters. It would be interesting to see how the time series for Japan differs from the other countries to see what accounts for this result. I suspect it reflects lack of variation rather than lack of a relationship.

The unit root tests are of particular interest to me because we have a chance here to compare across countries. It warmed my heart to see only one variable that is apparently stationary in levels less than expected by chance out of the 54 variables if these series were all unrelated. And that one variable is a T-bill rate (for Australia), which is already first differenced because it is a growth rate. What is perhaps more surprising is how few other variables are stationary in growth rates. For Australia, stationary inflation and growth rates for GDP and SSM3 go along with stationarity in the level of the T-bill rate. But only 13 of the 54 series are stationary in first differences at the .05 level. Indeed, countries as seemingly regular as Switzerland have non-stationary growth rates, and for Japan it is only the T-bill rate that is stationary in first differences. Evidently, we live in an I(2) world.

Chrystal and MacDonald draw on the technology of cointegration to try to detect long-run relationships among the variables. Since the variables are generally I(2) while the VAR model used for detecting cointegrating vectors is to be estimated in first differences of I(1) variables, it is growth rates which become the relevant "levels" for this analysis. The authors report finding one or two cointegrating vectors for all the countries, implying that there is a long-run relationship among growth rates of the variables. The M1 aggregates for the United States are an important exception. In general, though, we would be missing some long-run information if we looked only at relationships among the stationary second differences of these variables. It would be interesting to see what those cointegrating relationships look like, whether they resemble a money-demand function or are something quite unexpected.

The VAR is then combined with the error correction term implied by cointegration (where applicable) so that each variable (in turn, the change in the growth rates of money, real GDP, the deflator and the T-bill rate) is predicted by four lagged values of itself and each of the other variables, as well as by the error correction mechanism (ECM). As in so many VAR studies, it turns out that the strongest predictor is simply the lagged value of the variable being predicted. For the United States, lags of other variables are generally not useful in predicting GDP or the inflation rate, except that the T-bill rate helps to predict—and, in turn, is predicted by—GDP inflation and M. The ECM also helps to predict inflation in the case of the broader aggregates. As we look across countries, the most striking regularity is the power of awkward, unclear lags in predicting each variable. Otherwise there is little regularity in the pattern of results which range from Switzerland, where almost every variable helps to predict every other variable, to the United Kingdom, where only the ECM seems to matter for GNP. Why the great differences?

If there is one variable that money should be able to predict, it is inflation. If the Divisia aggregates are superior measures of money, then one might expect them to be superior predictors of inflation. There is, however, very little difference in the significance of lags of Divisia aggregates verses simple sum, and no clear margin in favor of the former. However, the ECM also presumably includes the money aggregate, so differences in the contribution of the ECM must be attributed to the distinction between the aggregates. In the case of Australia, for example, lags of SSM2 are more significant in explaining inflation than are lags of DM2, but the error correction term that appears in the DM2 equation is more significant. Since the two equations differ only in the choice of the money aggregate, one must credit DM2 with the greater predictive power of the ECM in that equation.

In fact, lags of the aggregate may not matter at all given the ECM, and yet the aggregate may be playing an essential role in the ECM. There are many examples in the tables where the money aggregate itself is not significant but the ECM is. We cannot conclude in these cases that money does not matter, and for that reason I would not call these causality tests.

Another reason to be cautious in concluding that money does not matter if the lags of it are not significant is that the VAR is a restrictive framework in which to detect dynamic relationships. A few lags of a noisy variable will contain little information if the variable operates with a long lag. The interest rate is a powerful leading indicator probably because it smoothes much of the information contained in the very noisy money-growth series. I think that this limitation of VARs is one of the main reasons why we have learned so little from the large volume of work based on them. Perhaps it is time to take seriously again distributed-lag modelling, which allows for differing lag structures on different variables.

I would like to conclude with a plea for visual presentation of data. Economists are traditionally afraid to look at their data—it is considered cheating. I find, on the contrary, that plotting the data is an invaluable tool for understanding models, why they work or do not work, and how specification might be improved. I am uncomfortable with a statistical result that I cannot see in the data. Often, plotting the data reveals why a relationship we expected to find does not show up in formal tests and where it has gone off track. In this spirit, I have prepared a few charts that may be very familiar to many, but which I found helpful in putting in perspective the notion of a long-run relationship between money and income.

In Figure 1, I have plotted the velocities of M1 and M2 along with the T-bill rate. I did not have ready access to the Divisia counterparts. It makes clear the huge difference between the stability of the M2 velocity and the great variation in M1 velocity. Clearly, in a model of the money-income relationship, it will be very important to be able to explain the latter but relatively unimportant to explain the former. It also makes clear the fact that M1 velocity reflects long-term variation in the short-term interest rate but not short-term variation, as Rotemberg (1993) and others have noted. It is by no means obvious to me that the decline in M1 velocity since the early 1980s is in any way inconsistent with the decline in interest rates. M1 velocity and the interest rate are plausibly cointegrated; that is, they appear to track over a long time period, although they move apart over shorter periods.

These dynamics are evident in Figure 2, which is a scatter plot with the log of the velocity of M1 on the vertical axis and the log of the T-bill rate on the horizontal. There is a clear difference between the small, short-run response of velocity to a change in the T-bill rate and the large, long-run response. The last several points represent the period since the recession when the sharp decline in short-term interest rates has been accompanied by only a modest decline in M1 velocity. But it is not clear that this sluggish short-term response is out of line with experience.

Since the velocity of M1 evidently responds to the T-bill rate with a lag, I have smoothed the T-bill rate by replacing it in Figure 3 with the T-bond yield. While the long-term bond market may not provide the optimal smoother for this purpose, it is free and was not contrived. Now the scatter follows a smooth curve and recent experience is indistinguishable from past experience, a fact noted by Poole (1988) and others. I fail to see why we should abandon the idea that there is a stable, long-run relationship in levels between money, interest rates and nominal income. I wonder whether the substantial changes in parameters associated in this paper with the 1979 change of monetary regime would hold if the bond rate replaced the bill rate.

I do want to call your attention to the scatter plot for M2 velocity and the T-bond yield in Figure 4, because this presents more of a puzzle in its recent behavior. Keep in mind that we are looking at relatively little variation in the velocity, but certainly the bond yield accounts for little of it. Indeed, the recent rise in the velocity of M2 runs counter to the decline in both short- and long-term interest rates. What gives? Perhaps it is the beginning of the end for M2 as Higgins

Figure 1

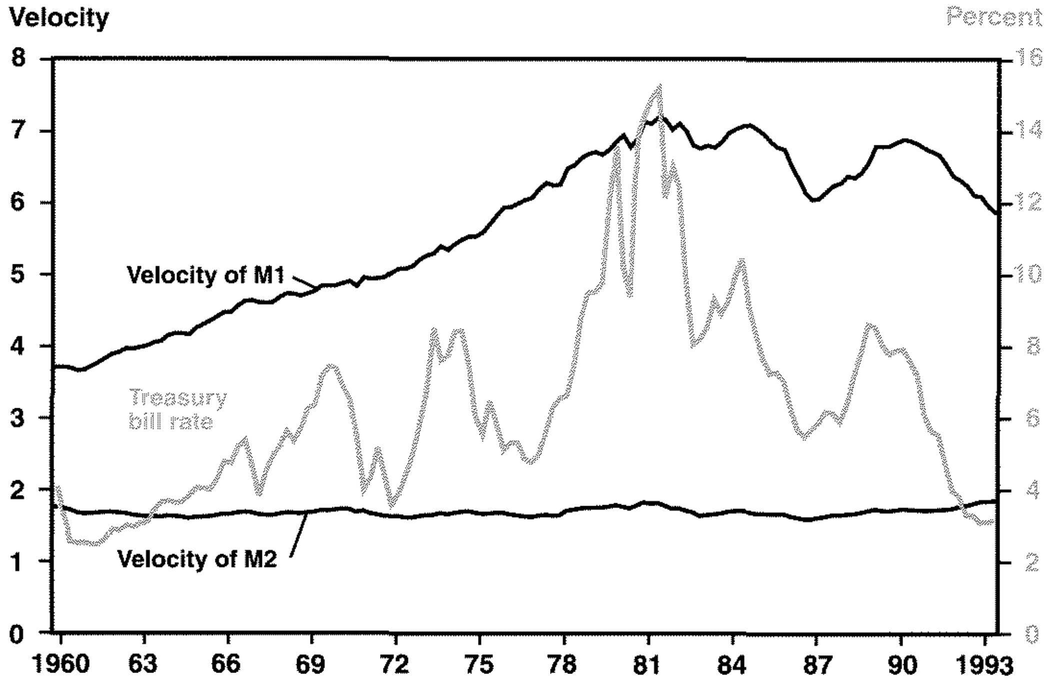


Figure 2

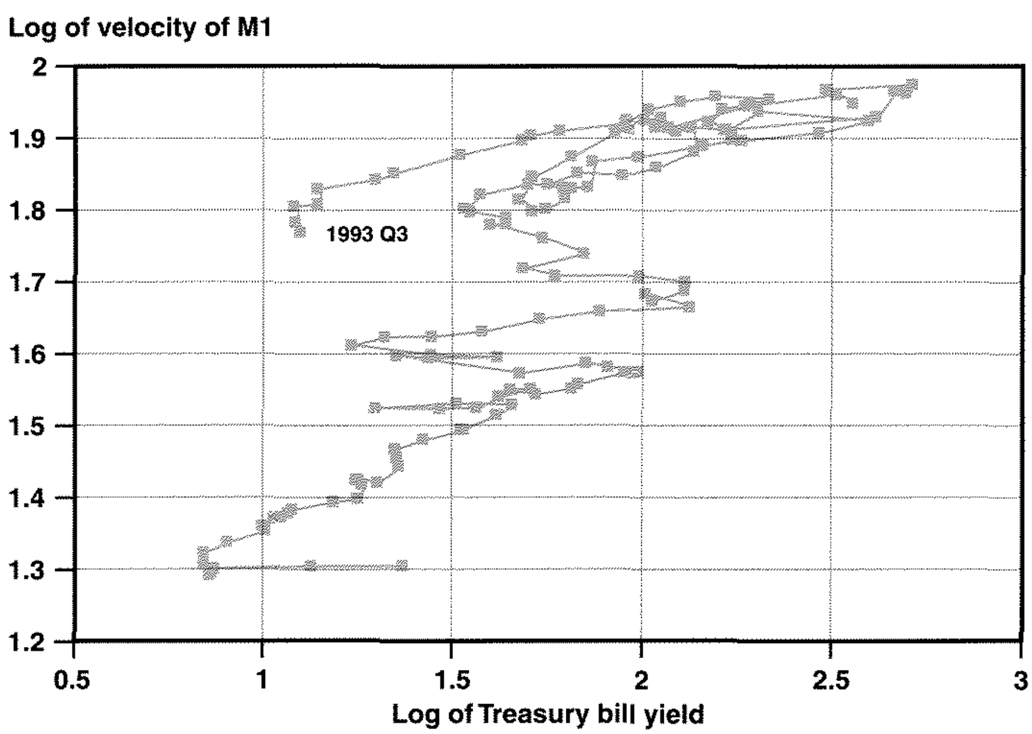


Figure 3

Log of velocity of M1

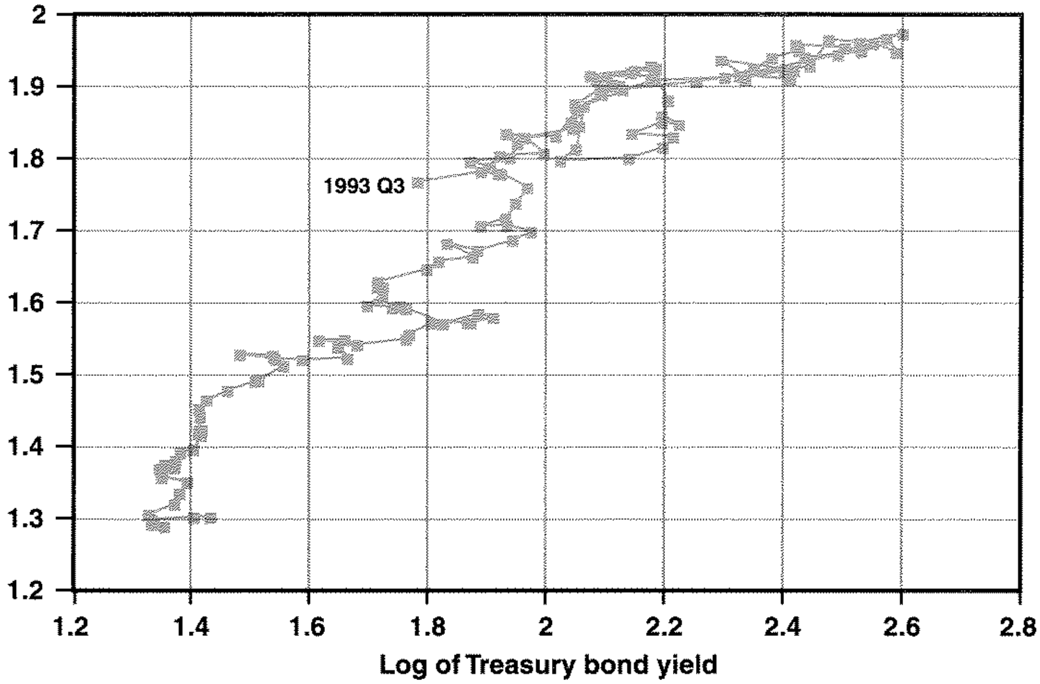
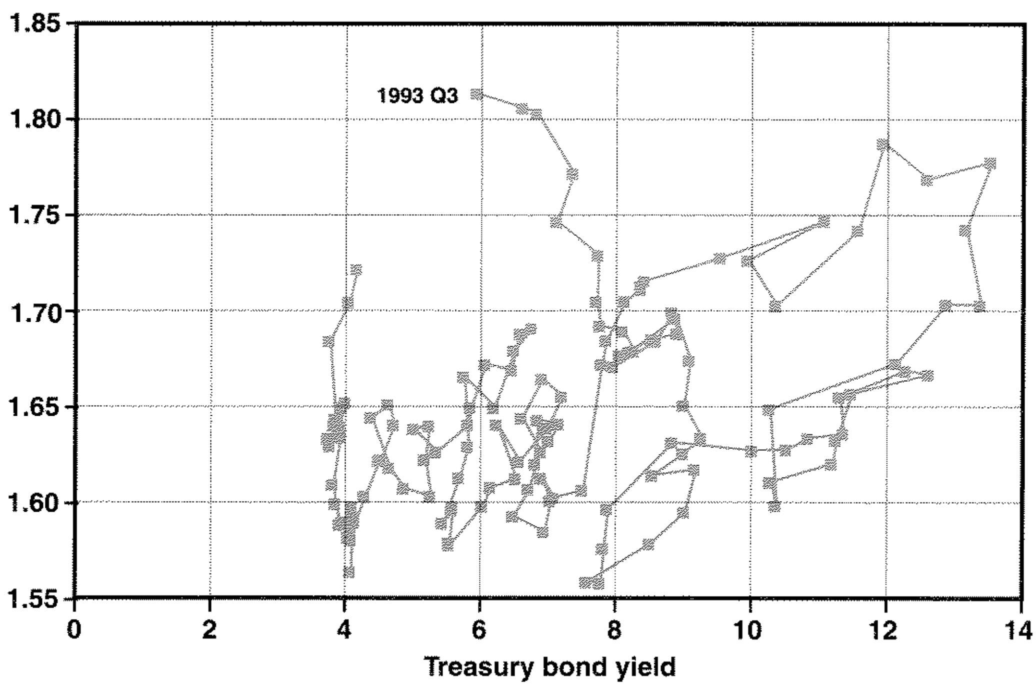


Figure 4

Velocity of M2



(1992) and others have suggested. My own view is that this is a temporary phenomenon related to the discovery of equity mutual funds by traditional holders of CDs. Even relatively sophisticated individuals have been explaining to me recently how mutual funds pay 15 percent compared to only 3 percent at the bank. There is an expected opportunity cost to holding M2 that we do not measure. My expectation is that M2 velocity will again fall into line after the public is awakened, perhaps rudely, to the fact that mutual fund shares are not CDs.

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