

David Laidler

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Commentary

CARL CHRIST'S PAPER is a worthy tribute to Ted Balbach. It is broad ranging, thoughtful and provocative; and it deals with serious issues too. Moreover, no small matter for this discussant, it is readily accessible to the stochastically challenged. The best compliment I can pay it is to add a few reflections of my own on the questions it raises.

It must now be at least 25 years since I first heard Carl Christ discuss the importance of testing models against data that had not been used to build them. Even then he distinguished between data generated before and after not just the model's estimation period, but also the actual time at which the model was constructed. This last distinction is not often made, but Carl convinced me that it is more important than we might think. I am glad he still stresses it. The simple fact is that what we know about economic history influences how we build our models in ways that we barely recognize. Suppose we decided today to build a model of the U.S. business cycle, to estimate it for the period 1948–70, and then to test it further against data for 1971–92. When we constructed our model, would we be able to ignore the two oil price shocks during the 1970s, and would we even be right to ignore them if we could? But if we did remember the activities of the Organization of Petroleum Exporting Countries, would it really be the case that the structure fitted to the data for 1948–70 would yield parameter estimates unaffected by any influence from data generated after 1970?

It is at least safer, and more convincing too, if we test our models against really new data—data

of which we were unaware at the time those models were constructed. I must confess, though, that the first time I heard Carl Christ make this point, I was discomfited by his argument. In the 1960s I was estimating demand-for-money functions, and I did not much like the idea of waiting another decade or so before submitting my results to a journal. The right scientific approach was all well and good in its place it seemed to me, but there were more mundane matters to consider—promotion and tenure, for example. But here we are 25 years later, and the back issues of economics journals are full of empirical studies, which were influential in their time but are now half forgotten, whose results could be subjected to real tests. How would the Jorgenson investment equation or the Andersen-Jordan equation stand up?¹ There is a market niche here waiting to be filled by applied econometricians, not least those currently worrying about the above-mentioned publishing criteria for promotion and tenure.

In his paper, Christ has shown us how to do such work with his investigations of what he calls the plain-vanilla velocity equation, first proposed by Henry Latané in 1954. This rather odd equation has held up surprisingly well. The use of the inverse of the rate of interest as an argument surely (as Robert Rasche has suggested to me) reflects Latané's reluctance to use logarithms to deal with a nonlinear relationship in an age when such a transformation of data had to be carried out using tables and much tedious interpolation therefrom. In the light of Carl's results I am relieved to be able to report that, even before reading his paper, I had decid-

¹See Jorgenson (1963) and Andersen and Jordan (1968).

ed to retain the paragraph dealing with Latané's study in the new edition of *Demand for Money*.²

From a certain viewpoint, the survival of the Latané equation for a full three and a half decades is remarkable. It is, after all, best interpreted as a rearrangement of a supply-and-demand-for-money system, and as Carl also tells us, the last two decades have not been kind to empirical demand-for-money functions. But at least one precedent in the literature occurred, namely Robert E. Lucas Jr.'s demonstration that Allan Meltzer's long-run demand-for-money function also seems alive and well when viewed in light of more recent data.³

Now we must not claim too much here, and Carl does not. The Latané equation displays many faults calculated to shock the econometric purist—for example, auto-correlated residuals. When these are attended to within sample, the out-of-sample performance of the more sophisticated formulation seems to deteriorate. Similarly, Lucas showed that though subsequent data still seemed to move around Meltzer's relationship, they did so with a great deal of complex serial correlation. But still, I think there is a lesson to be learned here, one which I began to develop in the second (1977) edition of *Demand for Money* and which work using co-integration techniques is now tending to support. The lesson is this: what we call the long-run demand-for-money function is indeed a stable structural relationship, give or take ongoing institutional change, which we often deal with by adapting our way of measuring money. What we call the short-run function, however, is not structural at all. It is rather an ill-understood, quasi-reduced form characterizing the mutual dynamic interaction of the money supply and the variables on which the demand for money depends in the long run.

This way of looking at things helps explain why co-integration studies produce evidence consistent with the existence of a stable long-run demand-for-money function and why simple regressions of the type estimated by Latané and Meltzer hold up rather well. As David Dickey has told us here, simple regression is one way of looking for co-integration. It also helps explain why the error correction mechanisms associated with co-integration relationships are complicated and unstable, why the dynamics of

so-called short-run demand-for-money functions have tended to break down as sample periods are extended, and why more sophisticated estimation techniques, designed to cope with auto-correlated residuals, applied to relationships like the Latané equation produce results that are less robust over time than the plain-vanilla version. Have we not, after all, known all along that changes in the money supply affect the economy with long and variable time lags, which, among other things, involve feedbacks to the money supply itself? And if we have known that all along, should we be surprised if we get nowhere with studies of monetary dynamics that do not begin by specifying a model of the aforementioned interaction that will permit us to identify the structural parameters of the system we are investigating?

It is all much easier said than done, of course, but it will not be done if no one tries, and I hope therefore that Carl Christ's striking results for Latané's equation will prompt someone to carry his investigation further.

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²See Laidler (1993).

³See Lucas (1988) and Meltzer (1963).