What Will Take the Con Out of Econometrics? A Reply to McAleer, Pagan, and Volker

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Our 1981 paper, criticized by Michael McAleer, Adrian Pagan, and Paul Volker (1985), made two points. First, we argued that specification uncertainty renders suspect practically any but the weakest inference about the interest elasticity of money demand. Second, we contended that there is no credible reason to imagine that simultaneity problems are adequately dealt with in existing studies of money demand. Now, if McAleer et al. had wanted to make a truly effective criticism of our paper, they might have pointed out that if the second point is granted, the first does not follow. The Leamer-Leonard method for ascertaining fragility is based on the maintained assumption that the error is orthogonal to all the candidate regressors—precisely the assumption that we criticized in the second half of our paper. Had McAleer et al. argued along these lines, we would have been hard put to come up with a convincing reply. It is true that we suggested (p. 827) that extreme fragility is evidence of serious simultaneity problems. We suspect, however, that this argument would not bear close examination, except perhaps in special cases or as a loose statement of why we were motivated to think about simultaneity problems. Our argument reflected the rhetorical exigencies of a difficult transition, rather than any line of reasoning we could readily make precise. We are surprised that no one has called us on this point.

McAleer et al., of course, could not pursue these lines without invalidating their own purported contribution, which consists of attempting to rehabilitate one-equation-at-a-time estimation despite the fact that the equations being estimated are presumably embedded in simultaneous equation systems. It is therefore no surprise that McAleer et al. ignored our invitation to engage in a serious discussion of macroeconometric practice, given that their ox would be gored more than ours.

McAleer et al. either have an understanding of the nature of simultaneous equations estimation very different from ours, or they completely misunderstood our argument. For example, consider their Section IV, Part A, where they found it strange that we deleted the lagged value of the money stock as an explanatory variable for the current money stock despite our expressed opinion that "such lagged endogenous variables as the lagged money stock...cannot plausibly be excluded from the demand side either explicitly as observable explanatory variables for the demand for money or implicitly from the time dependence of the error" (p. 840). Our intention in the passage just cited, contrary to McAleer et al.'s interpretation, was not to criticize estimates of money demand (such as our own) that exclude the lagged dependent variable. Rather, it was to cast doubt on the presumption that lagged money could plausibly serve as an instrument for the interest elasticity of money demand, as has widely been recommended. Despite the simplicity of this argument, McAleer et al. completely misread it, finding only that it is "strange" that even though we conceded that m_{-1} should in principle enter the money demand equation, we nonetheless suppressed it from an ordinary least squares equation.

I

Let us suppose, contrary to what we argued in our 1981 paper, that simultaneity problems can magically be assumed away. Thus assume that even though we do not know

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what the correct explanatory variables are, we are nonetheless sure that the unobserved determinant of money demand is statistically independent of these variables. Thus there is no problem with ordinary least squares. These were the conditions assumed in the first half of our paper, and throughout by McAleer et al. Our suggestion was simply that specification uncertainty be explicitly acknowledged, and that the sensitivity of the estimated interest elasticity to respecifications be assessed using the methods developed by Leamer and others. Our idea was to encourage econometricians to report priors explicitly so that readers can compare their own priors to those of the econometrician and evaluate the results accordingly.

This suggestion appeared uncontroversial to us, but apparently not to McAleer et al. They are troubled by the fact that different extreme bounds can result from different classifications of variables as doubtful or free. They conclude that since assessments of fragility depend on a "whimsical" choice of priors, such assessments are altogether unreliable. We are mystified by this criticism. It is indeed true that different priors lead to different posteriors—what would be the point of Bayesian econometrics if it were otherwise? Prior beliefs are by definition treated as given; what is gained by calling them whimsical? McAleer et al. appear to suppose that there is some way to dispense with prior beliefs in doing statistical inference, so that a data set can be made to reveal a single correct inference with the appropriate application of statistical technique. On the contrary, both Bayesian and classical statistics of the Cowles variety depend essentially on prior information. The conclusion that an inference is fragile is not some kind of descriptive fact, as McAleer et al. presume, the validity of which can be impugned if it is shown to depend on a whimsical choice of priors. Rather, the fragility of an inference is conditional on the choice of prior. It is precisely the virtue of Leamer's method, not its fault, that it relates fragility to prior beliefs.

McAleer et al. found fault with extreme bounds analysis because no account is taken of prior beliefs about the signs of coefficients; instead variables are classified only as doubtful or free. Here again we are unpersuaded. One of the most important tasks of empirical econometrics is the verification (or falsification) of sign priors. Now, if the reader knows that in the process of arriving at a preferred specification the econometrician has incorporated his (or her) prior beliefs about the signs of coefficients, as by deleting variables which have "wrong" signs, will he (or she) be persuaded by a report of an estimated equation in which all coefficients have the "right" sign? We doubt it. It is exactly because it is (in many contexts) so easy to find in the parameter space a regression that reproduces prior beliefs that readers are routinely unimpressed by reports of "success" in estimation.

Having demonstrated, at least to our satisfaction, that McAleer et al.'s reservations about extreme bounds analysis are without substance, we must acknowledge that one of their criticisms of our application of extreme bounds analysis is correct, and it is not minor. Just as they suggested, we treated the constant term as doubtful. This was inadvertent; since we are not aware of any theory that could justify suppression of the constant, it should be treated as free. McAleer et al. repeated our calculations with this correction and found that the extreme bounds are near zero. The interpretation is that our data and priors justify a confident conclusion that the interest elasticity of money demand is approximately zero, not that any inference about this parameter is fragile. Since McAleer et al. successfully duplicated our results, we have no doubt that their calculation is correct. But the conclusion that the interest elasticity of money demand is zero reflects the exclusion of lagged terms from our regressions. We suppressed dynamics because their inclusion would only widen extreme bounds which, we (incorrectly) believed, were already very wide. Since we have not recalculated the extreme bounds under a less stringent restriction on priors, we must concede that we have not demonstrated the correctness of our contention that, on reasonable priors, any inference about the interest elasticity of money demand is extremely fragile.

II

The question arises whether McAleer et al.'s rejection of our strictures on conventional estimation practices means that they accept these practices. Although there is no necessary reason for one to imply the other, examination of McAleer et al.'s proposed procedure for arriving at a "tentatively adequate" money demand equation reveals that they do in fact accept received practice, subject to some new bells and whistles which are discussed below. They ignore simultaneity problems, and see nothing undesirable in the informal and unsystematic infusion of prior information during the estimation process. Indeed, they recommend it. Our 1981 paper, of course, criticized received practice on exactly these two points. As an example of McAleer et al.'s casual incorporation of prior information, we need do no more than consider their choice of sample period. Their data set ended twelve years ago. Why not use more recent data? Because "a large number of studies have experienced difficulty in estimating conventional money demand functions for the post-1973 period" (p. 302). This explanation, accompanied by a nod in the direction of financial innovation (the alleged cause of the unruly money demand equations estimated from more recent data), appears to McAleer et al. sufficient to justify truncating the data set to the convenient 1952–73 period for which "satisfactory" estimated money demand equations are a dime a dozen. *Plus ça change, plus c'est la même chose.*

Beginning with this informal choice of an adequate data set, McAleer et al. approach the problem of modeling money demand as a four-step process: 1) begin with an overparameterized model; 2) attempt to restrict the number of parameters in that model using tests for common dynamic structure in the variables; 3) set to zero all coefficients in the restricted model that are insignificant; 4) perform a battery of diagnostic tests to see if the model passes quality control. The first three steps in this process reflect McAleer et al.'s version of David Hendry's (1980, 1983) philosophy of modeling "from the general to the specific." The line of reasoning that underlies this approach is that sequences of properly nested tests can be treated as independent. Thus, by nesting tests in this way, the overall significance level of the tests can be controlled. This feature, however, is not going to be very important when the tests at each stage have low power against reasonable alternatives, as do those used by McAleer et al. Moreover, properly nesting tests requires setting in advance fixed criteria for accepting restrictions, not looking at a collection of t-statistics and making arbitrary decisions about which variables to delete. The end result of the McAleer et al. procedure is a simplified model that represents just one of many possible paths through the thicket of restrictions.

It should also be clear that the general-to-the-specific approach, while it may have some benefits when properly executed, is philosophically at odds with structural econometric estimation. The latter requires one to begin with a model that is subject to identifying and possibly overidentifying restrictions. In the former approach one begins from an overparameterized model and identification and exogeneity restrictions play no particular role. Viewed as a nonstructural model, however, the initial single equation considered by McAleer et al. is too narrowly conceived. Considering that the variables being modeled consist of money, *GNP*, interest rates, inflation rates and wealth, all likely to be jointly endogenous, a vector autoregression would be a more appropriate overparameterized nonstructural model.

The final step of the modeling procedure advocated by McAleer et al. is designed to confront the model with a variety of problems and performance criteria to see how well it holds up. Here the authors make a complete about-face on modeling philosophy. They switch to a specific-to-the-general modeling approach by testing whether the model should be generalized to include a group of variables not considered previously; seasonal variables, additional lags, trends and so on. It is not clear (because it is never discussed) why these variables were not included in the initial overparameterized model. At this stage, McAleer et al. also act as though they have been dealing with a structural model by reporting the results of a test for simultaneous equation bias even though the test requires candidate instrumental variables, and is consequently applicable only in the presence of identifying restrictions.

Finally, McAleer et al. report the fact that although their model "gave satisfactory performance up to 1973, just like automobiles, age finally caught up with it, and after that date its predictive performance declined dramatically" (p. 305, fn. 11). This might lead the unwary reader to conclude that their equation fails one of the diagnostic tests the importance of which they stress: out-of-sample prediction. On the contrary; McAleer et al. report no problems in this respect. To arrive at such a startling conclusion, they truncated the data set at 1970, reestimated their model, and then compared out-of-sample forecasts with the data through the end of 1973. The fact that the prediction errors had variance comparable to that of the sample errors then led them to report success in the out-of-sample prediction! The breakdown of the model after 1973 apparently has no bearing here. It appears as if McAleer et al. see no need to hold themselves to the demanding standards of model adequacy that they recommend to us.

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