Comment on “Chaotic Monetary Dynamics with Confidence”

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The paper in this volume by Serletis and Shintani, “Chaotic Monetary Dynamics with Confidence,” is important, since it resolves some of the problems associated with a long standing controversy. In fact the paper is close to being the “last word” on the subject. As they state, Barnett and Chen (1988) motivated many papers on the question of whether or not there is chaos in economic data. Using the algorithms and tests available from physicists at the time, Barnett and Chen found that certain Divisia monetary aggregate data passes the tests for chaos. Ping Chen is a physicist, who was associated with two of the most active centers for research on chaos at the University of Texas at Austin at the time, and was particularly well situated to know how to design and run those tests.

Barnett and Chen’s paper motivated many subsequent papers by researchers seeking to replicate the result and explore its robustness to testing method, seasonal adjustment, sample size, and data source. Surprisingly many conflicting results appeared, with little consensus produced. This problem motivated the design and execution of a controlled experiment, with results published by Barnett, Gallant, Hinich, Jungeilges, Kaplan, and Jensen (1997). The papers that led up to that experiment are gathered together into Part 4 of Barnett and Binner (2004). While that experiment revealed the source of many of the robustness problems, it did not resolve the problem of determining the statistical significance of tests of chaos using the highly regarded NEGM test of Nychka, Ellner, Gallant, and McCaffrey (1992) for positivity of the dominant Liapunov exponent. That positive sign is a necessary and sufficient condition for chaos, by one of its most common definitions. While the NEGM approach produces a point estimate of the dominant Liapunov exponent, the test’s authors, who are very sophisticated statisticians, were not able to produce the asymptotic distribution of their test statistic.

In a very important paper, Shintani and Linton (2004) solved this difficult theoretical problem in work related to Shintani’s brilliant dissertation at Yale. Using the Shintani and Linton (2004) result, Serletis and Shintani have now produced standard errors, as well as point estimates, of the dominant Liapunov exponent estimated with monetary data. They reject chaos. But since their point estimates are negative, the standard errors are far less important than they would have been, if their point estimates had been positive. With negative point estimates, the null would have been rejected regardless of the standard errors of the test statistics. In future research, Serletis and Shintani might wish to use their approach to replicate previous results reporting positive Liapunov exponent estimates, to see if those inferences were statistically significant. To do so, they would need to use precisely the same data and sample period as used in the prior studies producing positive point estimates.

I agree with Serletis and Shintani that their results cast substantial further doubt on whether it is useful to view economic data as chaotic. But in the context in which they are working and in which Barnett and Chen (1988) worked, I have never believed there was much reason to view an inference of chaos to be particularly useful. The problem is that time series tests, of the sort involved in these controversies, have no ability to determine whether or not chaos found in economic data has, as its source, the nonlinear dynamics of the economy, as opposed to chaotic shocks from outside the economy, such as from the weather. For this reason, He and Barnett in this volume and in Barnett and He (2002) argue for conditioning upon an economic model. It then becomes possible to test hypotheses about bifurcation subsets of the model's parameter space and thereby to permit inference about the nature of the dynamics produced by the economy itself.

In fact, with univariate time series data, I find it difficult to believe that there is no low dimensional chaos in the signal below the noise. Physical scientists have published highly convincing evidence of chaos in nature, including the weather; and no economy is separated from nature. Serletis and Shintani argue that high dimensional chaos is not useful. While that is true, I'd add that low dimensional chaos that comes from external shocks to the economy also is not useful. The search for chaos is motivated by results from mathematicians about information contained in the attractor sets of chaotic solution paths. That information regards the structure of the system that produced the chaos. If the source of the chaos in economic data is chaotic shocks from the weather or from other sources external to the economy, then the information in the resulting fractal attractor set is about the structure of a dynamical system that is external to the structure of the economy.

To put the Serletis and Shintani results into context, consider distribution effects, aggregation problems, and non-chaotic nonlinearity. All exist, but often are ignored in economic research. The evidence in Serletis and Shintani should suggest to us that findings of chaos from time series data should concern us less than many far more conspicuous phenomena that routinely are overlooked in economic research. For that same reason, I was surprised by the controversy that was produced by the Barnett and Chen (1988) paper, which I had never viewed as a finding of chaos imputable to the structure of the economy. We did not condition upon an economic model, and hence we had no way to distinguish between chaos produced from within the economy, as opposed to chaos produced from the nature of shocks to the economy.

In summary, I think it would be useful for Serletis and Shintani to use their technology to test for the statistical significance of former findings with precisely the same data used in former published results. But even without those results, I do agree with them that there is little that is likely to be gained from findings of chaotic structure in atheoretical time series, regardless of the amount of noise in the data. On the other hand, I do not believe that we know whether or not the economy itself produces chaos, since I am aware of no convincing published tests of chaos conditionally upon an economic model. Such a test would require the ability to locate the subset of the model's parameter space that can support chaos, and to design a test of the null of chaos, when the likelihood function has singularities over the null subset. In my work with He in this volume and elsewhere, we produce and apply a method for searching for the bifurcation boundaries of a model's parameter space subsets, but in our applications we linearize in a manner than excludes the possibility of chaos. Numerical methods for locating bifurcation boundaries without linearizing are very difficult to implement, and to my knowledge have never been used with an economic model to locate and test the null of chaos.

It is my belief that the economics profession, to date, has provided no dependable empirical evidence of whether or not the economy itself produces chaos, and I do not expect to see any such results in the near future. The methodological obstacles in mathematics, numerical analysis, and statistics are formidable.

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