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Interactive comment on "First and second derivative atmospheric CO_2, global surface temperature and ENSO" *by* L. M. W. Leggett and D. A. Ball

Anonymous Referee #1

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This paper conducts time series analyses of the relationships between monthly CO2 and temperature data, with an emphasis on testing for Granger causality, in other words the phenomenon that one variable improves the forecasts of future values of another. Because the authors find that the CO2 series appears nonstationary, they work with the first differences of the series, what they call "first derivatives" (and also second differences). They pay special attention to the use of econometric time series techniques in their analysis.

My comments on the paper are as follows.

1. The issue that the series for temperature and CO2 since 1850 exhibit different degrees of integration, and hence cannot modelled conventionally, was the subject of an important paper by Beenstock et al. (Earth System Dynamics 3 (2012), pp 173-188). These authors studied annual data, and concluded that the series over the 1850-2007 period were best described as integrated of order 1 (I(1)) in the case of temperature and I(2) in the case of CO2. They therefore conducted a cointegration analysis between temperature and ïĄĎCO2 (ïĄĎ denoting first-differencing), rather than a correlation analysis, as appears here. Both studies therefore focus on dealing with the fact that a statistical model linking the levels of CO2 and temperature cannot be constructed. However, differences of timespan, and data frequency, lead them to different interpretations of this fact, which is an issue that deserves careful consideration, in itself. It is clear, in any case, that the present authors must reference the Beenstock et al. study, and reconcile their findings with the previous reported ones.

2. In fact, there is a considerable degree of controversy (see for example the comments on the Beenstock paper in ESD) about the order of integration of these series, and as to whether they are stochastic trend processes (I(1) or I(2)) or "trend stationary" over sub-periods, with periodic breaks in trend The essential problem here, I think, is that the time series models invoked in the literature on nonstationarity are rather simple, and cannot play the role of what econometricians call the "data generation process". At best, they are simplified descriptions that apply only over limited spans of time. This fact throws conventional inference procedures (which have a large-sample justification) into some doubt.

3. The present authors report ADF tests which reject unit roots (e.g. Table 3) yet it is clear from Figure 3 that the series exhibit an upward drift – clearly not stationary, although possibly "trend stationary". This would need to be allowed for by including a trend term in the statistic and using the appropriate Dickey-Fuller table. Otherwise, these ADF results are not valid. This issue of the treatment of drift has not been discussed anywhere that I can see, but it definitely needs to be.

4. In page 29109 line 11 the authors say “temperature is not stationary of itself but must be made stationary by differencing. . .” (my emphasis). It is important to make clear, something on which the authors are at best equivocal , that a time series cannot be made stationary. It either is stationary, or it isn’t. The differences of a series are a different series! It is not difficult to construct examples where the sign of the relationship between two series is reversed in their differences, or where two series are correlated in differences by exhibit independent stochastic trends. Since the AGW hypothesis is that more CO2 in the atmosphere translates into higher surface temperatures (not that temperatures respond to changes, but not to levels), this fact is crucial in understanding the results of this study. They really don’t receive sufficient discussion here. Are these results viewed as supportive of the AGW hypothesis, or not? We would appear to need continuously accelerating growth in CO2 to produce warming on an alarming scale. Is this hypothesis proposed, and what mechanism is envisaged? These questions badly need answering, or at least posing, if the reported results are to be understood.

5. In their analysis of the monthly data, the authors explain how they have smoothed the CO2 series by a moving average (Page 29113, line 10). This is evident in any case, because the raw CO2 series is highly seasonal, and no seasonality is apparent here. The problem is that smoothing and seasonal adjustment filters are notorious for changing the dynamics of relationships. I do not see how the lag-correlograms of Figures 4 and 5 are to be interpreted if they are computed for smoothed and deseasonalized data. They really prove nothing – and the same criticism has to be made of the various Granger causality tests reported, if these are conducted on smoothed data. The only legitimate way to conduct these kind of tests, where timing shifts of one or two months is critical, is on the raw observations, where extraneous data features such as seasonality have been accounted for by effective modelling. This may be tricky, but in the case of a seasonal pattern it might, for example, be effective to employ polynomial dummy variables to explain seasonal changes,.

Additional comments.

6. (Page 29019, line 20) The authors are right to avoid autocorrelation corrections in regression. In econometric practice such corrections, sometimes called “Cochrane-Orcutt” methods, are nowadays discredited since they have the potential to distort the relationships of interest. The authors are correct that dynamic modelling is the right technique. They are also correct (but could emphasize this more explicitly) that regression analysis (which I assume is taken to include contemporaneous drivers) cannot test causality, but can at best calibrate an (untestable) assumption of causality. The Granger-style test is the only legitimate means to explore causality in time series. I think the authors appreciate this fact, but their defence of their approach could be more clearly articulated.

7. (Page 29110, line 2) How can an “anthropogenic warming trend” be an explanatory variable or influencing factor? This seems to seriously beg the question. There are anthropogenic trends (e.g. level of industrial output) and warming trends (rising temperature?) but if we already know that these are one and the same, we need not bother with studies such as this one! I know the authors are commenting on previous studies here, but elucidation would nonetheless be most desirable.

8. (Page 29114, line 5) A Dickey-Fuller test is not a test of stationarity. It is a test of a unit root, and there are nonstationary cases of the alternative hypothesis. A test of stationarity (as the null hypothesis) might be the KPSS test (Kwiatkowski et al. (1992), Journal of Econometrics 54, 159-178). However, the KPSS test is not strictly a test of stationarity either. It is a test of weak dependence (i.e., summability of the autocovariance sequence) which is not a necessary condition for stationarity, as such, although it is a condition for conventional inference based on correlations to be valid in large samples, via the central limit theorem. Care needs to be taken to distinguish these different time series properties, and the statistical techniques appropriate to them.

9. (Page 29114, line 21) Pankraz (1991). Reference missing.

10. (Page 29118, line 6) Where is Supplementary Table S1? I don’t think that re-

sults should be discussed unless they are included in the paper being submitted for publication.

11. (Page 29126, line 24) “data not amenable to time series analysis. . .”? This is an odd statement that needs explaining. How correlations can be “visually observed”, if they cannot be tested conventionally, is even odder. I suggest this paragraph needs rethinking, and I will also mention that Figure 9 is puzzling, especially the green plot described as “first derivatives”. What are the vertical scales here? Have the curves been shifted and units of measurement changed so as to superimpose them. What’s the implication of this? (The same query may be asked about other graphs too).

12. Final comment. Many readers will have the paper as a monochrome print-out, and for such readers the colour-coded graphs cannot be deciphered. BW versions, with patterns instead of colours to distinguish the curves, are a must!

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29101, 2014.