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Interactive comment on "Comment on "Polynomial cointegration tests of anthropogenic impact on global warming" by Beenstock et al. (2012) – Some fallacies in econometric modelling of climate change" *by* D. F. Hendry and F. Pretis

Anonymous Referee #2

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Hendry and Pretis (HP13) are, in my opinion, correct to point out that some of the issues that can arise in time series analysis may afflict Beenstock et al.'s (2012) paper (B12). This part of their comment up to the end of section 3.2 is reasonable though not very focused on the main issues. The analysis in sections 3.3 and 4, however, commits some of the same potential errors as B12. Specifically, they over-rely on a particular test of time series properties and fail to specify a plausible physical model for the way they model the data. As the issues may be complicated for those unfamil-

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iar with econometric time series analysis, I first review the main points of B12 before addressing HP13. I think that B12 raise some interesting points that deserve further research but they come to overly hasty conclusions on the implications for climate change.

As explained by B12, the characterizations of time series that are most used in econometrics are that series are: stationary, trend stationary, difference stationary (designated I(1)), and second difference stationary (I(2)). A stationary variable has a constant mean and variance. A trend stationary variable is stationary after removing a linear trend, a difference stationary variable is stationary after differencing - converting the series to the period to period changes. For a second difference stationary series, only the changes in the changes are stationary. The simplest difference stationary time series process is a random walk. The simplest second difference stationary series is the partial sum of a random walk - an integrated random walk. In a simple bivariate climate change model, the properties of the forcing variable should be transmitted to temperature. For example, if radiative forcing is an I(2) variable and causes temperature to increase and there is a long-run relationship where greater forcing is associated with higher temperatures then temperature too should be I(2).

B12 investigate annual data for the 1880-2007 period for the radiative forcing of major greenhouse gases (CO_2, CH_4, N_2O), aerosols, solar irradiance, stratospheric water vapor, and global mean temperature. When we look at these time series as in B12's Figure 1, we immediately see that temperature looks very different to greenhouse gas forcing. The GHGs increase fairly smoothly over time and the rate of increase of CO_2 and NO_2 appears to be accelerating. B12's statistical tests show these variables to be I(2). The temperature series, by contrast, fluctuates a lot from year to year, and definitely does not increase smoothly. Statistical tests show that it seems to be I(1).* Because of this difference in properties, B12 argue that there cannot be a simple long-run relationship between the level of greenhouse gas forcing and global temperature. They proceed to estimate a model where forcing by greenhouse gases only has temporary effects on temperature. There is a long-run relationship between changes in forcing and the level of temperature. They do not explain the physical rationale for such a model.

This discrepancy between the properties of the temperature and forcing series was already noticed by Stern and Kaufmann (2000). However, they took a very different approach. Instead of a simple autoregressive model, they proposed that temperature could be an I(2) variable plus serially correlated noise presumably related to the internal variability of the climate system. They present reasonable evidence that this a possibility and also that the extracted I(2) trend could be related to the radiative forcing variables. An I(2) variable plus serially correlated noise with the signal to noise ratio shown by Stern and Kaufmann (2000) will appear to be I(1) using conventional tests on 100-150 observations. This is the problem. For the length of time series we have it is hard to come to very strong conclusions about the nature of the series using simple tests and, therefore, the possible relations between them.

B12's Figure 2 does raise questions though. Clearly there was a sharp increase in the rate of growth of the CO2 series after 1950. But there doesn't seem to be as an abrupt change in the temperature series. The discrepancy can't in fact be explained by the other forcings either. A possible explanation is an increase in uptake of heat by the ocean. Unfortunately, there are only ocean heat content data available from 1955 on, so that this hypothesis can't really be simply tested. B12 test for cointegration between temperature and ocean heat content. They find ambiguous results. Stern (2006) claims there is cointegration. But B12 argue that, as both these variables are I(1) according to their tests, this is irrelevant as to whether greenhouse gases causes climate change.

HP13 first argue (p220) that it is known that greenhouse gases warm the atmosphere and so there must be errors in B12 if they find that there isn't a long-run relationship between the levels of GHGs and temperature. This isn't necessarily the case. There are various reasons why there might not actually be warming or warming might be

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difficult to observe. For example, negative feedbacks such as increased cloud cover could cool the planet. Transfer of heat to the ocean might reduce the warming in the atmosphere, or aerosols emitted alongside greenhouse gases might obscure their effect. In reality it's likely that feedbacks are quite positive, but the latter two effects do make anthropogenic warming harder to observe. Still, I don't think that B12's basic question is misplaced.

Section 2 of HP13 then shows a time series example of the relation between kilometers driven and road fatalities in the UK. The two have an inverse relationship. This does not, however, mean that more driving causes fewer deaths. On the other hand, examination of Figures 1c and 1d in HP13 suggests that the changes in the two series are positively correlated. More driving does cause more deaths in the short-run, ceteris paribus.** A regression of the level of deaths on the amount of driving will give spurious results because of omitted variables. This is dealt with in section 2.1 of HP13.

Other issues raised by HP13 in section 2 are aggregation bias, unmodelled shifts, incorrect functional forms, data measurement errors, and that failing to reject the null hypothesis doesn't necessarily mean that the null is exactly correct. While some of these issues seem relevant to B12 others are less so and in the end HP13's critique seems to centre mainly on measurement errors and regime shifts. It would be good if there critique was more focused.

The measurement issue is that, prior to 1958,*** carbon dioxide data come from ice core measurements, which are far less accurate and far less frequent than atmospheric sampling. Reconstructed annual series are overly smooth compared to atmospheric measurements. This could result in a finding that the data are I(2) when in fact the true series is not. When HP13 break the CO2 series into these two regimes they find that the changes in CO2 series before 1958 are stationary around a constant but stationary around a trend after that date. The unstated inference from this is that the levels of the CO2 series are difference stationary. In section 4 they state: " a simple bivariate plot of temperature and log(CO2ML) over the second period, matched by means and ranges, suggests the obvious: they are closely related". Figure 4, which shows this plot is not very convincing. First, it uses monthly data for CO2 and annual data for temperature. This should be fixed. Second, both series are trending, though one is very smooth (if we used annual data) and the other very noisy, but the relationship could be entirely spurious. And in fact there are many more variables apart from carbon dioxide involved, so it's definitely spurious as it stands. What would also be interesting is testing the first differences of temperature over these two sub-periods. Are they stationary pre-1958 and trend stationary post-1958?

But, despite the measurement problems prior to atmospheric observations, it doesn't make physical sense to split the series in two and assume that different models of the climate apply to the two periods. Nothing in the actual system changed. The full series of changes in the CO2 series are neither stationary around a constant nor a trend. This means they must be more integrated than a simple random walk, which is what B12 found.

So, in conclusion, I find merit in the first part of both B12 and HP13. But both the original paper and comment go off the rails when trying to apply their basic findings to a model of the climate. HP13 could also focus their critique on the actual important issues affecting B12's analysis.

I think HP13 would mostly be understandable to an inter-disciplinary audience who has read and understood B12. Tables 1 and 2 and the brief discussion of unit roots could be explained better.

Notes

* B12 also test whether there could be a regime change in the CO2 series but they fail to reject the null hypothesis that there is no structural break. However, the critical value of -4.27 stated in B12 is higher than any value in Table 1 in Perron (1990) so it is unclear exactly what test was carried out or whether the null could actually be rejected. ** Oddly, this is not pointed out by HP13. *** 1978 for N2O.

References

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