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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Systematic attribution of observed southern hemispheric circulation trends to external forcing and internal variability" *by* C. L. E. Franzke et al.

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Thank you very much for reviewing our manuscript and your constructive comments.

The reviewers comments are in italics.

General Remark: The authors investigate the origin of the long term secular trend present in the large scale atmospheric circulation regimes in the Southern Hemisphere (SH). They use a new method of detection and attribution based on the Granger causality principle. They found that one of the main driver mechanism of the secular trend is the CO2 , and they contrast their results with the previous attributions with the Ozone depletion. This is an interesting systematic exploration of the driver attribution that de-

serves publication but I have a major concern about the choice of drivers. The authors used CO2 , OMD (I suppose that it is the ozone mass deficit, but never mentioned in the text or captions), solar constant, stratospheric aerosol optical thickness and sulphate aerosols. Although this choice is probably a good sample of the different possible drivers, the authors should make the reader aware that it is a limited choice and other drivers could be responsible of this secular trend. To my opinion, if you take any series for which you have a similar secular trend then you will attribute the trend of the original time series to this specific predictor. I was therefore wondering why you did not try the time series of sea ice extension displaying a secular positive trend during the last 40-50 years. I suspect that it could provide as good result as the CO2 increase, and it is probably a more direct mechanism of circulation modifications than the CO2 , whatever the specific origin of the Antarctic sea ice extension increase is (Note that this increase is not well explained by current climate models even when CO2 increases, rather most of the models predict a decrease of the sea ice extension). I therefore think that caution should be taken in drawing definite conclusions by considering a limited number of drivers, and I would be very much interested to know what will be the impact of other drivers like the sea ice extent, obviously related to the thermodynamic properties of the underlying ocean (or other drivers directly related to the dynamics and thermodynamics of the ocean).

The power of our method is that is able to take account of missing covariates. For instance, if you would include something like sea ice extent into the set of covariates and would get a result that the sea ice extent is more statistically significant - then it would not contradict our study. Simply because the variable gamma (describing the switching process and taking into account all of the unresolved covariates in our study) would be a different one and not the same one that we have obtained in this study. From our study we can guarantee that in a given set of explicit covariates we found the one covariate that is most important (in Granger-causality sense) - and that this covariate is the CO2. All of the other eventually-important covariates are sitting in the regime-switching process that we have also identified but where not presenting in our

results.

We do not consider sea ice extent regime patterns. We do not consider Antarctic sea ice extent because of its marginal expansion and because this slight expansion in extent is largely wind-driven (Holland and Kwok , 2012) and likely a response to the changes in the large-scale circulation. Furthermore, the changes in sea ice extent and area have been spatially heterogeneous, with increases in some areas like in the Ross sea and decreases in other areas like in the Bellingshausen/Amundsen seas (Parkinson and Cavalieri, 2012). This is despite the trend towards the positive SAM; thus, it is unlikely that sea ice extent would have a significant impact on the secular circulation trends.

We discuss this point in sections 2 and 3.

Minor points: Line 13, page 676 : a "a" should be removed.

Done.

Figure 1. Please define the different curves in the caption.

Done.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 675, 2015.