

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

C. L. E. Franzke et al.

christian.franzke@uni-hamburg.de

Received and published: 16 August 2015

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

The reviewers comments are in italics.

1) First paragraph, page 691, lines 8-9: Do you think that the results are sensitive to the reanalysis used? Though this paper covers a more reliable period, some studies found that, even after 1979, large differences still exist between the reanalyses in the circulation, precipitation and SAM trends (e.g. Bromwich et al., 2007; Bromwich and Fogt, 2004).

C320

We have an other recent paper where we use the JRA-55 reanalysis for a regime study of the SH circulation and find similar results. Thus, we feel that our results are robust. We discuss this in the first paragraph of section 2.

2) There need to be more discussion of the Figure 3, especially in terms of time of residence in the summer season, which presents the most distinct pattern in relation to other seasons and annual: the regime transition have not occurred around 1980 and the frequency and persistence of the positive SAM and wave 3 are almost the same from 2000-2007.

We extended the discussion of Fig. 3. See first paragraph of section 4. Thank you very much for pointing this out.

3) Zonally asymmetric fields are used in the statistical analysis and not just mean zonal indices. This is important, as Ho et al. (2012) and others demonstrated that there is sensitivity associated with the index chosen to represent SAM and thus, it is necessary to consider the impact that the choice of SAM index has on the outcomes of any SAM attribution study. I suggest the inclusion of this reference in the Introduction section at the end of last but one paragraph.

We added a discussion of the Ho et al. paper. See first paragraph of section 1.

Technical corrections 1) Section 3.2, line 27 introduces the acronym OMD, which is not defined previously. Is it the acronym for ozone mass deficit? This needs to be specified.

Done

2) Missing units for geopotential height and surface air temperature in Figure 2.

Done

3) Missing the statistical significance in Figures 2 and 4. Because the statistical significance has already been discussed in many previous papers (e.g. Steig et al. 2009)

C321

we do not think that this will add anything substantial to this current paper.

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