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Climate of the Past Discussions

Interactive comment on "Climate variability of the mid- and high-latitudes of the Southern Hemisphere in ensemble simulations from 1500 to 2000 AD" *by* S. B. Wilmes et al.

S. B. Wilmes et al.

wilmes@climate.unibe.ch

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Review comments on "Climate variability of the mid- and high-latitudes of the Southern Hemisphere in ensemble simulations from 1500-2000 AD" by S.B. Wilmes et al. This paper investigates Southern Hemisphere variability in a multi-centennial context and studies the relation between variability modes among each other and climate variables. The topic is interesting, in particular because a detailed understanding of Southern Hemisphere dynamics and its connection to the mid and low latitudes is still missing. Therefore, the approach of ensemble simulation over a period where both natural and anthropogenic forcing, and internal variability play a role in shaping long-term climate

evolution and transition is very adequate.

1) However, I find that the paper is not well presented. The manuscript is often not very clear and could be more to the point. There is an imbalance of lacking important information and quite a bit of unnecessary or overly detailed repeating what is found in the literature. Also the number of figures could probably be reduced. I would suggest that the authors revise the manuscript focusing on the topics that are new and important. There are also (as pointed out below) some questions on the methods that should be clarified. After such (major) revisions the manuscript should be appropriate for publication.

We substantially shortened parts of the discussion, but added a discussion on the comparison of model results and reconstructions with respect to precipitation. Additionally we followed the suggestion to remove Fig 7.

2) Model and expt. Description, page 3095, line 9ff: I don't think that 50 years are enough for an adjustment from 1990s to 1500 conditions. There is a big change in CO2 forcing that will influence surface patterns but also ocean heat content etc. Given the length of the run, it would be more appropriate to disregard the first 150 or 170 years.

In principle the referee is correct with this statement. However we realized that the equilibrium state of the model for 1500 AD conditions is far too cold given the already rather strong cold bias of its 1990 AD control simulation of roughly 1C. Moreover, the model state was close to a threshold which strongly affects the ocean circulation (see Yoshimori et al., 2010). Thus we decided not to start the model from an equilibrium state but to try to account for the remaining drift by a correction which is described in detail in Hofer et al. (2011). The procedure is similar to the ones performed by other modeling groups, e.g., Ammann et al. (2007). To clarify this we extended the description of the correction procedure: "The transient simulations were detrended prior to analysis using the same procedure as for the control simulation in order to minimize the effect of the drift in the simulations which is mainly due to the long adjustment time of the ocean. This procedure similar to the one applied by, e.g., Ammann et al. (2007)"

3) SAM, page 3096, lines 23ff: does it make any sense to do spectra over multi-decadal times for the 40-year ERA data? I find very little information value in the spectrum figures other than that model and reanalysis data are quite different.

Probably, the formulation is misleading. The spectra of Fig. 3 show only the period range of 2 to 140 months which is possible to estimate which ERA40 data. We changed the MS to: "...show that on timescales between 1 and 10 years SAM represents a white-noise process."

4) Atmospheric modes, page 3098, lines 15ff: I don't think that fig. 4 can tell us anything useful about the 16th (!) century or so. We can just see by eye that SAM appears to be more susceptible to volcanic forcing and GHG increase (btw: what is the red line underlying the black?). It would be more appropriate to discuss if variations or trends seen in the time series under external forcing conditions are significantly different from the control simulation.

We corrected to 16th century. We changed the figure such that it shows the 12 month running means (in grey) and the 11*12 month running means (black) in order to highlight the interannual and interdecadal variability; and the red and blue areas over/under the black graph are dropped as little information is gained by them.

5) Regional changes, page 3099. It is an interesting finding that the different regions respond differently to the ext. forcing. However, it would be good to learn more why that happens. Unfortunately that point is not taken up again in the discussion of the regression analysis in 4.3. Figure 8 is just interpreted in terms of similarity to the modes, but there should be more information how the different regions are influenced.

As also suggested by referee#1, we extend the discussion on this point. See response to referee#1 point 4) for a discussion of this issue.

6) Page 3100, lines 20ff. Figures should be discusses in order of their appearance. Figure 7, since it just shows a number of straight lines, could probably be taken out. It would be enough to say in the text that there are no changes in the running correlations.

We decided to remove Figure 7.

7) Impact of external forcing: page 3101. This entire paragraph is very hard to understand; I had to read it three times to get it. Also fig 9 does not clearly say what the regression coefficient mean variable per standard deviation or per ext. forcing in Wm-2 or arbitrary aerosol units? Also line 26: should this be per 1 Wm-2?

We rewrote the paragraph and clarified Fig. 9 and its caption. The paragraph now reads "The external forcing appears to have a strong impact on regional climate in southern South America. Therefore, it shall be analyzed by separating the effects of the volcanic eruptions from the combined GHG and solar forcing. In a first step the impact of the volcanic eruption was determined, and in a second step it was removed from the time series of the respective variable.

First, the average anomalies for year 1, year 2 and year 3 after the volcanic event were determined against the 3 years mean prior to the eruption. This was done over the period from 1500 AD to 1620 AD as during these years the eruptions are temporally well separated and the changes in GHG and solar forcing are small. In a next step the regression coefficients between the anomalies of the respective variable in year 1 to 3 and the strength of the eruption were determined. Subsequently, the impact of the volcanic eruptions was removed by multiplying the regression coefficients for year 1 to 3 with the time series of the volcanic eruptions and subtracting it from the time series of the respective variable. The impact of the combined GHG and solar forcing was estimated by performing a regression between the summed solar and GHG forcing and the variable of interest (e.g. temperature and precipitation)."

We changed the caption of Fig. 9 to "Regression coefficients between the volcanic forcing and the changes in (a) surface temperature, (b) surface pressure, (c) precipitation, (d) sea surface temperature, (e) U-winds and (f) V-winds shown as responses to a change in aerosols by 1 optical depth unit for year 1 after the eruption." No, line 26 is supposed to read 1 degrees C.

8) Discussion: in general, the discussion of the findings from the literature could be more concise. In 5.2, a very detailed account of the reconstructions is given but the comparison with the models is relatively superficially done (and missing for precip).

We have substantially shortened the discussion and included a comparison of model results and reconstructions with respect to precipitation.

9) Page 3103, line 17: What is the "Fogt" reconstruction?

The Fogt reconstruction is the name given to one of the reconstructions used in Jones et al. (2009). "One set of reconstructions [hereafter the Jones and Widmann (JW) reconstructions] use the ïňĄrst PC of extratropical SLP as predictand, while the other (hereafter the Fogt reconstruction) uses the Marshall index." (Jones et al., 2009). We substantially shortened the paragraph in which SAM reconstructions are discussed to aid the understandability and to make the discussion more concise and now no reference to the Fogt reconstruction is made anymore.

10) Page 3111: do you mean CMIP5/AR5?

We did refer to the IPCC (2007), so to AR4 and the CMIP3 simulations, however utilizing the set of CMIP5 models used for AR5 would also be a possibility for obtaining a comparison between a range of models. An even better set of models would be given by the upcoming PMIP3 models which are tailored for paleoclimate simulations. Therefore, we changed P3111 L18 to "It would be interesting to now carry out the same analyses on a whole set of models (e.g., the upcoming PMIP3 simulations) to extract robust results and to assess uncertainties of the individual models."

11) Fig. 6: temperature <> precipitation.

Done as suggested.

Dear referee #2,

Thank you for your comprehensive comments and helpful suggestions substantially improving the revised version of our manuscript.

Yours sincerely, S. B. Wilmes and C. C. Raible and T. F. Stocker.

Interactive comment on Clim. Past Discuss., 7, 3091, 2011.