Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
https://doi.org/10.5194/cp-2021-156-RC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on cp-2021-156

Anonymous Referee #1

Referee comment on "Influence of long-term changes in solar irradiance forcing on the Southern Annular Mode" by Nicky M. Wright et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-156-RC1, 2022

This article addresses an important subject, the influence of solar variability on the Southern Annular Mode (SAM). The paper demonstrates that large changes in the solar radiative forcing can lead to significant changes in the SAM, and investigates the spatial impact of such changes. The authors also find an increased agreement with paleo-reconstructions of the SAM. In my opinion this paper provides a useful contribution to this important area of research and therefore deserves to be published. Before final acceptance, I have a few suggestions which I hope will be useful in improving the article. I think that the bulk of the paper looking at the effect in model simulations of changes in the SAM is well presented and clear, but I think that the interpretation of the comparison to the reconstruction of the SAM could be improved.

The introduction should contain a discussion about the uncertainty around the solar reconstructions and where it comes from, particularly in reference to the reconstruction from Shaprio et al used here. The study of Judge et al mentioned later should be discussed here as should the latest solar reconstructions made available for CMIP6 (as described in Jungclaus et al). In particular it is worth noting that the PMOD reconstruction which has still much larger amplitude than the Steinhilber et al reconstruction shown here, has less amplitude that the original Shapiro et al reconstruction and is described in Jungclaus et al as an “upper limit”. I do not think that this in anyway invalidates the results shown in this paper – but they should be framed so that it is clear that the solar forcing used is now considered to have too large an amplitude.

I also think care is needed about the interpretation of the comparison of the model results to SAM reconstructions. For the main results the correlation to one SAM reconstruction is significant but isn't to the second reconstruction. I am not sure how to interpret this – particularly given the small number of degrees of freedom due to the strong 70-year filter used. How strong a result is this and how much does the result hinge on the low period of SAM in both reconstructions and models after 1400? Could one interpretation of the results be that the reconstructions have a very low SAM amplitude starting in the 15th century which cannot be explained by the model simulations unless a strong solar amplitude forcing is used? If so this seems to me to be a nice, strong and clear conclusion. As well as the analyses shown here – one simple analysis which I would find very useful would be if you could show what the variability of the piControl simulations were (e.g. two standard deviations) – then you could quite easily make the point that the observed variability cannot be explained by internal variability (assuming that the model variability is correct).

In addition to the above I also have a few more minor comments which I hope will be helpful.

L34-35 The Neukom et al references are of course fine here, but I wonder if a reference to the PMIP3-PAGES2k paper doi.org/10.5194/cp-11-1673-2015 would also be useful.

L205. Although mentioned later – I think it would be useful to mention here that HadCM3 also does not have interactive ozone.

L206. Which reconstruction is this calculation of the SAM consistent with? The one using monthly means or annual means as a calibration?

L248 and fig 7 – is the radiative forcing just for solar, or does it include all forcings?

L353 – it would be worth checking the latest PMIP4 simulations runs to confirm if this is definitely still the case.

No mention is made in the results about the 11-year cycle despite the fact you mention that it could have an effect on the SAM in the introduction. I know that the title makes it clear that you are interested in "long-term changes" so the 11-year cycle may be outside the scope of this paper but I wonder whether this is something you have looked into? Is there any evidence of an effect in the transient simulations? Alternatively is there much of an effect in the first decade of the constant forced simulations, how long does it take for the climate to react to a change in forcing? I think this would be a useful addition to this study, but appreciate that there is already quite a lot of work in this paper already so may not be something the authors wish to pursue.