Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-585-AC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the variability of the annular modes: Reanalysis uncertainty vs. sampling uncertainty" *by* Edwin P. Gerber and Patrick Martineau

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Received and published: 2 October 2018

Thank you for these detailed comments our manuscript. We respond below to your comments of a scientific nature, and will correct all the grammatical and typographical mistakes in our revision.

In the context of troposphere-stratosphere coupling, it would have been useful to see more Southern hemisphere analysis, particularly associated with the final warming.

We appreciate the author's concern about neglecting austral hemisphere. We avoided the final warming in part because several authors have noted that the response to the

Stratospheric Final Warming (SFW), is structurally different from the annular modes (Black and McDaniel 2007a,b and Sheshadri et al. 2013). In addition, the SFW is being explored as part of SPARC Reanalysis Intercomparison Project, and will be reported in Chapter 6 of the report. We had intended to highlight how the annual cycle of annular mode variability in the austral hemisphere is quite different from the boreal hemisphere, and will make an effort to better capture this in the revised draft.

It seems curious that satellite observations are necessary for representing the SAM but not the NAM, one wonders why.

We speculate that this is due to the sparsity of conventional observations in the Southern Hemisphere, particularly in the higher latitudes.

P2 L20 The correlation between jet responses to global warming and annular mode persistence was not clear in the CMIP5 models (Simpson and Polvani 2016). In fact, since the annular modes are not system modes, it is possible that the overly persistent annular more timescales in comprehensive climate models may have no implication for their response to global warming (Sheshadri and Plumb 2017).

This is a good point, and we will modify the introductory text her to be more circumspect. We believe that the link was more useful in assessing CMIP3 and CCMVal2 (the Chemistry Climate Model Validation Project, Phase 2) because there were models that more radically over-estimated the annular mode time scales.

Should one reasonably expect tropospheric jet variability that extends equatorward of your 65âŰę definition (e.g. Madonna et al. 2017; Woollings and Blackburn 2012)?

Yes! As the second reviewer also had concerns about this simplified definition, we will better explain this in our revised draft. It turns out that the polar cap average does capture much of the variability of flow equatorward of 65 degrees, due to the dipole nature of the annular modes. Figure 9 of Baldwin and Dunkerton (2009) shows that correlation between the polar cap average geopotential is greater than 0.95 with the

full NAM at all levels, and greater than 0.99 in the stratosphere.

Fig. 3 Has this been smoothed? If so, it would be useful to people will might try to replicate this result to mention the details.

There is no smoothing, other than taking the daily mean. It looks smoother in part because chose a stratospheric level (10 hPa). At lower levels, you see more variability on synoptic scales.

P6 L18 Do you have theories as to why this would be the case?

This is related to the reviewers question above about why satellite measurements are needed in the austral hemisphere. We did note that JRA-55C appears to better capture the annular modes at upper levels than the surface: this may be due to the fact that annular mode captures

P9 L4 How do you deal with final warming (FW) events while defining strong and weak vortex events?

For consistency with previous work, we tried to follow Baldwin and Dunkerton (2001) procedure. It is possible that we have captured a few of the "dynamically" induced final warmings with our definition. In the revision, we will check to see if these late term events influence the result.

It might be useful to examine the downward influence of FWs in both hemispheres across reanalyses, particularly since this might be of relevance to the effects of the ozone hole on tropospheric circulation in the SH.

The influence of the ozone hole on the austral circulation will be invested as part of the SPARC Reanalysis Intercomparison Project.

FWs are an aspect of stratosphere-troposphere coupling that have conventionally been studied using annular modes, that this manuscript completely ignores. Also, in general, stratosphere-troposphere coupling is thought to be strong at the end of winter and into

early spring in the SH.

As noted earlier, we avoided the SFW in part because much of the literature (Black and McDaniel 2007a,b and Sheshadri et al. 2013), argued that the downward influence was not optimally captured by the annular modes. There is also less consistency on the precise definition of the final warming. The papers above defined it by a reversal of the winds at 50 hPa and 70 degrees latitude, while in Butler and Gerber 2018, a new definition using the winds at 10 hPa and 60 N was proposed (largely in an effort to make it more consistent with the definition of Sudden Stratospheric Warmings).

Another reasons that we avoided the final warming is that it cannot be defined from the annular mode index alone. It will be explored in the SPARC reanalysis intercomparison project. All this said, in the revision we will make a note that final warmings are important signal, especially in the austral hemisphere, where annular mode variability is concentrated at the end of the winter and spring.

P9 L19 It is somewhat inaccurate to refer to it as a downward propagating signal, as the word propagation is typically associated with the propagation of waves. Downward "influence" or "migration" might be a better choice.

We agree that this signal is not associated with the downward propagation of a wave. To avoid confusion, migration might be a more appropriate term and we will be careful to fix this in the revised manuscript.

P11 L2 There have been follow-up studies on the eddy feedback arguments of Lorenz and Hartmann that might be worth mentioning (e.g. Byrne et al. 2016), as these indicate that the persistence of the first annular mode might not really be the right way to think of eddy feedbacks.

We agree that these follow up studies should be discussed, both here and earlier in the manuscript, and will fix this in the revision.

P11 L6 Could you expand on how timescales are computed, since you do not use an

EOF-based definition of the annular mode?

We applied the same procedure as in Baldwin et al. 2003. The only difference is that we used our simplified annular mode index, as opposed to the EOF based index. In a large number of calculations that I did with CMIP5 models (which I unfortunately never published), I found that the time scales were quite similar using both indices.

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