**We appreciate the reviewer’s comments. Details on each comment are given in red below.**

**Reviewer A:** Comments on “Changes in Variability Associated with Climate Change” by Rosenlof et al.

Overall, I found this to be a well-organized, well written, and concise review of the topic. In particular, the outstanding research questions at the end of each subsection provide a strong foundation for future directions by the WCRP. In the spirit of strengthening the paper I offer the following minor comments:

1. The paper is lacking in illustrative graphics and figures beyond Figure 1. Sections 2-4 would clearly benefit from a few more figures that highlight the key points being discussed.

We have added a figure 2 to illustrate tropical width, and a figure 3 to illustrate projected changes in the NAM.

1. There is no mention of changes in monsoonal variability associated with climate change. Either there should be a reference to another paper in this volume or this topic should be treated here.

At this point, we don’t know what is covered in other papers in this volume. Since monsoonal variability was not covered during the session at the OSC, we have not covered it here.

1. Similarly, the PDO and AMO are only treated in passing at the end of Section 3.2. There should be a separate section that discusses decadal variability associated with climate change.

*This paper does not deal specifically with the PDO and AMO. They are mentioned only because they may affect the SAM.*

1. Section 3 would benefit from a summary discussion of similarities and differences in NAM and SAM variability associate with climate change.

*We added:* While many of the same issues pertain to the NAM and SAM responses to

*anthropogenic forcing, there are some differences in the factors affecting the two annular modes. A primary consideration is that polar stratospheric ozone depletion has been stronger in the Southern Hemisphere (SH) than in the Northern Hemisphere (NH) over the past few decades. Given that ozone depletion and GHG increase both act to strengthen the SAM and to shift it poleward, one may expect the annular mode response to anthropogenic forcing to be stronger in the SH compared to the NH in the late 20th century and weaker in the 21st century due to SH ozone hole recovery.*

1. Lastly, I found section 4 on GHG interactions with ozone depletion to be once removed from the focus of the paper on changes in variability associated with climate change. Should this section stay, it could be strengthened with a review of UTLS changes in water vapor and subsequent impact on ozone.

*We added:* There may very well be coincident changes in ozone, water vapor (a key GHG) and circulation. Randel (2006) demonstrated a strengthening in tropical upwelling led to decreases in stratospheric water vapor as well decreases in ozone in a narrow layer near the tropical tropopause. They note that part of the temperature changes may also be explained as a radiative response to the observed ozone decreases. The changes in water vapor were subsequently used in a model study that demonstrated that the water vapor change may have induced a surface temperature response (Solomon et al., 2010). There are clearly feedback processes operating here, but they are not fully understood and warrant additional study*.*

**Reviewer B:** Review of ‘Changes in variability associated with climate change’ by Rosenlof et al.

This chapter discusses past and future changes in several aspects of the atmospheric circulation. In each case a list of priorities for further research is given.

**General comments:**

1. My main issue is that the contents of the chapter do not match the title. Based on the title I was expecting to read a discussion of how the variability in atmospheric variables may or may not change under climate change – for example through summer reductions in soil moisture increasing temperature extremes. But, changes in variability are only discussed in 2.1 on ENSO, and 3.3 on sea ice, and even here they are not central to the discussion, with the other sections containing no discussion of changes in variability. I would recommend that either the contents of the chapter need to be substantially revised in order to focus on changes in variability in the modes discussed, with additional material added on aggregate changes in variability of temperature and precipitation. Or alternatively the title should be changed to more closely match the contents of the chapter. Most of the material relates more or less directly to changes in atmospheric circulation. So ‘Changes in atmospheric circulation’ might be an appropriate title.

*We have kept the title to be consistent with the session at the WCRP OSC and the talks given there. The bottom line is that we can’t cover everything, and prefer to leave the title and subject stand as is.*

1. In general the chapter would benefit from more citations to relevant published literature. In particular the chapter would benefit from more references to recent assessment reports such the IPCC Fourth Assessment Report and the WMO Ozone Assessment. For example the 2010 WMO Ozone Assessment has a whole chapter on ozone depletion and climate change, and it discusses in detail possible interactions between ozone depletion and climate change, but it isn’t cited. In several cases questions posed in the research proposals have already been directly addressed in the published literature.

The ozone assessment was already referenced (Forster et. al., 2011). We have added a subsequent reference.

1. The sections are a bit disjointed, and consider different aspects of changes in each of the phenomena described. Some effort should be made to ensure consistency of treatment across the sections.

We have attempted to make this smoother.

**Specific comments**

* A few figures might be helpful, improve the presentation, and help in communicating some of the results discussed.

We have added two additional figures.

* Pg 2, Introduction: First, this needs to be updated based on my general comment 1. If the title of the chapter is changed to focus on circulation, then the intro will need to be changed to reflect this. Second, some motivation for why we should be interested in changes in atmospheric circulation would be good to include here.

We have added the following two sentences in the introduction: “ We are interested in changes in the mean circulation and variability of that circulation ultimately because it impacts surface temperature and precipitation. “ (as motivation) and “We consider both changes in variability as well as trends in the mean state in our discussion “(to clarify what we’re covering in the paper)

* Pg 2, para 2, ln 6 onwards: ‘There are multiple ways to assess whether such changes may be occurring.’ The most obvious way to assess whether a change in atmospheric circulation is occurring is to use instrumental observations. The two methods listed – model simulations, and paleo climate data, will tell us about expected changes in circulation, or about changes in the preindustrial climate, but they won’t tell us whether circulation changes are actually occurring now.

*We have modified the sentence in question to read :* Past changes in climate variability can be addressed via analysis of historical data, both recent measurements as well as geologic records or ice core records.

Pg 4, para 1: Point out that there is a projected mean state shift in the Pacific towards enhanced warming of the western tropical Pacific. But this is best thought of as a change in the mean state, not a change towards more El Nino events. Cite and discuss IPCC AR4 10.3.5.3 and 10.3.5.4, and whether and how research has evolved since the publication of AR4.

*Paragraph in question as been modified.*

Pg 4, suggestion 2: I would not use the term ‘detection and attribution’ here. In IPCC usage, these terms refer to the detection and attribution of the response to a particular climate forcing in climate observations. I would say ‘Can models represent these mechanisms, and can observational networks support their characterisation in the real system?’

*Done.*

Pg 5, para 4: Note that Swart and Fyfe (GRL, 2012) find no significant shift in the annual mean position of the SH surface westerly wind maximum in reanalyses.

*Noted.*

Pg 5, para 5, ln 4-9: This is true for the width of the tropics, but its not true for the latitude shift of the SH mid-latitude jet, at least at the surface. Swart and Fyfe (2012) show that this shift is larger in models than in observations.

Paper now states “Neither reanalyses nor models show a robust trend in annual mean jet position over the historical period, though significant trends do occur in the Austral summer position.” Because neither appear to have a significant trend, we choose to not highlight the statement made that a shift is larger in models than in observations.

Pg 7, para 3, ln 7-9: Gillett et al. (2002) detected an anthropogenic influence in global SLP, not in the NAM. Note that Gillett and Stott (GRL, 2009) carried out a detection and attribution analysis on global seasonal SLP using data to 2009, detected anthropogenic influence, and did not find an inconsistency in the magnitudes of the simulated and observed responses. Also they showed that SLP detection was possible for the tropics separately, but not for the northern or southern extratropics, suggesting that the NAM did not dominate the detection in the earlier work.

*This comment is addresses in the revision.*

Pg 9, 7: The high latitude node of the SAM is really located at about 65°S, not at ‘polar latitudes’.

*We prefer to keep our original wording as this is consistent with the results in the cited Thompson et al. (2000) study.*

Pg 9, sect 3.2, ln 15: Is the SAM response to solar forcing really robust?

*We did not mean to imply that the SAM is sensitive to solar forcing, but to changes in earth's orbital parameters. We have therefore omitted "solar output" as requested.*

Pg 10, para 3, ln 3: Replace ‘cause’ with ‘drive’ – ‘cause’ to me implies that the net SAM trend will be negative.

*Done as suggested.*

Pg 10, research questions: Question 2 is to some extent answered by Kang et al. (Science, 2011) who demonstrate a SAM influence on the subtropics. Question 3 is addressed by Li et al. (JGR, 2010) who identify an influence of the Brewer-Dobson circulation on the SAM.

We have rephrased questions 2 and 3 slightly to indicate that some work has already addressed these issues. We also added a sentence at the end of the paragraph following the questions that cites the work of Kang et al. (2011) and Li et al. (2010) as suggested.

Pg 12, para 3, ln 1-2: This seems to contradict the argument made above that the variability scales with the square root of the mean extent.

*This discussion is valid only for the Arctic, the scaling with the square root of the mean extent being valid for the Southern Hemisphere only. This is now specified in the revised version.*

Pg 13, section 4, para 1: This section should refer to Chapters 3 and 4 of the 2010 WMO ozone assessment, and also Eyring et al. (GRL, 2010) and Eyring et al. (Atmos. Chem. Phys., 2010). The latter two studies directly addressed the effect of climate change on ozone in state-of-the-art coupled chemistry models.

*The Ozone Assessment was already referenced. The Eyring CCMVal GHG reference has been added.*

Pg 14, ln 1: It seems strange to just cite one study here, when there are tens or hundreds of such studies. This could at least include a reference to chapter 4 of the WMO Ozone Assessment.

*Ozone assessment reference added another time (note this is Forster et al., which may differ from the WMO 2011 the reviewer may have been expecting.)*

Pg 14, para 2, ln 8: The increase in Antarctic sea ice is not attributable to stratospheric ozone depletion. Stratospheric ozone depletion causes a decrease in Antarctic sea ice. See Sigmond and Fyfe (GRL, 2010). This finding has since been reproduced in other other models – reported in submitted papers by Polvani and colleagues and in an eddy-resolving model by Bitz and colleagues.

*This sentence has been modified to be more neutral and the reference to Sigmond and Fyfe 2010 has been added.*

Pg 14, para 2, ln 11: But there is of course relevant literature on the effect of ozone depletion on carbon uptake including studies by Le Queré and colleagues and Zickfeld and colleagues. Also this topic is reviewed in Chapter 4 of the WMO Ozone Assessment.

*Ozone assessment reference added.*

Pg 14, para 2, ln 11: A link between ozone depletion and NH circulation has not been seen in obs, but it is seen in models. See, for example, Morgenstern et al. (JGR, 2010).

*We reworded the sentence to specify that the link has not been seen in observations.*

Pg 14, Research questions: There is already a lot of research on these issues, in particular as summarised in the WMO Ozone Assessment. Question 1 is answered using state-of-the-art models in chapter 3 of the assessment – the answer depends on the location. Question 2 is also answered there. In most regions the responses to GHGs and ODSs add almost linearly. Question 3 is answered in chapter 4 of the WMO assessment.

*We agree there has been a significant amount of research done on this topic, but not that the answers are definitive. In particular, it is not clear that the state of the art models are entirely accurate in the stratosphere. We choose to leave these questions as they stand.*

Pg 15, para 4, ln 7-8: Whether or not coupled chemistry is required depends on the process of interest.

*If stratospheric radiative processes are important, ozone needs to be accurately simulated. For that to be true, ozone chemistry needs to be included. We will leave this sentence as is.*