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Discussions

Interactive comment on "Nighttime Mesospheric Ozone During the 2002 Southern Hemispheric Major Stratospheric Warming" *by* Christine Smith-Johnsen et al.

Anonymous Referee #2

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First review of "Nighttime Mesospheric Ozone During the 2002 Southern Hemispheric Major Stratospheric Warming" by Smith-Johnsen for possible publication in ACP. The paper presents SSW effects on the secondary ozone maximum in the SH using Specified Dynamics version of WACCM and compares to GOMOS ozone measurements. These SH results are contrasted with NH SSW effects reported by Tweedy et al (2013). Increases in nighttime polar mesopause ozone are attributed to enhanced upwelling and cooling and "effects of atomic oxygen" (vs. hydrogen, as is the case in the NH). I have questions about the spatial regions chosen for the averages, concerns that the model output is not at the GOMOS profile locations, and left wondering how the results are different from and novel compared to Tweedy. The paper discussions are too brief,

and so the authors have the opportunity to expand on the significance of geographical distributions (if they are relevant this might be a possible novel result to emphasize?), and do a more thorough job of comparing temperature and trace gas distributions to other satellite observations (such as SABER as mentioned by Siskind). I think this paper has the potential for publication but not in its present form.

General Comments:

Need line numbers for the next review.

The intro is too general. Give brief motivation and literature review of SSW effects on the secondary ozone layer and why it is important to understand this coupling.

2. Methods, model and instrumentation – mention the accuracy of SD-WACCM mesospheric temperatures. I believe there are very large biases that depend on latitude and season. What are the biases in the Antarctic winter polar night jet above the nudging altitude? Cite others such as Garcia, Marsh, and other papers by Smith who go into more detail on model specifications.

Defend averaging 55-75 instead of 60-pole or 70-pole. Why an annulus instead of a polar cap area average?

Specific Comments:

Scherag-> Scherhag

This sentence doesn't make sense: "The main ozone source at this altitude is atomic oxygen, while the sinks are atomic hydrogen and atomic oxygen." Please check. Is atomic oxygen a net source or a net sink? What about molecular oxygen talked about in section 3.2?

Smith et al. (2014) -> should be 2015

Figure 1 – add figure panels a) – h) for reference. Figure 1 does not support this assertion, "After the onset, the PNJ recovery to eastward direction is only found above

10-1 hPa, while between this level and 101 hPa, westward winds persisted into the summer."

Figure 1 of w, T, O3: why average between 55-75 and not over the polar cap 75-pole? The anomalies the authors attribute to the 4 SSW pulses are not apparent. Averaging over the polar cap is more physically appropriate due to the meteorology (roughly vortex centered vertical motions, T, tracers) as well as providing more nighttime conditions. The current analysis is neither appropriate nor convincing.

Clarify "The zonal wind reversals mark strong ozone departures from the climatology in the mesopause region." – what does this mean? Change "mark" to "coincide with" ???

Figure 2: I wonder if averaging over the polar cap will result in more robust correlation between ozone and atomic oxygen? Give correlation coefficients between ozone and other variables. The statement "Atomic oxygen shows weak decreases, within the standard deviation of the climatology." Is not really supported due to the large fluctuations. What appears more clear is that overall ozone is elevated because overall (on average) hydrogen is depleted. In addition, the short term ozone enhancements during the SSW pulses appear to nicely coincide with episodic low hydrogen values. This seems consistent (not contrary) to the NH results of Tweedy.

Figures 3-4: The paragraph devoted to describing these figures is too brief. Can DIFFERENCES from 20 Sept to 25 Sept be shown to clarify where there are changes in the different fields? I think showing the "geography" of the ozone changes is a novel result but the discussion is lacking and the authors do not pursue this avenue. Figure 3 caption and text: specify how many days before the major SSW. But given the multiple warming pulses, isn't it really both before and after SSWs? Figures 3-4: consider combining these figures with F3 as the left column of 4 maps and F4 as the right column of 4 maps. Side-by-side would make the comparison easier. Why are these two particular days chosen? Are they representative? Increase ozone color bar range. Add

contours to all panels. Specify outer boundary (30S?). How well does the temperature map compare to SABER (or any other measurement of) temperature at these altitudes on these days? Can we trust daily geographic maps of SD-WACCM fields at these altitudes? If so, say so explicityly. If not, add a discussion including caveats.

Section 3.2 this is the first mention of molecular oxygen. Figure 5 – add figure panels a), b). In discussion of these results be explicit about comparisons to Tweedy. What is different and what is novel?

3.3 GOMOS satellite observations – move discussion of the instrument sampling and dataset gaps up to section 2.

Figure 6. I agree with reviewer 1 that this analysis should be SD-WACCM at the GOMOS profile locations (not spatial averages where GOMOS did not sample). This analysis needs to be re-done as it is critical to the paper. It should also be given more emphasis. Interpretation of results would be easier if the authors included GOMOS and SD-WACCM on the same plot. Is there agreement between the model and the observations? If so, what do both say about SSW impacts on the secondary ozone layer that are different from Tweedy? If not, why not and what does that mean in terms of the model capabilities.

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