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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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General comments

The manuscript addresses a topic of high scientific interest for knowing the state and variability of the mesosphere, in particular, the perturbations of the O3 secondary maximum distribution to a major SSW in the Southern Hemisphere. It is then within the scope of ACP. I have though some major issues with the current version of the paper that I recommend to be addressed before accepting the paper for publication.

a) This is a short paper and about 90\% of it is just a repetition of a MODEL analysis of a SSW (e.g. as in Tweedy et al., 2013). In this case the novelty is that it occurs in the SH but, as shown, the physics (dynamics) and chemistry is the same as in the NH. Apparently the new aspect is the more important role of O in the SH than in the NH

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during SSWs (see the abstract). However, as discussed in text, it seems more related to the timing of the SSW along the winter rather than to the hemisphere itself. Then, I cannot see significant differences between the effects of SSWs in the NH and SH, a major motivation of the work. Therefore, the current version of the manuscript seems like an exercise and with very little NEW information.

As the manuscript is mainly based on model analyses, I would have expected (and I suggest to make it for the revised version) a more detailed analysis of the differences between the dynamical effects in both hemispheres. I think this would be interesting because the origin of GWs is substantially different in both hemispheres and this might have a different impact on the propagation and evolution of the SSWs effects. This would require model comparisons in both hemispheres.

b) The experimental "evidence" of SSW in mesospheric O3 in the SH from GOMOS is not very convincing. Fig. 6 shows that the O3 enhancement actually occurs 2 days BEFORE the response in the model, e.g. before the actual SSW. Also similar enhancements are seen at other days in GOMOS measurements which are not particularly correlated to SSWs. Personally, I am not convinced of such effect from Fig. 6.

This "experimental" evidence is relegated to just one page at the end of the paper. I would have written the manuscript the other way around. First show the evidence of the effect and then do the analysis. However the evidence is so weak that I doubt if it worth presenting it and then focus the manuscript only on model simulations.

In case the authors (or editor) decide to include the measurements, I would strongly recommend to include a point-by-point measurements/model comparison, by using model output at measurements geolocations. Not having WACCM data available at the time of writing should not be a reason for not performing an appropriate analysis. Such a comparison is fundamental for the paper, if including the measurements.

c) I convey with David Siskind's comment that the previous work by Cox et al. GRL,

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2005, should be discussed and the model inter-comparison are highly recommended, particularly since the result on the hemispheric differences (SSW timing differences at the end) are based on WACCM model simulations.

Comments:

Abstract.

Lines 12-13. I would have written the sentence the other way around, e.g., model reproduces the measurements. Or change "demonstrate" for "show".

Lines 15-17. This sentence does not fully reflect the results. From this reading it seems that O/H plays a different role during SSWs in the Southern and Northern Hemispheres. However, as discussed in the manuscript and summarized in the Conclusions (lines 246-248), this seems more a question of the timing of the SSW along the winter rather than related to the hemisphere itself. I suggest to re-write the sentence. Also, state explicitly that it is a model result, i.e., not seen or being derived from the GOMOS measurements.

Lines 57-59. Note that the units of c1 are different from those of c2 and c3. They are cm6 molecules-2 s-1.

Lines 68-69. According to this statement I was then expecting to see the evidence (GOMOS measurements) first and its explanation with WACCM simulations later, not the other way around. I am not fully convinced of this "for the first time" O3 enhancement during a SH SSW (see below).

Sec. 3.1

Related to previous comment, make clear from the beginning in this section and in the legend of the figures that the results shown in Figs. 1-4 are model simulations.

Line 177. Would then be better to use zonal mean of nighttime data only?

Lines 222-223. Sentence: "The largest enhancement is observed in the end of

September, when ..." But the largest peak in the measurements occurs 2 days BEFORE the SW, on 23rd Sep !!

Line 229. About the sentence "This may also contribute to the lack of exact day-to-day correspondence with the WACCM-SD..." Correspondence with WACCM is very important since it is nudged so it should reflect very well the timing of the SSW. So this correspondence is crucial to attribute the causes to the effects and, in my opinion, the lack of such correspondence cast doubts on the evidence of the SSW effects on the mesospheric O3 in GOMOS. By the way, the drawing of the "Sep. 23" line in Fig. 6 is misleading since the SSW actually occurs on Sep 25 (see Fig. 5).

Lines 231-233. To make a direct comparison GOMOS/WACCM is essential to this study and to the confirmation of the O3 enhancement during this SSW. You should consider the temporal and geographical collocations of WACCM with GOMOS measurements in order to make the comparison meaningful and credible.

Lines 236-239. I do not agree with this sentence. In my opinion, in the best case, the detection of the O3 enhancement during the SSW by the presented GOMOS measurements is questionable.

Lines 246-248. Then, the attribution of the O3 changes in the studied SSWs in the SH and NH seems more related to the timing of the SSWs than to the hemisphere itself. This should be reflected in the abstract.

Figs. 1-4. Clarify that the plotted results are model calculations

Fig. 2. There is no "dash" line, is it the THICK GREY line?

Figs. 3 and 4. Typo, mesospause -> mesopause. Better say "around the mesopause"? (The mesopause is not defined by the 1e-3 hPa pressure level).

Fig. 5b. It might be more clear to show the effects of perturbing T, H or O on the O3 anomaly rather than O3 itself.

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