

Interactive comment on “Ensemble simulations of the role of the stratosphere in the attribution of tropospheric ozone variability” by P. Hess et al.

Anonymous Referee #2

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Review: “Ensemble simulations of the role of the stratosphere in the attribution of tropospheric ozone variability” by Hess et al.

This study examines the stratospheric influence on tropospheric ozone using a set of four WACCM ensemble simulations. This topic is currently of significant interest in the community. Overall, this work is well written and provides an extensive analysis of the simulations. However, I feel the authors need to adequately address the specific issues outlined below before I can recommend publication.

Primary concerns:

1. Several of the results should be compared/contrasted in more detail with previous studies. In particular, the magnitude of the stratospheric impact on the tropospheric

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ozone found in this work appears to be much greater than what is found by Neu et al. (2014) in their observational based study. Also the ozone flux across the 150 hPa surface ($\sim 1.1 \times 10^4$ kg/yr; Figure 7) is significantly less than current estimates of stratosphere-troposphere exchange of ozone (For example, the model study by Hsu and Prather (2009) and the observation/model study by Olsen et al. (2013)). I first assumed these results were per unit area and just missing the units, such as km². Even if so, the reported values are still much less than current estimates.

2. Much more discussion needs to be made of the lag times used in the correlations. The lag times were selected by minimizing chi squared. However, these considerable lags (e.g. Table 3) do not seem to have any physical justification. The tropospheric lifetime of ozone is thought to be on the order of a month (e.g. Stevenson et al., 2006). If a mass of ozone descends into the troposphere, I would expect most of those ozone molecules to be lost by 5-6 months later. Later in the paper (p20486), the first EOF is associated with regions of known stratosphere-troposphere exchange, particularly deep exchange. The works cited do demonstrate these preferred regions but they also demonstrate that the exchange is relatively rapid. This does not support the justification of 6-9 month lags used for the correlations in Table 4. Also, a 3-month lag for the 150 hPa EOF correlation with ozone flux at 150 hPa seems counterintuitive. However, this could be justified if the air mass flux was significantly out of phase with the seasonality of ozone.

Other comments:

I question why many of the figures are placed in the supplementary material. Many of these are presented and discussed on par with the rest of the material in the paper. These would not be “supplemental” and only make it more difficult on the reader to have to jump back and forth between two different places to look at the figures discussed! Most of these should be placed into the body of the paper.

P20461: The title of the paper should reflect that the study considers only the Northern

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Hemisphere extratropics.

Section 2.1: It would be helpful if it were explicitly stated earlier that these are free-running simulations. I currently see that in the beginning of the conclusion section. Also, this section could additionally describe how the ensemble members were created (differences in the initializations).

P20473, L17: This really doesn't suggest a long-term ozone decrease, especially with the large standard deviation. It appears rather flat over the long-term.

P20473, L26: Given this statement, should the Northern Europe value in the Table be in bold?

P20474, Section 3.2.2: As I understand it, the model values are averaged over the region and the observations in each region are averaged together. If so, I am not surprised that the 500 hPa measurements have a standard deviation much larger than the model but they are much more comparable at 150 hPa. The spatial variability of ozone in the troposphere is much greater than in the stratosphere. An average of a small sample of points in the troposphere (the observations) is likely to have greater variability than the average over that continental-scale tropospheric region. Figure 4 also provides supporting evidence of this. The standard deviations are quite large (and time series look completely different) during the earlier record when there are far fewer sites and measurements. After about 1980 in Canada and the early 90s in Europe, the number of sites increase and observation frequencies become greater. This corresponds to the time when the observations and model results begin to agree much better.

P20478: It appears that the increase after 1990 could be due to the impact of Pinatubo. Thus, it appears that the 1960-2005 trend would be fairly linear if 1990-1995 were removed from the time series.

Table 4, note 3: I don't understand exactly what you mean by "The correlation in paren-

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thesis is computed individually for each simulation; however, the correlation coefficient comprises the overall relationship for all ensembles."

P20488, L6 (referencing Fig. 13): The individual titles in each panel of Figure 13 labels Mace Head, Lassen, and Alpine as the surface rather than 500 hPa. I assume it should be 500 hPa.

P20488, L28: And the most minor comment. . . the words "entire" and "the" should be reversed!

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 20461, 2014.

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