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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling Trans-Pacific Transport using Hemispheric CMAQ during April 2010: Part 1. Model Evaluation and Air Mass Characterization for the Estimation of Stratospheric Intrusion on Tropospheric Ozone" *by* Syuichi Itahashi et al.

Anonymous Referee #3

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Review of the paper

Modeling Trans-Pacific Transport using Hemispheric CMAQ during April 2010: Part 1. Model Evaluation and Air Mass Characterization for the Estimation of Stratospheric Intrusion on Tropospheric Ozone by Syuichi Itahashi and co-authors

The paper is the first in a series of two investigating CMAQ simulations of trans-pacific pollution transport for one month (April) in 2010. In contrast to what the main title

promises the paper deals exclusively with ozone transport from the stratosphere and a method for a quantification of the stratospheric contribution to tropospheric ozone. The approach followed here to answer this question is quite interesting and generally well described. However, the study suffers from the extremely short period under investigation. The authors justify why they selected this period for their study. They claim that it was published earlier that trans-Pacific transport played an important role during this period (Uno et al., 2011, Lin et al., 2012a). However, they try to draw more general conclusions about the contribution of stratospheric ozone to the concentrations in the troposphere and the model performance. I believe these findings on biases and model skill are not well justified because the data set used for this type of evaluation is too small. I suggest that the authors investigate a longer simulation period in order to derive statistical parameters for the model performance and the stratospheric contribution to tropospheric O3. They can then still investigate April 2010 as a special case in more detail like it is done now.

Specific major comments:

Page (P) 1, line (l) 21: You describe the bias in RH given by H-CMAQ, however RH should be a quantity given by the driving meteorology model, which in this case is WRF.

P 1, l 28-32: These statements are based on an investigation for April 2010 but the reader gets the impression that they have a more general validity. You should extend your evaluation period for this type of conclusions.

P 2, l 17: Is it still true that the emissions in East Asia increase? There are more recent publications reporting the contrary.

P 3, l 1/2: How can a publication from 1999 say something on real trends until 2010? You need to point out that this was a model study looking into the future. In addition, it would be interesting to know if the predictions for the development of Asian emissions were correct.

P 3, l 8/9: When Lin et al. modelled May/June 2010 and you do April 2010: Why don't you extend your model period and put the results in perspective to their results?

P 3, l 19-21: Because this is the case you need to cover other seasons with your model in order to evaluate it properly.

P 3, l 29/30: This objective is not covered in this paper at all. You should say more clearly what the objective of this paper is.

P 5, l 1 / 2: You state that O3 in the stratosphere is parameterized based on PV from WRF and an O3-PV function from Xing et al. (2016). Could you say a few words about how accurate this parameterization is?

P 5, l 17-19: Why did you simulate only such a short period? Is it computationally expensive to run the model? Which boundary conditions of those reported in the Hogrefe et al. (2018) paper did you use?

P 8, l 13-30: Again, given these significant deviations between model results and observations, it would be beneficial for your interpretation if you extend the modelled time period.

P 10, l 8/10: Doesn't this suggest that the scaling approach is not accurate enough for modelling ozone concentrations in the upper troposphere. So isn't there a need for adding a model component that covers stratospheric ozone with its entire chemistry and dynamics?

P 10, l 19-21: What could be the reason for this positive bias if it occurs despite nudging of RH from reanalysis data?

P11, l 10-20: The comparison of the ozone profiles to the model values (Table 3) suggest that the model gives too low O3 concentrations, in particular in the free troposphere. You also state this in l 9/10 on P 11. However, in Figure 7 we see a mostly positive bias with too high modeled column values, in particular over the continents, where the O3 soundings were performed. Could you explain this? Does it tell me that

satellite observations deviate quite much from ozone soundings? And oes it mean that your findings whether the model is too high or too low depends on the observations you compare it with?

P 13, l 16-18: This is another example for the main problem of this study: You investigated April 2010, only, but you give the impression that you could draw more general conclusions out of it. You should extend the modeled time series in order to give these conclusions.

P 15, l 21-24: Which measure did you use for saying the model has good skill for representing the main hemispheric O3 distribution? The model is obviously too high over Africa in the equator region and it shows higher values over continents and lower values over oceans compared to the satellite observations.

Minor comments:

P 2, l 16: do you mean that the number of low O3 days increased or the concentrations on the low O3 days? These are very different things and it is not clear, here.

P 14, l 31: shows

P 15, l 31/32 and P 16, l 8: which emissions lead to high surface O3 mixing ratios?

P 17 – 22: The references need to be revised with respect to formatting and initials.

Figures:

Figure 2: It is impossible to judge the distribution of the red and the green points when the blue squares are plotted in this way.

Figure 4 and Figure 5: These plots look nice but I think not all of them are needed. You may put some of them into the supplemental material.

Figure 8: It is not clear to me what the exponential fit stands for. Is it used somewhere else?

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