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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of transported background pollutants on summertime Western US air quality: model evaluation, sensitivity analysis and data assimilation" *by* M. Huang et al.

Anonymous Referee #1

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General comments

This study conducted a series of model experiments to explore the sensitivity of simulated background ozone over the western United States for one month period (June 5-July 15, 2008). I do recognize the authors' great effort to conduct lots of model experiments to address the uncertainties in estimating background ozone. However, the analysis is lack of focus and in-depth discussions. The authors need to carefully interpret the results. Some statements in the abstract are confusing as discussed below, and I do not see convincing analysis in the paper to support these conclusions.

Major comments:

1. In the abstract, the authors stated that “Forward sensitivity simulations show that TBG extensively affect Western US surface O3, and can contribute to >50% of the total O3, varying among different geographical regions and land types”. This is a very big statement, how do you get the number “>50%”? Figure 4a shows that the maximum background occurs over the EPA Region 10 and the magnitude is no more than 15 ppbv, which is up to 20% of total ozone shown in Figure2c. In fact, the simulated total ozone at 60 km resolution is biased high by approximately 10-20 ppbv over the EPA region 10. Is this bias caused by excessive background influence in the model? A very important piece currently missing in the analysis is the influence of model bias on estimated background. Comparing Figure8a and Figure 7a, the strongest sensitivity occurs on days when the model bias in total ozone is largest, e.g., June 18-22, June 24-25, and July 12-14. The model is clearly overestimating background influence on these days. How these biases affect the overall conclusion regarding the contribution of transported background to surface ozone should be clearly addressed.

2. In the abstract, the authors stated that “The stratospheric O3 impacts are weak”. This statement is vague. Do you actually implement a tracer to quantify the influence of stratospheric ozone? It is better to just state what you have done, i.e., the sensitivity of simulated ozone to the top boundary conditions. What is the time frequency of the top boundary conditions? What is the model top? Is the model vertical resolution fine enough to resolve the tropopause? This important information related to STE is currently missing in the manuscript. Figure 7 shows that the model fails to reproduce the layered structure of observed ozone at the Trinidad sonde site, which raises the concern about the credibility of the model in attributing the influence of stratospheric ozone.

3. Abstract, Lines 19-20: “The probabilities of air masses originating from MBO (2.7 km) and THD (2.5 km) entraining into the boundary layer reach daily maxima of 66% and 34% at ∼3:00 p.m. PDT, respectively, and stay above 50% during 9:00 a.m.–4:00 p.m. for those originating from SC (1.5 km)”. Using the different time frame for

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MBO/THD and SC is confusing. I think you want to compare the lower free troposphere (MBO and THD) versus the surface site (SC).

4. Abstract, last paragraph and section 3.3.2: This seems to disconnect with the focus of this paper regarding transported background ozone. Better if you can phase the discussion in terms of how the assimilation improves estimated background.

5. Different emission data (for both anthropogenic and biomass burning sources) are used for the model simulations at 60 km and 12 km horizontal resolutions. This model setup precludes the capability to isolate the influence of emissions vs. transport processes on simulated ozone. Please quantify the percentage difference for total NOx and VOCs over the common domain in the two emission inventories. In page 15250, the authors attribute the ozone bias at the Trinidad Head sonde during June 28-29 in the 60 km simulation to the uncertainties in biomass burning emissions. In Figure 4b, there is a clear hotspot of biomass burning influence over Northern California. Is this hotspot still present if using BB emissions from the 12-km simulation?

6. Figure 4, please clarify whether the w126 weighting function is applied before or after the subtraction of two sensitivity simulations. I believe the results are likely very different. How does the calculation method affect the attribution?

7. Figure 1: Is this the only flight available during the study period? Why not show the overall statistics (observed vs. modeled mean and standard deviation for each 1 km altitude bin) using data from all flights available?

Specific comments:

Page 15236, Line 20: what is the resolution (thickness) near the transition layers, e.g. near the PBL top and near the tropopause? I think this can explain why less background influence is estimated if the model include more vertical layers.

Page 15237: Line 12: Time-varying BCs at what frequency?

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