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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Impacts of transported background ozone on California air quality during the ARCTAS-CARB period – a multi-scale modeling study" *by* M. Huang et al.

**Anonymous Referee #2**

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This paper describes the transport of ozone from Asia to California during the ARCTAS-CARB experiment in June 2008. The authors compared in situ measurements from surface stations in California and NASA-DC8 with the STEM model output at different resolutions. They confirmed that Asian inflow can increase surface ozone in California. They also showed that using different model lateral boundary condition of ozone tends to modify ozone concentration in the boundary layer of STEM. Those results are interesting and make this paper useful. I recommand this paper for publication after addressing the following comments.

Specific comments:



The ozone concentration and trajectory studies (section 3.1, 3.2 and 3.3) are based on the 12km run because transport processes and ozone concentration are better represented in the 12km run than the 60km run (first paragraph section 3.1). Surprisingly, section 3.3 that treats the lateral boundary conditions (LBC) sensitivity uses exclusively the 60km run to show differences in ozone over California using different LBC. The authors introduce this section by saying page 12097 line 8 "To demonstrate the impact of the western LBC on surface ozone predictions over California, a series of LBC sensitivity simulations were performed in the 60km model grid". In my opinion, you cannot say that. You demonstrate that the 60km run has a strong sensitivity to LBC conditions, but to demonstrate the sensitivity of ozone prediction at the surface by the STEM model, I think the results should be based on the 12km run as the 12km run is the one that you would use in case of ozone prediction with STEM.

In section 3.4, the model performance is analysed using a base case and a observation case for LBC. Figures 16 and 17 show that the model at 60km and 12km doesn't have the same response at the surface due to a change in LBC. If you are right by saying that it's because of the mesoscale flows in central valley, does that mean the results in the 60km and 12km run are similar at say 3km in altitude? If this is the case, it doesn't explain why the ozone background at TB is so well represented by the 12km run (Fig9b). I think that the reduction of ozone at the surface of the 60km run due to changes in LBC improves the surface ozone prediction but for the wrong reasons.

Speaking about "surface ozone prediction", it would be interesting to know by how much is the ozone prediction improved (or different) by changing the resolution from 60km to 12km. Figures 18 and tables 3a 3b only "quantify" the significant improvements at different surface stations (3 stations for the 60km run, 1 station for the 12km run). It can potentially confuse the reader. You should show the results of the base case and observation case of the 12km and 60km run for the 4 stations on figure 18. At least we will see how different are the 60km and 12km runs.

Furthermore, the improvement of ozone at the JOT station in the 12km run is so small that it's hard to say it's significant. The surface ozone prediction improvement in the 12km run is not very convincing. I would think that more work is needed on other parameters (surface emission inventories, vertical transport, sea breeze, etc...) to improve surface ozone prediction in the model before modifying the LBC. Results along the DC8 flight (figure 19 and section 3.4.3) are more convincing though. Maybe the reason is that the ozone prediction along the DC8 flight is less dependent on the accuracy of surface ozone precursor emission inventories and mesoscale transport at the surface. It would be interesting to add some comments in the text about that.

My second point is that it is hard to speculate on any future improvement of the model from a 2-day Asian inflow event using your methodology. I agree that the ozone predictions in the boundary layer are improved by using the observation of the DC8 rather than the LBC from RAQMS. Is that always the case anyway? I assume that there are cases when the latitudinal variability of ozone is larger due to differences in long range transport from Asia (passage of a cold front for instance). What would have been the consequence of using in situ measurements made in a region of 5 degrees in latitude as a LBC in such a case? You should be more cautious in your comments and conclusions about improving the surface ozone prediction with localized in-situ observations instead of RAQMS LBC. With an analysis based on a 2-day event, you cannot say page 12104 line 2 "Accurate real-time LBC for long-live species together with high quality meteorology fields improve model predictions at areas where the background pollutants are transported aloft as well as their downwind regions". This conclusion is definitively overstated.

technical comments:

figure 14a: You should plot red dots and not lines between the data points from the DC8.

page 12084 line 11: "high resolution meteorology fields". high resolution is somewhat arbitrary. Better say "12km resolution" instead of "high".



WCB: put in the text that WCB stands for western clean boundary.

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