

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 #1

Received and published: 21 June 2010

This paper describes a model analysis of the influence of ozone-rich air from over the Pacific on surface ozone in California during the period of a measurement campaign in June 2008. It focuses on the transport of ozone from coastal to inland areas and quantifies the sensitivity of this transport to the choice of boundary conditions, comparing with ozonesonde profiles and surface observations at four locations. The focus on summertime conditions is particularly welcome, as the most intense ozone episodes from local emissions occur during this season, and previous studies have generally focused on periods of rapid transport during spring. The performance of the model is quantified, and the use of above-surface measurements to constrain the vertical profile of ozone over the Pacific and its variability is shown to improve representation of the

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inflow to ozone over the west coast.

Although the paper is interesting and the concluding recommendations are useful, it provides only limited new insight into the problem. The study confirms the earlier observation-based analysis of Parrish et al., 2009, but misses an opportunity to extend this to wider temporal or spatial scales. The model applied here should be able to provide a monthly or seasonal picture of ozone contributions beyond the very short measurement period considered here, quantifying the variability in these influences, and relating them to meteorological conditions in summer. While the focus on the measurement period will be of great interest to those involved in the campaign, the study would be of value to a much larger audience if it provided a wider seasonal context, allowing a more robust estimate of surface contributions and their variability. This would make the results relevant to larger-scale issues, such as how surface ozone over the West Coast might be expected to change in future. I would recommend that the authors consider making an additional model run that would allow them to extend the results of their study to monthly or seasonal scales.

The measurements are shown to provide better boundary conditions than the RAQMS model, a not unexpected result. Can a suitable method be derived (based on meteorological variables or some assessment of other RAQMS variables) that would allow improvement of model performance when measurements were not available? This would also add significant value to the study.

Specific comments

p.12085, l.12: Please explain "highest O3 design value"

p.12086, l.10: Some details of the tagged tracer scheme are needed here. Are the tracers used real or artificial? How are they removed, and what decay rates are applied? CO has a large secondary source, is this accounted for? How appropriate is this scheme for estimating Asian impacts, given that the contributions identified here are large?

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p.12088, top: Why were different emission date sets used at the different model resolutions? How much do they differ?

p.12088, l.26. State the time resolution of the LBC data applied here (6 hours?)

p.12089, l.5. Please explain "Step and Stare".

p.12090, l.8. The use of "complicated" is not informative here - more variable? In space or time?

p.12090, l.15. How typical are the meteorological conditions described here? Providing a longer-term context here would make the results much more useful.

p.12091, l.27: These fire events are not introduced earlier. Where did they occur and how large were they?

p.12093, l.25: The equation here is the standard definition of a mean, and is not needed.

p.12097, Section 3.3 How do the results here compare with the (numerous) other studies that have evaluated the impact of boundary conditions?

p.12101, The use of "SI" in equations 3 and 4 is confusing - it is described in Eqn.2 as a percentage. Eqn. 3 and 4 should be reformulated to clarify them.

p.12107, l.6: reference should cite NASA as a source here

p.12107, l.7: Parrish references not adequately differentiated.

p.12107, l.20: NCEP (or NOAA) should be cited for authorship here.

Figures

This paper has a lot of figures, and I would encourage the authors to reduce the number for clarity. In some cases it would be clearer to show changes at selected locations rather than all of them (Figs 6-8). Specific recommendations follow.

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Several figures show both observations and model results (Figs 9, 13-15, 18) but the color schemes for these figures is different. The figures would be easier to interpret if consistent colors were used, e.g., observations blue (or black?) and model results red.

Fig 1: It is not clear if the regions marked represent the model domains, if so, please state this in the caption. The flight track in panel 3 would be clearer in white.

Fig 2: panel (b) should be reoriented (centered on the date-line?) or a more suitable projection used. It would be helpful to include the flight track in panel c and/or d.

Fig 3: This is not easy to interpret without additional meteorological information (e.g., mean sea level pressure?). I would suggest removing this figure.

Fig 5: Note in the caption that these are observations.

Fig 6: Elevation labels would be clearer if moved into the figure caption.

Figs 16, 17: These figures would be easier to interpret if they used a two-color scale (e.g., red-blue) with values around zero in white. The rainbow color scheme masks regions of agreement (close to zero) and is used differently for O3 and CO. The captions for these figures would be clearer if they stated what the figures show, e.g., the difference in O3 and CO if using observation-based boundary conditions rather than the default RAQMS LBC (i.e., Base case - Obs case),

Minor issues

There are minor grammatical inconsistencies in a number of places, e.g., in point 2 of the conclusions.

The abbreviation "a.g.l." needs to be spelled out somewhere.

p.12081, l.9 "Oceanic O3 profiles" -> "O3 profiles over the ocean"

p.12104, l.5: remove 'are needed'

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12079, 2010.

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