

Author's reply to Referee #2

We like to thank the referee for her/his time and effort and appreciate the constructive comments and suggestions to improve the quality of the manuscript. We further would like to acknowledge the recommendation for publication. We made changes to the manuscript to reflect the comments and suggestions made by the referee. Please find below a response to the individual points.

This manuscript gives some interesting and important results by analyzing and simulating ARCTAS/CARB, MOZAIC, TES and ozonesonde data in western United States. This referee thinks that it does add some new values and could be accepted for publication if the following comments/concerns can be well-addressed.

First, the discussion part of this manuscript is probably too long, which makes it less focused and difficult to follow. It will be better if the authors can condense the Section 3.1 (i.e. Section 3.1.1-3.1.5).

We understand the point the referee raises, but at the same time also find it hard to cut down on the discussion given the amount of important information available from the data. We however tried making these sections easier for the reader by reducing the details given in the text, such as avoiding stating numbers the reader can extract from the Tables or omitting some details that are not crucial for the discussion. Please see the revised manuscript for all the changes that have been undertaken.

Second, the authors used two models to do simulation for the ARCTAS/CARB experiment. The readers should be interested if they also present the WRF-Chem simulation results in Fig.2, 3, 5, 7 etc or in a new graph. That definitely can give some new information about the uniqueness of a high-resolution regional model in reproducing detailed structure/layers of pollutions, at least in the lower troposphere.

The referee makes a very good suggestion and analyzing the differences between the global and the regional model in detail would make for an interesting analysis. We are concerned though that adding additional graphs and accompanying discussions of such a comparison significantly lengthens the manuscript and diffuses its current focus. As an alternative we include a supplement in which WRF-Chem results are added to Figures 2, 3 and 5 (see Section 4 in the revised manuscript and the added Supplement).

A comparison for WRF-Chem to TES retrievals similar as shown in Figure 7 for MOZART is not possible since the TES retrievals are mostly located outside the WRF-Chem domain.

Third, regarding to the discussions on the underestimation of mid/upper tropospheric plumes in MOZART results, the author need to consider the following questions: 1) Was the MOZART model driven by using meteorological data (i.e. NCEP-GFS) with a resolution as high as 0.7 by 0.7 degree (T170)? Please clarify. 2) Does the fire emission inventory (i.e. FINN ver1) used in the MOZART model have a high temporal resolution? It should be noted that in East and North China there are a lot of biomass burnings activities associated with wheat harvest in June and these activities are mostly concentrated in mid-June, i.e. one to two weeks before ARCTAS/CARB experiment. So if only monthly averaged emission data were used, biomass burning influence from these regions could have been underestimated. Also another important thing is that the plume rise of fire weren't considered in the MOZART model. 3) Besides the

possible underestimation of fire emission and the numerical plume dissipations of Eulerian models, the convection/cloud scheme of MOZART model could also be a factor. In June, especially during the Meiyu Season, there are a lot of convections along the Meiyu fronts, which generally can last for week(s) and probably lift anthropogenic and biomass burning pollutions from the boundary layer.

The referee raises some very important sources of uncertainties in the model simulations and we address the points as follows:

MOZART is an offline model, driven by analyzed meteorology, and takes its horizontal and vertical resolution from the meteorological fields, in this case $0.7^{\circ} \times 0.7^{\circ}$ horizontal resolution and 64 vertical levels between the surface and 10 hPa. This description has been clarified in the manuscript (Section 2.3)

FINN produces daily fire emissions and these were included in MOZART-4. We revised the manuscript and provide more information about the temporal resolution in Section 2.3.

Missing representation of (and also uncertainties in) fire emissions injection height together with model transport errors such as related to convection are additional model errors that have to be considered. We added these to the discussion of model uncertainties in Section 3.1.2.

While MOZART does not include fire plume rise explicitly, the model transport and convection efficiently lofts pollution in the free troposphere as previous studies have shown (e.g. Pfister et al. (2006), Ozone Production from the 2004 North American Boreal Fires, J. Geophys. Res., 111, D24S07, doi:10.1029/2006JD007695). Large scale frontal systems are generally reproduced in the model due to the use of analyzed meteorology.