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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CO source contribution analysis for California during ARCTAS-CARB" *by* G. G. Pfister et al.

Anonymous Referee #2

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Anonymous referee comments to the ACPD paper of Pfister et al, CO source contribution analysis for California during ARCTAS-CARB

This is the modelling study of the long-living pollutant, which originates from several types of sources, including anthropogenic, natural, fire sources, and chemical transformations. In general, the paper is solid and quite well presented study. However, reading it I was missing a few significant items listed below. All-in-all, they sum-up to major revision because some simulations are missing.

General comment I am somewhat puzzled by the stress in the paper. The authors have found a major deficiency of the input data, namely the fire emission being strongly under-estimated. However, no effort was made to deal with it or, at least, to study this problem (the sensitivity run did not help – see below). Instead, lengthy considerations

were presented about the relative contribution of various sources regardless the fact that, if the fire emission is estimated properly, this budget would be different.

Specific comments

Introduction P.3628. Line 25: the opposite is true. Lifetime from weeks to months means that the CO distribution does not resemble its source distributions. About a week is enough for the plume to cross the American continent mixing-up the emission of all sources there. Near the sources, CO can even be considered as a passive tracer, so that its distribution pattern would be a result of competition of emission flux and dilution due to transport. The sentence has to be rewritten or removed.

Introduction The technique of CO tracers should be explained. At present, the paper assumes that reader knows it, which is not very appropriate since the paper is significantly based on this technique.

Section 3.1. P.3634, Line 19->. The good representation in the free troposphere is essentially due to the inflow from the boundaries. Does MOZART show the same quality? Does MOZART have the same low bias closer to the surface? Figure 1. Offset of 100ppbV is not justified and only overshadows the actual fire impact predicted by the model. Also, the MOZART data should be added at least to some panels – similar to Figure 2

Comparison with in-situ data (p.3637). This part turned disappointing. I strongly doubt the possibility of averaging over sites, even after splitting them into two groups. Before doing that someone has to prove that these sites have the same statistical features – at the very least. Since they probably do not, the "mean" time series shown in figure 5 and discussed in the text do not have much value. As an additional confirmation, the correlation coefficient for the mean time series not affected by the fires is zero. Is the model so bad that it cannot get the simple diurnal variation? If the problem persists for individual sites, it has to be discussed and measures taken but I would expect some "poor" and some "good" sites to show up when the analysis is done individually. Figure

5. A technicality: the charts are essentially unreadable, especially the upper panel. They should be widened or split into several panels, the main lines should be made thinner.

Sensitivity simulations. I am greatly surprised by the fire sensitivity run. The authors contradict to themselves. Firstly, throughout the paper the red line is that the fire emission in underestimated – no matter whether near-surface of aloft observations are taken for the model evaluation. Secondly, emission under-estimation means that the fire intensity is under-estimated as well. Thirdly, the plume rise routine gave comparatively reasonable estimation of the injection height, may be, only slightly too high. According to, for example, MISR analysis, the split between the ABL and FT is closer to 80-20 but varies greatly, so that for powerful Californian fires I am not too surprised with 50-50. Fourthly, increase of the estimated fire intensity would mean increase of the injection height. Nevertheless, the authors reduce the injection height and leave intact the emission! What was studied by this run? The problem is even admitted in the paper (p.3643, lines 20-30 and further) but no efforts to correct it were made. I think that the sensitivity study has to be rethought and the fire simulation redone.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3627, 2011.