

Reply to anonymous referee #2

We would like to thank the referee for the thoughtful and insightful comments. We have addressed all of the comments. Our responses are itemized below.

General comments:

The critical issue is that the authors have not presented any quantitative/statistical evaluation of the model performance against observations. All of the comparisons are shown as graphics and the readers is left to puzzle it out whether the “green” line really is a better fit than the “purple” line. This is critical as many of the authors assertions about the model performance and the various emission (anthro, bb) inventories rely on the quality of these fits. This can readily be rectified with a table showing the means, bias and correlations for each model-obs comparison. This is critical and the paper should not be published without it.

Point well taken. We have included statistics.

The second critical point is that Table 1 (and its discussion) is clearly wrong. The author’s model analysis demonstrates poor agreement with the observations. So how then can we believe the modeled results on different sources? That is not good science.

Agreed. Table 1 deleted.

Finally there is some important discussion that’s missing. This has to do with our overall understanding of the BB emission, generally, and GFED v2 specifically. Since one of the co-authors is the lead on GFED, it is certainly reasonable to expect some discussion on issues such as: 1) Why increasing the BC emissions by 50% didn’t help the model performance all that much? 2) Whether this is likely a problem just for BC or is it also important for other compounds? If only BC then it’s important to provide some references to show how well GFEDv2 does for these other compounds. 3) What is the likely cause for the underestimate in emissions?

Point well taken. More discussion added.

Other comments:

Pg 13430, Line 28: On model resolution: I was surprised that nowhere do the authors discuss the impacts of model resolution on their analysis for the mountainous western US. Surely this is an important consideration when discussing model performance and PBL heights.

Indeed. Discussion added.

Pg 13431, line 16: The conversion of hydrophobic to phallic aerosol must be a function of many factors. In a bb plume, O3 may be depleted early on and so oxidative processes may slow. I am surprised the analysis did not examine the influence from this factor.

Indeed. However, it is beyond the scope of the present study. Park et al. [2003] has examined the conversation of BC in GEOS-Chem in more detail.

13432, line 1-5: I am puzzled over the use of 8-day GFED bb emissions. How can you compare daily observations to an inventory that has an 8-day resolution? While this won't change the main findings (that the emissions are significantly under-estimated) it certainly will impact the ability of the model to capture the observed variability. I thought the GFED inventory was available on a daily basis and if so, why wasn't this used? Comments?

Discussion added.

13432, lines 8-9: The inclusion of synoptic variability is not well described and it's unclear what is being corrected for. If there are synoptic variations in fire activity, why won't the MODIS fire counts pick this up?

Clarified.

13432, line 28: Given the 8 day emissions, wouldn't it make more sense to compare means over longer time periods?

See reply above.

13433: These qualitative comparisons are not helpful. Paper must include a table showing mean values, bias and R2 for all of these comparisons.

Statistics now included.

13433, line 15: The specific statement about agreement with the anthropogenic emissions must be supported by a specific site comparison showing the means, bias and R2.

Statistics included.

13434, lines 10-13: Model resolution is important for variability, but means should still agree.

It is inherently problematic to compare, mean or otherwise, the localized IMPROVE observations with GEOS-Chem model results that are representative of a much larger area (~200 x 250 km²)

13343, end of page: The authors should cite some observations here, not just models.

Indeed. References added.

13436, line 4: It's not the fire timing that matters here, but rather the fact that you are using an 8-day emission inventory.

Not really. Discussions added.

13436, line 8: Typo: temperature.

Changed.

13437, line 8: Why 1300 LT? The maximum in PBL height is usually later.

Because of Daylight savings time, it's should be 14:00 LT. NARR data have a temporal resolution of 3 hours. 14:00LT is the best option. Discussion added.

13437, line 16-end: I don't agree that the precipitation is such a strong factor. Precipitation in the western US in summer mostly occurs in a small number of large events. It's not clear how important this will be in the overall BC concentrations.

Agreed. We calculated the ratio of wet deposition to the total deposition (dry + wet). Dry deposition is the dominate factor (~90%) in the WUS during summer. The discussion about the precipitation deleted.

13438, line 20: I cannot see in the figure that the GEOS 5 results are better. Again summary statistics would add to your arguments.

Quantitative analysis included.

13439, line 1-8: The change in precip is confined to a relatively narrow zone of Wyoming and Western Colorado. I don't see the precip factor as important as the PBL factor.

Agreed.

13439, line 15-20: This section has some contradictions. First you say there is a "near linear increase" then a few lines later say "have rather small impacts". Graphically, the increase does not appear to be linear, but I don't understand why it wouldn't be?? Not clear what surface means in this context. All of these are surface sites. Some are elevated. In any case, even at the lowest elevations, the increases DO NOT seem to be "near linear". Need quantitative info to see if this is true or not.

Quantitative analysis included. And also the contributions of biomass burning emissions from different regions included.

13440: This section is confusing. The comparisons between GEOS 4 and GEOS 5 was confusing. Had a hard time following this section.

Revised.

13441, line 7: I don't understand why injecting the emissions above the PBL has so little influence. Certainly this must reduce the surface concentrations???

Discussions added.

13441: This section is clearly in error. Your own results show you cannot reproduce the observations. So Table 1 is clearly wrong. This is not a small point. Figure 4 is quite clear in that the largest contribution for much of the year is due to BB emissions. Yet table 1 would tell us that BB only contributes 10% even at the elevated sites. What's up with that?

Table 1 deleted. Discussions added.

Figure 3: Very hard to see individual lines. Need to clarify if this is indpt 3-day mean or running mean.

Changed. Clarified.

Figure 5: Can't distinguish red and pink.

Different color used.

Figure 7: Is this "Above mean sea level" or "Above model ground level" If AMSL, then the GEOS3 values are below the terrain height on some days! Need some discussion on impacts from model resolution here.

Clarified.

Figure 10: Graph labels for Aug and Sept should be moved to start of month.

Changed.

Figure 12: This figure seems to be thrown in as an afterthought and it's not well described. I recommend to remove it since it detracts from your main points.

Deleted.