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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Top-down estimates of biomass burning emissions of black carbon in the Western United States" *by* Y. H. Mao et al.

Anonymous Referee #1

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REVIEW OF THE DISCUSSION PAPER TITLED "TOP-DOWN ESTIMATES OF BIOMASS BURNING EMISSIONS OF BLACK CARBON IN THE WESTERN UNITED STATES"

GENERAL COMMENTS

This paper reports an inverse modeling study of black carbon (BC) emissions in the western US (WUS) for May-October 2006. The study uses the GEOS-Chem atmospheric chemistry model as the forward model. The a priori BC emission used in the study were an adjusted version of GFEDv2 for biomass burning sources while anthropogenic BC emissions were from Bond et al. (2007). The biomass burning BC emissions were aggregated monthly by three regions (Rocky Mountains (RM), California

and Southwest (CSW), and Pacific Northwest (PNW)). The inversion used BC surface measurements from 69 sites in the Interagency Monitoring of PROtected Visual Environment (IMPROVE) network as observations. The study conducted inversions using coarse (2 deg. x 2.5 deg.) and high (0.5 deg. X 0.667 deg.) spatial resolution grids. The averaging kernel and degrees of freedom for signal (DOFs) indicated the inversion was successful for all four state elements (biomass burning for 3 regions and WUS anthropogenic emissions) for the months July – September at both model resolutions. The a posteriori BC emissions were significantly higher than the a priori and the GFEDv3 BC emissions. The a posteriori emissions for the coarse resolution model were 50% higher than the high resolution model for July – September. The a posteriori anthropogenic BC emissions were reduced relative to the a priori emissions by about 50% for the coarse resolution grid and about 40% for the high resolution grid. The error in the predicted surface BC was significantly reduced for the a posteriori emissions with the improvement being greatest for the high resolution grid. This study provides new and valuable results pertinent to atmospheric chemistry, air quality, and climate. The paper is within the scope of ACP and well suited for publication in ACP.

I have a few reservations regarding the a priori biomass burning BC emissions used this this study. However, I do not believe my concerns, if founded, would necessarily invalidate the inversion modeling approach as employed in the study or the study results or its main conclusions. I suspect the authors can adequately address my concerns regarding the a priori biomass burning BC emissions (which are described in the Specific Comments section). The paper needs to be revised to include additional details regarding the methods. The presentation and discussion of the results also needs to be improved. Both issues are commented on in the Specific Comments section. The paper has numerous, mostly minor, errors with in English usage. Most of these errors have been identified in the Technical Comments section. However, I suggest the authors have the revised manuscript edited for English usage. I recommend this paper for publication in ACP if the authors satisfactorily revise the manuscript.

SPECIFIC COMMENTS

ADJUSTED GFED EMISSIONS

The authors created an adjusted GFED emission dataset and used it as the a priori: P28071, L9-12: "We first improve the spatial distributions and seasonal and interannual variations of the BC emissions from the GFEDv2 using the Moderate Resolution Imaging Spectroradiometer (MODIS) 8-day active fire counts (0.5 ×0.5, available at ftp://fuoco.geog.umd.edu) from a 3 yr period (2005–2007)." However, in the presentation and discussion of results they compare the a posteriori emissions and simulated surface BC concentrations against the standard GFEDv2 simulations, which were not used in the inversion. The authors should be consistent and compare a posteriori results vs. the a priori results, i.e. the adjusted GFED not standard GFED. This would require revision of Figure 8 -12 and the associated text. I have significant reservations regarding the purpose and approach for creating an adjusted GEFD emission dataset. The authors adjusted the GFEDv2 emission dataset (standard GFED) to improve the temporal and spatial accuracy of the emissions and to better represent small fires. The description of how the standard GFED emissions were adjusted is not clear and must be revised (see below). However, if my interpretation of their adjustment method is correct then what they did does not make sense. It is unclear why the authors chose to use CONUS wide MODIS hotspots and GFED emissions over three years to modify western US emissions for the summer of 2006. The authors should have used daily active fire detections and BC emissions for the study area for the year 2006. The magnitude and spatial and temporal distribution of fire activity in the western US can be highly variable from year to year. Contaminating the 2006 data with 2005 and 2007 doesn't make sense. The fire regime in the Midwest, southern plains and southeast is very different from that in the west. In these regions large, widespread wildfires are not an annual occurrence. Also agricultural burning is more important, and is the southeast prescribed fire is dominant source of fire. Further, biomass burning emissions, including GFED, depend not only on burned area, but on fuel loading, combustion complete-

ness, and emissions factors, all of which vary spatially. The approach used is based on the spatial and temporal distribution of fire occurrence, but redistributes emissions without regard to the fuels involved. Their approach will move emissions from agricultural fires to montane forests, emissions from forest fires to sagebrush lands, and so on.

Adjustment Method

P28075, L4-9: The description is not clear. My interpretation is as follows: for each of the 3 regions 1) you summed the GFEDv2 monthly BC emission for the 3 year period (sum of 36 months) providing a single BC emission value, EBC, 2) then for each eight day period, i, and each model grid box, j, you summed the MODIS active fire detections for all 3 years (AFDi,j), and 3) then BC emissions for each 8 day period within each grid box,

EBCi,j were calculated as EBCi,j = ECB * (AFDi,j)/sum(AFDi,j)

Is this correct? I think my interpretation I correct, but the text is unclear. Please revise text to clearly explain what was done.

MODEL RESOLUTION

The study investigated the role model resolution by using a coarse grid (2 deg. X 2.5 deg.) and a nested, high resolution grid (0.5 deg. X 0.667 deg.). Two important findings of this paper are 1) a posteriori emissions are very sensitive to model resolution and 2) BC simulations using the higher resolution model (and a posteriori emissions) produce better agreement with the IMPROVE observations compared with the coarse resolution model/a posterior.

However, in the results and discussion the a posteriori nested simulations of BC (0.5 deg. X 0.667 deg.) are compared with the 2 deg. X 2.5 deg. standard GFEDv2 BC simulations. This is not the proper comparison. The nested a posteriori BC simulations should be compared vs. the a priori nested BC simulations. Because the authors do not compare the high resolution simulated BC with the high resolution a priori BC simulations one does not know how to attribute the improvement with respect to IMPROVE observations. How much of the improvement is due to the inversion and how much is due to improved model resolution? The authors should include the high resolution a priori based BC simulations. For example, Figure 5 should include the a priori 0.5 x 0.667 results. Or better, replace Figure 5 with frequency plots of the BC error (Model – IMPROVE) for all sites for the a priori and a posteriori GFED adjusted at 2x2.5 and the GFED adjusted at 0.5 x 0.667, and include relevant statistics (e.g. mean).

Also, please describe how the authors account for the transport of BC from Canadian fires into the model regions or determine that it was not important?

FIGURES

Some of the figures need to be revised and I suggest replacing some figures with different figures.

The authors should:

1) Include the high resolution (0.5 x 0.667 grid) a priori BC simulations in the figures that compare simulated surface BC vs. IMPROVE observations. As discussed above, the high resolution a posteriori BC simulations need to be compared against the high resolution a priori simulations to properly assess the impact of the inversion.

2) Use the a priori 2x2.5 instead of the standard GFEDv2. Again, to assess the utility of the inversion compare a priori with a posterior.

3) The authors should include frequency plots of the simulated BC error (Model – IMPROVE) using data from all sites e.g. 4 panel with a priori 2 x 2.5, a priori 0.5 x 0.667 , a posteriori 2 x 2.5, and a posteriori 0.5 x 0.667. The figure could focus on the period where the inversion performs best (July – September).

Figures 4 & 5: With the exception of the 1-2 km sites during late August, Figure 5 shows little difference between the standard and adjusted GFED. I suggest replacing

Figure 5 with a frequency plots of the BC error (Model – IMPROVE) for all sites for GFED standard, and GFED adjusted at both resolutions (2x2.5 and 0.5 x 0.667) and include relevant statistics (e.g. mean, median).

Figures 8-12: Overall I find these figures informative, especially the plots of aggregate data Fig 11 and 12. I strongly recommend including error frequency plots as suggested above. These could perhaps replace one of the site specific figures (Figs. 8-10).

Figures 13 & 14: I found the effort required to interpret the Taylor Diagrams (Figs 13 & 14) and associated discussion was not worth the payoff. I believe a simpler presentation (error frequency plots) would be more informative and certainly less cumbersome to the reader.

SECTION 2

The author should include a table listing information (name, id, location, etc.) of the IMPROVE stations used in there study supplementary material.

SECTION 3

P28075, L12 – P28076, L14: The authors should include and discuss summary statics on the error in simulated BC (Model – IMPROVE) for the GFED standard and both the coarse and high resolution GFED adjusted simulations.

SECTION 4

I was wondering about the linearity of GEOS-Chem for BC. What are the GEOS-Chem removal processes for BC? Will they depend on chemistry or other factors related to emissions? See P28070. In Sect. 2, P 28072, L18-27, the GEOS-Chem loss processes for BC are listed – wet deposition and dry deposition. Are there any aerosol chemistry processes or cloud chemistry processes in GEOS-Chem that could have a significant impact on BC removal (e.g. by impacting hygroscopy)? And if, would this make the forward model non-linear? Errors in the a priori BC emissions will almost certainly include associated errors in other biomass burning emissions (VOC, OC, NOx) which could have a non-linear impact on BC removal. Such effects would not be detected by your linearity test that perturbed only BC emissions. Please comment.

P28079, L14-16: Please give some justification or explanation for the choice of 50% uncertainty for anthropogenic BC emissions. What uncertainty does Bond et al. (2004, 2007) estimate for their US or North American BC inventory? Also, please note if the Bond et al. emissions were monthly and if not how they were apportioned over time or explain why temporal apportioning was not needed.

P28079, L17-21: The uncertainty in the biomass BC emissions were simply assigned a range from the abstract of a review paper (Ramanathan and Carmichael, 2008). For a model inversion the uncertainty used for the a priori emissions should be an attempt at providing the best possible representation of the true uncertainty. The inversion should use a properly specified error covariance matrix. I believe one could use van der Werf et al. (2006, 2010) and Giglio et al. (2006, 2010) can be used to provide a reasonable estimate of uncertainty in GFED BC for the a priori emissions used in this study. For example, the uncertainty estimates for GFED fuel loading (woody biomass 22%, herbaceous biomass 44%) and combustion completeness (50%) from Table 6 of van der Werf et al. (2010) should be roughly applicable to GFEDv2. The BC emission factors (EFBC) used in GFEDv2 are from Andreae & Merlet (2001) and have estimated uncertainties of 34% for extra-tropical forest, 38% for savanna & grasslands, and 19% for agricultural residues. Taking the largest uncertainties and assuming 100% uncertainty for burned area a reasonable the uncertainty in GFEDv2 BC emissions, uEBC is: uEBC = sqrt(0.442 + 0.502 + 0.382 + 12) = 1.26, about 125%. Perhaps one could justify uEBC about 200% on the assumption that the aforementioned uncertainties, particularly that for fuel loading, are low. The authors used 300% to 500%, and found 500% to provide the best inversion. I find this puzzling. The averaging kernels and DOFs do indicate a successful inversion. How about the cost function? In a successful inversion the cost function value should be on the order of the number of observations. At P28082, L5-6 the authors note the cost function was reduced by about 40% after the

inversion. What was the value of cost function following the inversion and how does it compare with the number of observations? Does the cost function also indicate a successful inversion? Is it possible that using an uncertainty 2 to 3 times a seemingly reasonable would still provide a successful inversion? Is the system simply insensitive to Sa? Please comment.

P28080, L2-8: The authors use the representation error from Palmer et al. (2003) (5% – 10%). However, the Palmer study measured CO from aircraft over the ocean, with most measurements > 1 km above the ocean surface. I would consider their error estimate to be a lower limit for the representation error of surface measurements sites located in complex terrain. Please justify the use of representation error from Palmer et al. with respect to these comments.

SECTION 5

P28082, L5-6: "The cost functions reduce by about 40% after inversions in those experiments." This sentence is unclear. Please rewrite and clarify the significance of the statement. Also provide the value of the cost function before and after inversions for both the 2x2.5 grid and the 0.5x0.667 grid and compare with the number of observations. The value of the cost function is an indicator of inversion success and should be the same order as the number of observations, if the errors are properly specified (Palmer et al., 2003). P28082, L6-7: "The BC emissions after inversions with different error specifications show similar trend." This is statement unclear. How does it relate to Figure 7. Do you mean that the a posteriori emissions for each state element are similar for the different error specifications? This statement needs to be clarified.

P28082, L 10-15: The paper discusses/shows (Fig 7) the uncertainty of the a posteriori BC emissions. How were the a posteriori uncertainties determined? Please describe in an appropriate place, perhaps Sect 4. Also, please include these uncertainties in Table 2. This would be tremendously useful for other researchers!!

P28085, L6-10: If the authors wish to describe results of model simulations using

FLAMBE emissions they must reference the publication(s) presenting the results or describe the simulations and show the pertinent results. This could be included as a short supplement. Otherwise L6-10 should be removed.

P28086, L12-15): "Model simulated surface BC concentrations averaged for sites in California and the Southwest show slight increase compared with those averaged in the other two regions, which may be due to the fact that most of IMPROVE sites in California and 15 the Southwest are not located at the regions with large fires". A cursory comparison of the IMPROVE site locations and large fire perimeters from the MTBS fire perimeter database (www.mtbs.gov) show this statement is not strictly true and needs to corrected / modified. At least 6 IMPROVE sites in California and Arizona were located near large fires that occurred in 2006. The Joshua Tree NP site (JOSH1) is located 20 km from the approximate center of the 35,000 ha Millard Fire and within 5 km of the 575 Ha Whispering Pine Fire. The San Gorgonio Wilderness site (SAGO1) is < 25km from the approximate center of the 35,000 ha Millard Fire The Trinity site (TRIN1) is located about 30 km east of the 40,500 ha acre Pigeon Fire and 15 km from the 1330 ha Junction Fire. The Lava Beds National Monument site (LABE1) is adjacent to the 1740 ha Big Nasty Fire Indian Gardens site (INGA1) and Hance Camp at Grand Canyon NP (GRAC2) site in Arizona were both located near (within 1 to 5 km) of four fires ranging in size from 400 to 850 ha.

P28087, L5- P28088, L12: The authors need to elaborate on how more efficient ventilation of the lower atmosphere would lead to more accurate simulation of BC surface concentrations. Also, the authors need to discuss if the high resolution simulations using the a priori emissions produce improved results (simulated BC vs. IMPROVE BC) relative to the coarse resolution simulations using the a priori emissions. Comparison of simulated BC would be better presented using a frequency plots of the BC error (Model – IMPROVE) for all sites (e.g. 4 panel with a priori for both grid resolutions, a posteriori 2x2.5, a posteriori 0.5 x 0.667) and discussing these plots and summary statistics. See comments above.

P28088, L4-L11: The text is difficult to follow and needs to be rewritten and should be supplemented, or perhaps replaced as suggested in prior comment.

SECTION 6

P28090, L21-22: "Three a posteriori estimates with different sets of error specifications showed similar monthly emissions, which reflected that our retrievals were reliable." I disagree. I believe the averaging kernels and degrees of freedom for signal are most important in demonstrating that the retrievals were reliable. These results should be reiterated in the conclusion.

P28091, L15-16: "However, FLAMBE emissions might be systematically too high and problematic in its temporal variations." The study has not shown any evidence that FLAMBE BC emissions may be problematic in its temporal variations. This last portion should be removed, e.g. "Comparison with our a posteriori emissions suggests the FLAMBE BC emissions may be systematically high in the WUS."

P28090, L12-20: The authors should describe the improvements in the a posteriori simulated BC relative to the GFED adjusted (coarse & high resolution) simulated BC in terms of aggregate error (model – IMPROVE) statistics, e.g. mean error for all sites.

TECHNICAL

P28070, L6: change "burnings" to "fires"

P28072, L6: delete "that" between "column" and "extending"

P28072, L3-9: It's my understanding that lower levels of GEOS-5 are pure sigma levels. The text describes the lower model layers w.r.t. height above sea-level, many of the listed model levels are below ground level across a good portion of the western US. This needs to be clarified.

P28074, L16: Sentence beginning with "As a result…" needs to be rewritten. For example "As a result, active fire detection methods are probably better at capturing small prescribed fires and agricultural fires."

P28074, L19: Sentence beginning with "There are many…" needs to be rewritten. For example "There are many uncertainties associated with relating fire counts…"

P28076, L11: change "significantly" to "significant"

P28076, L16: change "We here" to "Here we"

P28076, L19: change "sources" to "source"

P28076, L22: change "daily" to "24 h average"

P28077, L2: change "linearization" to "linearize"

P28078, L16: insert "the" between "shows" and "inversion"

P28078, L25-27: The sentence beginning with "We aggregated…" needs to be rewritten.

P28079, L16: "was still not perfect to" change to "did not capture" or "did not closely capture"

P28080, L6: change "exam" to "examining"

P28080, L16: The sentence beginning "The number of pieces…" needs to be rewritten.

P28083, L18: change "control" to "controls"

P28083, L28: insert "the" between "over" and "three"

P28084, L11: change "pixels" to "pixel"

P28084, L20: change "coordinated" to "coorinates"

P28084, L23: Sentence beginning "All emissions…" is irrelevant and should be deleted

P28085, L10 – L16: This last sentence must be is unclear and must be rewritten, probably as two sentences.

P28085, L26: The sentence beginning "Here shown…" needs to be rewritten

P28086, L1: change "statistic" to "statistical"

P28086, L12-15: "Model simulated surface BC concentrations averaged for sites in California and the Southwest show slight increase compared with those averaged in the other two regions, which may be due to the fact that most of IMPROVE sites in California and 15 the Southwest are not located at the regions with large fires". Insert "a" between "show" and "slight", change "are not located at the regions with large fires" to "are not located in regions that experienced large fires"

P28086, L20: insert "relative to those based on the standard GFED emissions" between "enhancements" and "all"

P28086, L29: change "discrepancies" to "discrepancy"

P28087, L3: rewrite sentence beginning "Lack of…" for example "Undetected small fires may be a contributor to the aforementioned discrepancies"

P28087, L11: change "are in" to "is"

P28087, L12-15: Change "As suggested by Wang et al. (2004), higher-resolution model…" to "Wang et al. (2004) suggested that a higher resolution model…"

P28087, L18: change "statistic" to "statistical"

P28086, L24: The sentence beginning "With high-resolution…" needs to be rewritten.

In multiple locations:

Change 'collacted' to 'colocated' or 'co-located'

Change 'selective' to 'selected', e.g. P28075, L27.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28067, 2013.