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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 #2

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This manuscript presents a neat and important idea for evaluating BC emissions from fires in western US. While methodology and data used are reasonable, it's hard to discern "big picture" result from this work, as the reader gets lost in the many details of the work that are presented that are not fitted into a larger improvement in our understanding of BC emissions. The very few places that conclusions are offerred, we read that this work agrees with some previous findings. That alone does not yield any insight. However, the authors don't point out that almost none of those "previous studies" focused on BC, so that way, this work is one of the first ones assessing the effect of increased resolution on inverse constraints of BC emissions. That is important and valuable, and should be highlighted more explicitly. My overall suggestion is to greatly reduce the number of details in description of inversion algorithm and set up, and as

much as possible combine the results into a meaningful conclusion. It's hard to figure out what to do with few % change in this region with this type of emissions, and this many % change in emissions in some other region with this type of set up of GFED. It's not clear what we are learning. While I appreciate the authors' efforts in organizing the manuscript into separate thematic sections, I was getting a bit lost constantly being referred to other sections to read more (see Section 5.2, see Section 5.3). It would be helpful to have discussion and statistics closer to the results and not separate them so much. A big weakness of this study is the lack of independent measurements to verify the result of the inversion. This is not entirely authors' fault, given the paucity of BC data available. Meanwhile, their citation of prevous work makes it seem like a lot of very similar studies were done before. As mentioned above, it would be helpful to clarify how few inversion studies have been done before for BC emissions. l. 11: Heald et al. 2004 is not the best or the most up to date reference for that. Perhaps something from Tami Bond or David Street's work? The Bouding role of BC report could be a good reference too p28070 l. 15 to p28071 l. 5: I think this can be either deleted or replaced with a reference. Rodgers et al. 2000 could be enough, perhaps with some more specific references; for BC it might be Rokjin Park's work and Amir Hakami's adjoint work rather than the CO references here p28072, l. 19 "many updates" to BC simulation in GEOS-Chem: are there references? p28074, l.1-5 The way it's written it sounds like this GFED inventory development was done as part of the study, is that right? Is this what is referred to as "improvement" in the Introduction section? p28074, l. 16 "capturing" should be "capture" p28074-28075, I'm wondering here if I missed if in this study GFED was augmented to account for small fires that GFED is missing currently. Is that correct? and is that improvement based on Randerson et al. work? It would be great to state this more explicitly. I'm also confused about the redistribution of emissions rather than their increase. If GFED is missing the small fires wouldn't that mean that emissions need to be increased and not just redistributed? p2807, l. l11. "significantly" should be "significant"; how do the authors conclude that such small enhancement is significant? p28078, l. 7. this assumption that a posteriori J(x) should be of the same order as the number of observations assumes that the second term in equation 2 is nearly zero or at least an order of magnitude smaller than the number of observations. is that the case here? does it make sense to expect it? does it matter what the final J(x) is as long as it is smaller than a priori J(x)? p28079, l.8. should "exam" be "examine"? p28079, l. 18. uncertainties of 300 and 500% are uncharacteristically large. What is the justification for such uncertainty? I see justification of factor of two. Also, it would be great, if possible, to read a bit of a comment on the implication of this assumption. From what I can tell, this uncertainty is much larger than other studies assume (not that I disagree with what's probably a more realistic uncertainty here). p28079-p28080. It's a shame that there has been no progress in the past decade on how we compute transport error. Again, perhaps a comment justifying the use of this old approach vis-a-vis more recent studies would be great, if possible. p28081, l. 8 Please delete "both" or "two" p.28081, l.25 "comparable" instead of "compatible"? p28082, l. 11-17. To some (large?) extent, I believe those outcomes are a result of the mechanics of the inversion, not necessarily reflecting new insights. Section 5.1.1. I don't see much in a way of stating the absolute amounts of emissions. That would be helpful. Table 2. It would be more helpful to organize the table by source regions rather than a priori/a posterior/etc. This way, the numbers that are being compared are right next to each other. p.28083, l.14 "correspondingly" should be "corresponding" p28084. This description of fire emissions inventories (and their changes?) seems a bit out of place as it comes after the inveresion results. Perhaps this can be moved to where observational data are described? I'm also a bit confused which part in this description indicates current work and which part previous work and/or standard GFED p28085, l. 18-19. "selective" should probably be "a subset" or "select" p28086. l. 13-15. that's not very encouraging to hear that a posteriori BC emissions could be biased to due a non-idea location of the data used.... It seems to undermine the results a bit p28086, l. 24-29. It would be more interesting to hear about insight gained since the authors' last study rather than the % bias reduction which can simply be a result of the inverse model yielding a better fit to data used to drive the inversion. This is followed by an

interesting statement about model resolution. It would great to back it up with some numbers here, since this is very important as models are moving to increasing resolution. It's a shame that the statement about lack of detection of small fires remains just a speculation. It would be great to know more about it. p28087, l. 10-12. While maybe it's not overly surprising that this study finds higher resolution to be a better choice for model-data agreement, the papers cited are not focusing on BC, are not very recent, and it's not clear that they test equally high resolution. This is important and should be highlighted more here an in other instances throughout the manuscript. Not ever reader will know that from the text the way it's written right now. p28088, l. 1-2 I don't understand the connection between the results on the previous page and the explanation that emissions in the nested grid model are "more concentrated". p28089, l10-12. I'm not sure what we're learning here. Earlier, miniscule amount of BC was deemed "significant", here differences are dismissed as "not large", followed by a somewhat nonsequitor speculation. Section 5.3. Very nice quantification of skill improvement. p28091, l.14 should be FLAMBE

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