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.

In this paper the authors attempt to refine BC emissions in western US fire emissions for a 4 month time period through the inversion of IMPROVE BC data. I am not going to reproduce the writing or comments of the previous two reviewers. But I am going bring up some big picture issues which I think calls into question not only this paper, but circular reasoning that is pervasive in the inversion community. Simply put, the authors assimilate BC concentrations from IMPROVE into their source functions (the basis of their inversions) and "verify" that they get a better result in comparison to the very products they assimilated. There is no independent verification to give confidence that their modified source function is actually better outside of their source-model inversion space. Scientifically, there is not too much else to be said. This calls into question any specific applicability of the results as presented. I regret if this review sounds "cranky" but I firmly believe that inversely modelers have gone way off of the reservation as to what they can do and how their results are presented. This is not to suggest that there useful work and findings cannot come from this work or ones like it. However, how such work is framed and verified is critical for proper usage by the community, especially in a high impact factor journal like ACP. If the authors wish to simply describe their system and give a description of incremental progress from their well written 2011 analysis paper (a very fair thing to do), EGU has a journal (Geophysical Model Development) for just that, which should be utilized and used to provide details and sensitivities of the system. Before this paper can be published in ACP, however, a major effort is required to clarify what is actually going on, and the data needs to be cast in a light that is useful to the community. A simple acknowledgement of the problematic nature of such an inversion study (e.g., the end of section 2) is not sufficient. If it is the author's intention to then use the inverted BC source magnitudes as a benchmark for people to use, in the absence of any verification work this paper his incomplete. To make this acceptable will likely require withdrawal and resubmission at a later time. The authors are free to contact me if they want to have side discussions. Be well, Jeffrey Reid (NRL)

Points well taken. See our itemized responses below.

Major comments

1) To be absolutely clear, the authors are inverting IMRROVE samples that are collected in 1 day in 3, over a 6 month period at isolated surface sites. From a signal processing perspective, the authors need to demonstrate that 69 IMPROVE sites can resolve a 2 dimensional emissions field. By setting the background error in emissions to 300-500% and the observations to 30-50%, the investigators are simply letting the data define the magnitude of the source function. By then stating that their posterior results are better than their prior against the very same data they used in the inversion, what they have shown thus far is that the model is at least self-consistent. But this is the beginning of the problem, not something that can be used. Because there is no external verification of BC or even the amount of land that was burned, it is impossible to determine if the derived corrections are physical. Certainly whether or not the GFED source function should be modified cannot be discerned from this paper. So the authors need to prove, through

some form of independent control (say modification for the source function and application to different years, or even a day by day comparison of area burned based on forest service estimates) that we would expect the results to be representative. Fortunately, there is very good documentation of wildfire burned area for the study region.

Points are well taken. One important aspect of inversions like the one presented in this study is precisely to examine the consistency of bottom-up emission estimates with observations using a forward model, which represents our best understanding of the various processes controlling the atmospheric concentrations of the species in question. That is not to suggest that the a posterior estimates should be used to replace the bottom-up estimates, considering (1) the scarcity of observations available for the inversions and (2) the (sometimes large) model errors. Rectifying the underlying deficiencies within the bottom-up estimates (e.g., burned area) is not the primary focus of and thus beyond the scope of the present study. We have set aside a subset of the IMPROVE sites for evaluating the inversion results.

2) Regarding the issue of scale, as we warned in the Reid et al., JSTARS 2009 paper, the danger with tuning source functions is that they are not necessarily real, but rather what gives the model the best answer against a predetermined norm. All model errors then are folded into the source term as it is the biggest knob. Indeed, in the author's 2011 paper, they note sensitivity to the PBL parameterization in the model. In particular, we noted the scale issue as problematic (just as the present authors have found) as plumes are of fine scale. By using a coarse 2.5 degree grid, you are assuming uniformity in pixel. Assuming a simple x2 Niquist Frequency, at 2.5 degrees you can only resolve 5 degrees features, large than the state of Washington, Oregon or even Montana. Compare that to any satellite RGB image for any given day. The meridional scale length for most fire is less than 100 km for large events. At 2.5 degrees game over.

The inversions presented in this study, like abundant previous inversion studies, are not meant to constrain any individual fire events, given the nature of the model resolution, as the referee pointed out. That being said, we would argue that 2x2.5 model resolution is still adequate for constraining biomass burning emissions on regional scales (hence the aggregated state vectors used in the inversions).

3) A third issue is that the paper is inherently associated with surfaced concentrations, whereas BC emissions relate to the column. There is no discussion of how the model behaves in the vertical nor even in say AOT space over the period. For, if they change the BC emission but not the total emission, they will have changed the aerosol optical properties to an unphysical value. Functionally in observation space, it is difficult to separate BC from the rest of the mass. So by changing BC by integer factors, to keep the BC mass concentration into the physical range, what have you done to your AOTs at the sites? Similarly, as the model inverts sources based on the sites, what about AOTs away from the sites? The issue of vertical profile is a vexing one for the community. Given the mountainous terrain of the intermountain west, I would expect the numerics of vertical diffusion to be difficult. Again, their 2011 paper discussed the sensitivity to PBL. Such

parameters as dry deposition velocity (even for fine mode smoke) are enormous sources of uncertainty in the surface layers of models. What does say a 50% change in dry deposition velocity do to your surface layer concentration?

Points well taken. We wholeheartedly agree with the referee that measurements of vertical profiles of BC would be hugely helpful. Unfortunately, the scarcity of such measurements (at the time of this study) limited us to the use of IMPROVE data, the best set of surface measurements available to us.

4) Given 3 some measure of proof and discussion has to be made that the IMROVE data themselves are representative of column BC. Indeed, major events often have sequestered plumes above the PBL. These are unaccounted for in the presented analysis. I have significant doubts on whether this data which often has localized representativeness can be used in such a straightforward inversion process.

Whether IMPROVE data are representative of column BC or not is really beside the point. We are fully aware of the limitations of the surface measurements used for the inversions in this study. See our comments above. We also like to note that IMPROVE observations have been used for the inversion studies using GEOS-Chem, e.g. Henze et al. (2009), which we now cite in the text.

5) I was somewhat bemused by the application of our FLAMBE data into the analysis. What was showed was that a priori, FLAMBE actually beats GFED (FLAMBE is closer to posterior source magnitude than GFED). But it is later stated that FLAMBE has some temporal issues (not shown). This temporal issue then becomes a conclusion, with nothing actually presented in the paper (very poor form). The Reid et al. 2009 paper also reported biases for the study region. If the authors actually read the 2009 paper and in particular the discussion, they would know our opinion on the problems of interpreting source functions as truth- including a discussion of the likelihood of scale issues (again, comment #3). Bottom line is they are welcome to use the FLAMBE data, but it needs to be a full and fair presentation of the findings. While I believe that some of the problems with this paper are common to many top-down inversion studies, it does not mean that I think such work unimportant. I also respect the amount of effort that likely went into this work. However, I think inversion modelers have not been rigorous enough with issues of data quality and representativeness, which are extremely different with short-lived species such as aerosols compared with longer-lived species like CO2. There is real signal processing work to be done, but it is laborious. Perhaps the authors should see this as an opportunity. Maybe by continuing on the scale issues, and adding signal processing components, they can start to bound the problem.

Points are very well taken. We have revised our discussions accordingly to take these comments into account.

References:

Henze, D. K., Seinfeld, J. H., and Shindell, D. T.: Inverse modeling and mapping US air quality influences of inorganic PM2.5 precursor emissions using the adjoint of GEOS-Chem, Atmos. Chem. Phys., 9, 5877–5903, doi:10.5194/acp-9-5877-2009, 2009.