We would like to thank anonymous referee #1 for their helpful comments on this work. Below, original comments are in italics and our responses are in bold.

General comments:

1. The MEGANv2.1 biogenic CH3OH flux used in this study is found to be by 35% lower than the MEGANv2.1 inventory by Stavrakou et al. (2011), and by Guenther et al. (2012) (105 Tg/yr and 100 Tg/yr, respectively). The authors propose that the differences are due to different LAI and meteorology, however, Stavrakou et al. and this study use the same LAI database. Does this mean that the differences between the two estimates are due to meteorology? To clarify this, a global map of the MEGANv2.1 source is needed here to allow comparisons with published work. This is also necessary in order to facilitate comparison of the top-down results with those of Stavrakou et al.

We have added maps of the biogenic methanol emissions for DJF and JJA to the supplemental material, and have also updated our terrestrial emission map in Fig. 2 to include seasonally-averaged emissions for DJF and JJA. However, we note that it is difficult to directly compare the a priori emissions to those in Stavrakou et al. (2011)—this work does not use the MOHYCAN canopy model, and also includes different leaf age activity factors than those in the standard MEGANv2.1 algorithm. Also, we are using a climatological LAI product from MODIS, which may differ from what is being used in Stavrakou et al. (2011). The reason(s) for the differences between the global estimates is not entirely clear, but we note that differences in global isoprene flux estimates on the order of 30% were attributed to differences in driving variables (PFT, LAI, and meteorology) in Guenther et al. (2012).

2. A different inversion approach is used for tropical and extratropical regions. In the first case a seasonal inversion is performed, whereas in the latter, the seasonality is obtained from an earlier publication (Wells et al. 2012), so an annual inversion is conducted. The authors should clarify the reasons for this choice, especially since the seasonality in Wells et al. (2012) was derived from IASI measurements. It would be interesting to know if TES measurements would lead to a similar seasonality as in Wells et al. Please consider providing results using a uniform treatment for tropical and extratropical regions.

One of the primary reasons we chose to do an annual inversion in the extratropics is that data coverage from TES is not as good in these regions, particularly in the winter months. We have added some text in Section 4 to clarify this. In Wells et al. (2012) we found consistent seasonality between TES and IASI data in the northern extratropics, but chose to use IASI to derive the seasonality of emissions given the much better data coverage it provides.

3. In the pseudo-observation test, it would be insightful to add noise to the pseudo-observations in order to account for the observational error.

In the pseudo observation inversion we use the native TES noise error as the observational error covariance matrix, but did not add measurement noise to the pseudo-observations themselves. We have added some text to Section 5 to clarify this caveat.

4. An objective criterion should be used to test the convergence of the iterations. Please provide the final value of the gradient of the cost function. 26 iterations might be enough in the standard case ($\gamma = 0.5$), but I suspect that many more iterations are needed when γ is decreased to low values.
For all tests performed, the cost function is changing by less than 0.5% over the last several iterations. We now mention this in the text as our criteria for convergence.

5. Figure 7 indicates that the emission updates in tropical regions, except Southeast Asia, are extremely small, in spite of persistent underestimations. An annual inversion would have possibly done a better job in reducing this bias. The fact that the regional emissions in tropical areas show very little changes in the γ tests is very puzzling. The case $\gamma = 0$ would have been expected to eliminate those biases. This might be related to the previous comment on the number of iterations.
Prior to implementing a seasonal inversion in our analysis, we performed an annual inversion everywhere and found that we could not reproduce the seasonality of emissions in the tropics, particularly the large peak in emissions over Brazil in SON. The seasonal optimization does better at reproducing these features, but there is still a low bias relative to TES. It is not clear why the test with regularization parameter = 0 did not do more to eliminate this bias, but the cost function does appear to have converged in a manner similar to the other tests performed.

6. Is the AltOH test supposed to represent the true model uncertainty on OH? Its usefulness is clearly limited. But additional tests that could be interesting are the use of a lower deposition velocity, or reduced ocean source/sink of methanol.
While it is difficult to quantify the full uncertainty in model OH, these tests get at two key aspects: 1) uncertainty in global OH associated with errors in meteorology, and 2) uncertainty in OH over high-isoprene, low-NOx regions. We thank the reviewer for the suggestion to do additional sensitivity tests—we have performed additional tests on the deposition velocity and the ocean source/sink. Details are provided in the text. We note that the deposition velocity test resulted in generally less than 5% change in optimized emissions, as did the ocean tests outside of the tropics. Within the tropics, assumptions related to the ocean flux lead to a larger uncertainty (up to 15%).

7. p.21901, l.20-23 : FTIR measurements at Kitt Peak are available and could possibly confirm the need for larger emissions over the Western US in the summer.
Thanks very much for the suggestion. In Wells et al. (2012) we compared to surface observations at Blodgett Forest Research Station in California and found a significant model underestimate there.

8. p.21902, l.10 : It should be made more clear that the biogenic methanol emission underestimation refers to the specific inventory used in this study (which differs from previous implementations of MEGANv2.1, cf. point 1).
This a good point. We have added some text to clarify that the underestimation is associate with our implementation of MEGANv2.1.

9. In Fig.6, note that the regions with the lowest a posteriori errors are biomass burning hot spots in GFED3 database.
Thank you for the observation. This definitely seems true in the tropics, and could corroborate our conclusion that biomass burning is a key contributor to atmospheric methanol over Central Africa.

Specific comments:

1. p. 21892, l.13 : The scale factors are defined relative to what? Clarify also Figs. S1, S2 captions.
The emission scaling factors are defined relative to an a priori value of 1.0. We have added some text to clarify this here and in the captions of Figs. S1 and S2.

2. The color code of Figs. S1, S2, and 5 is inappropriate. Please add more intervals between 0.25 and 2.
Thank you for the suggestion. We have added more intervals on the color scale between 0 and 2, and removed intervals above 4. This helps to see some of the finer scale structure of the areas with downward emissions revisions.

3. Bousserez et al. paper : please provide the names of all authors
This paper has now been submitted and we have updated the citation.

4. The discussion of the results could be more quantitative (Section 6).
Thank you for the suggestion. We have added some information about the relative model- measurement bias in the text to make the discussion more quantitative in this section.