We would like to thank anonymous reviewer #2 for their helpful comments on this work. Below, original comments are in italics, and our responses are in bold.

1. Looking at Figure 1 it is clear that the signal of methanol is of the same size as that of other remaining residuals. The question is what variables contribute to that signal, and whether there could be any leakage of seasonal variation having to do with those factors into the retrieval of methanol. I imagine it would be easy to test the seasonal cycle at sufficient distance of continental sources to exclude the possibility of any external factors playing a significant role. There is little discussion about the filtering of data, except for a DOF criterion. For example, some procedure must be used to avoid influences of clouds, etc. Some further explanation is necessary.

We examined the IASI and TES data in oceanic regions far from sources and find no seasonality in the TES retrievals and only a very weak (~0.25 x 10^{16} molec cm^{-2}) seasonality in the IASI retrievals. The latter is minor compared to the seasonal cycle observed in IASI data over the northern midlatitude continental regions (~2 x 10^{16} molec cm^{-2}). Also, we note that the observed IASI and TES seasonality over midlatitudes are in phase, and consistent with the seasonality observed by Hu et al. (2011). As such, we do not expect significant leakage of seasonal variation due to interfering species such as ozone. The TES retrieval is performed after water vapor, temperature and ozone is retrieved, and a cloud screening criteria is also applied for TES (cloud optical depth < 1.0) and IASI (<2% cloud coverage per pixel). We now note these details in the text.

2. The description of IASI is very brief about the use of the averaging kernel to sample the model. It suggests that a procedure is followed that is similar to that used for TES, which received much more attention. If this is so it should be mentioned explicitly. Otherwise a description is needed at a comparable level of detail.

The application of the IASI averaging kernel and a priori is done in the same way as it is done for TES. The only difference is that there is an additional step to convert the sampled GEOS-Chem profile to a total column value for comparison to IASI. We now describe this explicitly in the text.

3. Looking at Figure 4 it is unclear why the level of methanol is lower for TES than IASI. According to the text the averaging kernel of TES peaks closer to the surface, which means closer to the sources. Therefore I would have expected higher methanol concentrations.

The TES and IASI data in Figure 4 cannot be compared directly because they have different units. IASI provides a total column value (10^{16} molecules cm^{-2}, corresponding to the left-hand axis in the figure), whereas the TES data are in units of Representative Volume Mixing Ratio (ppb, corresponding to the right-hand axis of the figure). The latter is a measure of the methanol concentration at the level of peak sensitivity of the retrieval. We have added a mention of units in the discussion of Figure 4 to clarify this.

4. Discrepancies between the model and the measurements are explained exclusively in terms of biospheric emissions, whereas several other factors influence atmospheric

concentrations such as chemistry and transport. It is unclear how realistic photochemistry is represented in the model. For example, CTM's have difficulty maintaining sufficient oxidizing capacity under conditions of low NOx and high VOC emissions. Such factors require further attention.

While absolute discrepancies between the model and measurements may be influenced by other factors in addition to biogenic emissions of methanol, we are focused here on the seasonal cycle, which is dominated by the biogenic source. We tested this by running a simulation with a different version of the modeled OH fields, and found negligible difference in the seasonality of methanol concentrations. Likewise, a sensitivity run using different meteorological fields (GEOS-4 versus GEOS-5 for 2006) showed no appreciable difference in the simulated seasonal cycle. We have added a mention of this to the text. Issues with OH titration in the model manifest mainly over tropical regions with very high isoprene emissions and low NO_x, whereas we focus here on Northern midlatitude regions.

5. It is unclear what seasonality is assumed for remaining emissions. It is known, for example, that fossil fuel emissions in the extra tropics show a distinct seasonal cycle. It is clear that the biosphere dominates in summer. However, this study focuses on the early spring when the seasonal signature of anthropogenic and natural emissions may be comparable.

There is very little seasonal variability embedded in the modeled anthropogenic methanol emissions. As such, it is possible that a seasonal bias in anthropogenic emissions exists. However, anthropogenic emissions are a small fraction of the total methanol source (~3% on a global scale), and hence are not a tenable explanation for the observed model-measurement discrepancy in the seasonal cycle, which as we see is just as pronounced over remote areas (e.g., Siberia) as it is over populated areas (e.g., Europe, E. US).

6. Page 3958, Line 28: Some further explanation is needed about the optimization procedure.

The optimized parameters were originally chosen manually based on a visual representation of their goodness of fit and the tendency for methanol observations to decrease as a function of leaf age. However, we find an improved fit (see response to comment #8) using a constrained multivariate linear regression in which the parameters are not allowed to be reduced by more than 90% of their original value. We use this approach in the revised version, and describe it explicitly in the text.

7. Page 3959, Line 11: What motivates the choice of LAI = 2?

The choice of LAI = 2 was made because it gives good closure with the observations, while not being so high as to be unrealistic for expanding canopies (i.e. using LAI = 5 also fits the observations, but would only be applicable in mature, fully-leafed canopies). However, we have now lowered this value to LAI = 1.2 (γ_{LAI} = 0.5) based on new results from the linear regression procedure (see response to comment #6). We now note our reasoning for this in the text.

8. Figs 8, 9, 10: More quantitative information is needed about the agreement between measurements and model simulations (for example in terms of correlation and rms difference) to support claims in the text that the agreement has significantly improved after optimization.

We agree that the inclusion of more quantitative statistics strengthens this paper's conclusions. We now include a table in the supplementary material that includes correlation coefficients and RMS differences with respect to the IASI, TES, and ground station data before and after optimization.

9. Page 3961, Line 26: the meaning of "higher slope" will be unclear to all those readers who will only read the abstract and the conclusions section.

Thank you for noting this. We have changed the word "slope" to "concentrations" to make this statement more general.