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

1. Page 3958-59: The authors do not give any details about how the sensitivities shown in Figure 6 are used to determine the optimized emission factors. More explanation on the optimization method are required here.

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 #4) 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.

2. Page 3959, line 9: Additional sources of methanol are invoked, leaf buds, soil emissions, and snow melt, to explain the model/data discrepancy during the canopy expansion period. Is there any concrete evidence that soils and snow melt could be at the origin of methanol emissions?

Lappalainen et al (2009) found high methanol concentrations in a boreal forest site at the end of the snowmelt period, and postulated that it could be due to release of methanol accumulated in or below the snow pack. Also, Schade and Custer (2004) reported methanol emissions from agricultural soils in Germany. We now cite these studies as potential evidence of methanol emissions from snow melt and soil. Given that the model-measurement discrepancy is apparent throughout the midlatitude regions, it seems unlikely that snow melt or soil are the major cause, though they could be contributing factors.

3. Page 3959, line 14: A main result of the paper is the derivation of optimized values for the age emission factors in mid-latitude forests. However, a quantification of the errors associated with these parameters is completely missing from the manuscript. A thorough estimation of the uncertainty on the proposed values should be addressed in the revised manuscript.

These age parameters were derived using constrained empirical fits to observations. Because of the constraint applied as part of the regression (parameters not permitted to decrease by more than 90%), we feel that the most appropriate and objective measure of their overall uncertainty is the quality of the resulting fit of the modeled seasonal cycle to the observations. We now include information in the supplementary material about the correlation and RMS difference of the modeled seasonal cycle with respect to observations before and after optimization.

4. Regarding the comparisons shown in Fig. 10, the absolute model and in-situ values should be also plotted, or at least the mean year-round, and seasonal bias for each of these should be provided. Clearly, here and elsewhere in the paper, the calculated correlation coefficients and biases would be welcome in order to strengthen the conclusions.

We agree that the inclusion of more quantitative statistics strengthens this paper's conclusions. We now include a table in the supplementary information that includes

model correlation coefficients and RMS differences with respect to the IASI, TES, and ground station data before and after optimization.

5. Section 3: Methanol sources other than biogenic are discussed very shortly. The global annual budget by emission source should be given, as well as the contribution of each source in the predefined five regions.

We have now added a table (Table 3) that includes the methanol budget broken down by sources globally and for the five midlatitude regions.