We thank the referee#1 for his/her positive review of our submitted manuscript and respond to the specific points raised.
Since performing the simulations presented in the ACPD manuscript, modeling errors were found in both the vertical distribution of lightning NO_x and the also in the declaration of coefficients used in the deposition parameterization. Examining the resulting differences introduced shows that increases in the chemical production term occur increasing the resident mixing ratios of CH_3ONO_2 by a few ppt at global scale, especially in the Northern Hemisphere and the tropical column. Therefore, the final version of the manuscript will use these improved simulations although the magnitude of differences is rather small.

Missing sources of methyl nitrate. The authors come to the conclusion that there are missing sources of methyl nitrate. Flocke et al. (1998a) and Archibald et al. (2007), have shown that the reaction: $CH_3O + NO_2 \rightarrow CH_3ONO_2$ can be a significant source of methyl nitrate under conditions of high NO_2 (the polluted boundary layer and the lower stratosphere). Have the authors considered this source?

We do touch on this when we mention the possible formation associated with biomass burning plumes as stated in Simpson et al. (2002). There a similar production mechanism was invoked. However, in that we use a rather coarse horizontal resolution for this study means that NO_x hot spots are lost due to homogenization over a large area, resulting in the $CH_3O + O_2$ reaction dominating. We agree that performing simulation at a 1° x 1° horizontal resolution or using a regional model could improve on this and provide an effective emission term for global models. Flocke et al. (1998) state that for a NO_2 mixing ratio of 1ppb, 1 molecule of CH_3ONO_2 will be formed for every 2.0e4 CH_3O molecules consumed. Obviously under urban conditions this would become more important but in our simulations we expect that the total contribution from such a source to be of the order of a few Gg/yr and not a dominant global source term. Therefore we feel that the comparisons presented here would only be marginally altered by introducing such a source.

In response to this point we do now add the following text to the introduction: "The formation of CH_3ONO_2 from the sequestration of NO_2 by CH_3O is also a possible source, but not considered to be significant at global scale because of the high NO_2 mixing ratios which are necessary for the reaction to be significant (Flock et al, 1998)".

20112 – line 17. I suggest you remove the "improves when" and add "upon" (**changed**)
20116 – line 24. I think the sentence may need a bit of re-working/tweaking. (**changed**)
20117 – line 6. The "on" at the end of the line should be "in".
20120 – line 14. Where is the reference for Flocke et al. 2008?! This is something that should have been picked up before the paper was circulated for review (if not by the

authors than by the publishers). (**Corrected**)
20122 – line 17. I suggest you define "hly" or replace with "hourly". (**changed**)
20128 – line 9. Correct "potolysic". (**Corrected**)
20129 – line 24. Could the reason for the steepness of the gradient be linked to the fact that the tropical middle troposphere is the region that dominates the loss of methyl nitrate (i.e. greatest photolysis flux)?

Table 3 shows that the dominant chemical loss route is via photolysis when integrated throughout the tropospheric column. There is a positive gradient in the magnitude of the photolysis rate with respect to height, associated with a higher flux of UV radiation and the absorption characteristics of CH_3ONO_2. However, the concave vertical profile of CH_3ONO_2 shown for the latitudinal region 10-20°N, where the photolysis rate is only fractionally lower, shows that transport is also important in determining the shape of the vertical profile.

20130 – line 4. Correct the double "the". (Corrected)
20130 – line 18. It is stated that the value of the branching ratio used for R10 is from Flocke et al., 1998a and takes the value of 4.5E-3 i.e. 0.45% (greater than the lower limit from Butska). Can the authors please clarify (i) the value of the branching ratio used in the FLIGHT scenario (ii) its origin. As it stands I am unclear on both. For example, in Flocke et al. (1998a) they determine a series of branching ratios of 5-10E- 5 for stratospheric conditions and 1.5-3E-4 for tropospheric conditions.

We thank the referee for bringing this to our attention. This was a typo error in the manuscript and we adopt a branching ratio of 0.0025%, which is in the middle of the range of 1.5-3x10^{-4} determined in Flocke et al. (1998) for tropospheric conditions.

20130 – line 25. Correct "thr". (Corrected)
20132 – line 3. Correct the hanging comma.
20132 – line 28. Correct "Hpa". (Corrected)
20140 – line 32. Correct "prodcution". (Corrected)
Figure 2. Superscript needed for text describing color scale.
Figure 4. I would suggest looking into splitting the y axis so that for the instances where the model significantly overestimates methyl nitrate, the observations can be seen (else what's the point of plotting them?). Or I would suggest that you scale the model fields (or observations) to give the same effect.

We now scale up the measurements such that all sites show a visible comparison.

What about also looking at the diurnal cycle in the observed and modeled methyl nitrate? How does that differ? By the looks of it, the HIGHBR scenario shows a very pronounced diurnal cycle that is not seen in the observations. This is an important further line of evidence to reject the use of the 1% branching ratio.

Given the rather long global tropospheric lifetime of CH_3ONO_2 of 31 days we feel that looking at the diurnal variability in mixing ratios of CH_3ONO_2 would not really add much to the analysis and that the evidence for the 1% branching ratio being too high is already fairly robust as is. As requested by referee #2 we now include a figure of the annual mean surface distribution of CH_3ONO_2 for some of the other sensitivity simulations, which provides further weight to our conclusions.

Figure 6. The figure caption states that the dark blue data represents the results from P_T_pt03, whilst the figure legend suggest the dark blue data are from LOWBR. Please correct. (**Corrected**)
Figure 7. Correct the figure caption (i.e. is EMISS or EMISSDD data shown? In any case the data plotted is not orange). (**Corrected**)
Table 2: The text describing the FLIGHT simulation is misleading/wrong. In the body text in section 2.2 FLIGHT is referred to as being based on EMISSPT but having a BR of 0.045%, whereas in Table 2 FLIGHT is referred to as being based on LOWBR. Please correct. Also, I think it would help the reader if you add a column or a reference to the total emission flux going into the model for each scenario in Table 2. (**Clarified**)