We'd like the thank Referee 2 for their helpful comments, which are found in italics below. Our responses to each point are provided inline and the manuscript has been revised accordingly.

Specific comments:
1) page 3866: I don't really understand if the background was measured for 1 minute or the catalyst heated for 1 minute. I assume the measurement was 1 minute. Please clarify.
The original sentence describing how zeros were carried out was poorly worded and has been revised:
"Zeros were carried out periodically in-flight by diverting the sample air stream for a period of 1-2 minutes through a custom-built stainless steel tube filled with a platinum-coated quartz wool catalyst (Shimadzu) continuously heated to 400 ºC."

2) Page 3867 line 26: Are the grey bars for the detection limits those for 1 second measurements?
Yes, these represent detection limits for one second measurements. The dwell time on the m/z used for the species reported in this paper was usually 1 sec, and the data have not been further averaged in this analysis. This detail has been added to the manuscript.

3) Page 3868: I appreciate the clear acknowledgement of the measurement problems and the good explanation of the detection limits of the various VOCs, but I am wondering what the reason was for the high backgrounds on for example toluene. Did other masses have high backgrounds as well?
As mentioned in the response to Referee 1, this particular instrument has chronically had high background counts at m/z 93 and m/z 107. This did not appear to be an issue for the other m/z we monitored. The relatively high detection limit for monoterpenes resulted primarily from a weak response at m/z 137.

It seems to me that the catalyst was not working quite right, which can be seen in the high background of mass 45.
For the species described in the paper, the signals produced when directing ambient air through the catalyst were indistinguishable from the signals produced when directing the output of a zero air cylinder to the instrument. Thus we are confident in the backgrounds used for acetone, acetonitrile, benzene, isoprene, and the sum of methacrolein and methyl vinyl ketone.
In the case of m/z 45, the signal measured through the catalyst when the aircraft was above the planetary boundary layer was indistinguishable from zero air, but when the aircraft sampled in the planetary boundary layer, the signal through the heated catalyst increased dramatically. This phenomenon also occurred for measurements at m/z 81, which we had hoped to use as a more sensitive signal for monoterpenes. Our interpretation is that reactive species exist in the planetary boundary layer, which decompose in the heated catalyst to produce signals at m/z 45 and 81. As a result of this, we do not have reliable backgrounds at these m/z and did not attempt to convert them to mixing ratios.

It also seems that the instrument noise was very different for different flights (see for example benzene in Figure 6).
There was some flight-to-flight variability in the noise at some m/z. The apparent difference between flights B228/B229 and B231 in Figure 6 is probably mostly a result of different

frequencies of data reporting. Benzene was only measured for one second out of a 20 second cycle time for flights B228/B229, whereas on flight B231, the cycle lasted only 11 seconds. When we look at the normalized background counts at m/z 79 on these flights, their standard deviations are very similar.

So overall there were some measurement problems, but it seems to me that the authors have carefully evaluated their dataset and used only data they have confidence in. I would still like to see more discussion on what the reasons for the instrumental problems and the varying performance of the PTR-MS were. How did the authors decide which measurements to use and what the possible uncertainties are that are caused by the problems with the catalyst. Also any explanation for the strange behavior of methanol?

As described above and in Section 2, the main criterion for which measurements to use was the reproducibility of the zeros:
"For the species discussed in this paper, there was very little variability between zeros through time or as a function of altitude."
The solution to the issue with the methanol signal is not obvious. Because the absolute humidity varied with altitude, the intensities of the O_2+ and $H_3O^+H_2O$ also had large altitude dependences. The former would lead to a changing offset, or background, whereas the latter might lead to differences in sensitivity. Correcting for the natural isotopic abundance of O_2^+ was not sufficient to remove the altitude variability of the zeros. If additional signal from $^{16}O^{16}O$ was somehow contributing to the counts measured at m/z=33, then it's possible that a larger correction should be applied. Because the catalytic scrubber was downstream of nearly all of the sampling line, it seems unlikely that a humidity-dependent adsorption-desorption process on the sampling lines can be blamed for variability in backgrounds. Because the data processing was much less straightforward for methanol, we chose to focus only on the chemical species discussed in the paper.

4) Figure 2 shows nicely that different VOCs have very different vertical and horizontal distributions. While the vertical distribution is nicely explained by the case studies the latitude profile should be better illustrated. The minimum would be to at least show the different source regions in Figure 2, but I would suggest to plot the flight tracks in the boundary layer color coded with the mixing ratios on a map that also show the cities, desert and forest.

We have provided this information in the form of a table indicating the average boundary layer mixing ratios over the ocean, forest, desert, and urban areas and added an additional paragraph at the end of Section 3.
"Table 1 provides the average and standard deviations of the mixing ratios of each of the VOC, and additionally CO and O_3 in surface air over different surface types. For this analysis, we have considered surface air to be anything below a GPS altitude of 700 m. The data are broken up into three regional types: Sahel region, mosaic forest, and ocean; and also into two urban areas: Lagos and Niamey. The conurbation of Lagos was estimated to have approximately 17 million residents in 2006, whereas the population of Niamey at that time was slightly under 1 million. South of 10 °N, the surface flow tended to be southwesterly, characteristic of the West African Monsoon, whereas the surface flow became westerly to northwesterly at higher latitudes. Acetone mixing ratios were similar over the Sahel and mosaic forest, but were much lower over

the ocean (described further in Section 4.4). There are enhancements near the urban areas, especially considering that the source air for Lagos is likely from the marine boundary layer, thus initial levels would be quite low. There were no statistically significant differences between acetonitrile mixing ratios in any of the source regions in Table 1. Benzene is lowest over the Sahel, then the ocean, with higher values over the mosaic forest. It is likely that benzene emissions from the coastal cities are advected over the forested region contributing to higher values there. The biogenic VOC, isoprene and MVK+MACR, are approximately zero over the Sahel and the ocean, with the highest values over the forest and also enhancements over the urban areas. Northwesterly flow appeared to deliver air with low CO and high O_3, whereas the monsoon flow brought air with higher CO and lower O_3, the latter of which was further depleted by deposition to the forest. The urban areas had enhanced levels of CO, but very little change in the O_3. Further descriptions of the geophysical distributions of the trace gases and aerosol can be found in Reeves et al., (2010)."

5) Page 3870 line 19: Please explain for people who are not so familiar with satellite measurements, what the MSG satellite is and why you show the IR brightness temperature.
The sentence has been edited to read:
"Figure 3 shows the Meteosat Second Generation (MSG) images with IR brightness temperature indicating the position and cloudtop altitude of large mesoscale convective system on (top) 20:00 UTC on Aug 14, 2006 and (bottom) 06:00 UTC on Aug 15, 2006."

6) Page 3871 line2-5: Why should the air in the early morning chemically be similar to the air lofted the previous evening. In the altitude profile CO and acetone are higher at 10 km than at the surface, which shows that the mixing ratios must have been higher the previous evening, when the air was lofted.
In Figure 4, the normalized ratios for acetone and CO at the highest altitude are within one standard deviation of one, and hence aren't significantly different than the boundary layer air. While it was not possible to design the flight to specifically sample the exact airmass both before and after it was convectively lofted, we are confident that the mixing ratios sampled during the lowest level leg are representative of the composition of the air before it was lofted.
This section has been edited to read: "Because this low level run was carried out early in the morning (08:00 - 08:40), the chemical composition likely reflects that of the residual layer. In the hours since the passage of the MCS, this layer would not have been subject to photochemistry and would have been dynamically isolated from subsequent emission from and deposition to the surface. Hence we assume that it should be chemically similar to the boundary layer air lofted during the storm passage the previous evening, and we use it to characterize the initial chemical composition of the lofted airmass."

7) Figure 4: The standard deviation for benzene and acetone are missing.
This information has been added

8) *Page 3871: Ozone is discussed in detail, but not shown in the Figures. Please add ozone to Figure 4 and Figure 5. The authors explain correctly that ozone is often low in layers observed aloft, because ozone probably was lower at the time and place where the airmass was lifted than in the free troposphere. It is therefore difficult to determine the actual ozone production in the plumes. What were typical ozone values measured during the boundary layer portions of the*

flight, especially during the time of lifting? Are there data available from some of the flights? And what values were observed in the layers?
The ozone data that have been added to Figures 4 and 5 provide answers to many of these questions. Additionally, the manuscript by Saunois et al., ACP 2009, investigates the regional ozone budget in more detail.

9) Figure 5: Please also add ozone here.
Added

10) *Conclusions: page 3877 line 7: Here the authors say that low acetone "can be explained by ocean uptake", which is contrast to the text. This sentence should be changed to something like: Low acetone might be cause by ocean uptake or long residence times of the measured air masses over the ocean without significant acetone sources.*
The sentence has been changed to read "It was not strongly correlated with CO in the mid-troposphere and its low abundance in the marine boundary layer implies either ocean uptake or that the sampled airmasses had long residence time over the ocean in the absence of significant sources."

Editorial comments:
1) page 3863 line 8: please explain abbreviation of FAAM
Done

2) page 3864 line 6: Holzinger et al 99 does not show that biomass burning is the main source of acetonitrile, better to use de Gouw et al 2003c here.
Done

3) Page 3871 line 12: typo: close to those observed
Corrected

4) *Marandino et al is twice in the reference list.*
The 2006 reference is a correction to the original paper. This has been updated in the reference list.