Response to the comments of Reviewer #2

This manuscript presents measurements of O_3, CO and CH_4 made from a ship sailing in the Bay of Bengal (BoB) during 2009. The work investigates the spatio-temporal variation of these trace gases, looking at the relationship between their observed mixing ratios and air mass origin and also investigates how well WRF-chem simulations can reproduce the observations. The paper is suitable for ACP and should be accepted subject to the following minor revisions:

We thank the reviewer for careful evaluation of the manuscript and valuable comments. All the comments and suggestions are incorporated as discussed below. Please note that the line number mentioned in the reply is corresponding to the revised manuscript "CTCZ-BOB-R1_Track_Changed".

General comments:

Section 5.1: The first part of the analysis looks at the variation in concentrations of the trace gases along the cruise track and attempts to explain them by looking at air mass origin (the % of residence time over land). This is shown nicely in figure 5, however I feel figures 2 and 3 could be merged (in general the paper has too many figures).

Figure 2 and 3 are merged in revised manuscript (please see Figure 2). Number of figures is now reduced to 12 (from 16).

The data seems to be divided into two regions (central and northern BoB) and I am not sure this is necessary. The difference in data taken in different areas in more likely to be driven by air mass origin rather than the area that the ship was in so I would stick to this analysis.

We have removed the discussion based on BoB region's division (Page: 1, Lines: 21-26; Page 7, Line: 244-248) and primarily used categorization based on trajectories (see also comments to reviewer#1). However, the computations of region wise mean values are only used for comparison with other seasons (subsection 5.4) to be consistent with previous papers. Such consistency is required for comparisons considering strong spatial variability over the BoB more pronounced during winter (David et al., 2011, Nair et al., 2011)

Section 5.2: In general I feel this section could be expanded. Why has CH_4 data not been investigated with the model here? From figure 5 it seems that there is reasonable agreement between the observed CH_4 and residence time over land of the air so it would have been interesting to see how well the model reproduced the CH_4. In general CH_4 data is often overlooked in the paper, even though the dataset seems reasonably complete and CH_4 is mentioned in the title. If the authors are not confident in the CH_4 measurements then they should be removed.

The section is expanded by adding the analysis of the influence of India's anthropogenic emissions to O_3 over BoB using model sensitivity simulation (See revised Fig 4, Page-11, Lines: 375-379). CH_4 from WRF-Chem was not analyzed as the existing model setup did not include explicit treatment of regional emissions of CH_4, being included through chemical boundary conditions from global model MOZART. Nevertheless the observational values are presented here for their use in future studies. To investigate the spatial variability in observed CH_4, retrievals of SCIAMACHY are now analyzed (Fig. 6) which reveals higher methane

concentration over central Indian region compared to southern Indian region during the study period, complementing the trajectory assisted analysis.
We are confident that our measurements and CH_4 data are reasonably complete to derive the spatial variation. Interestingly, despite of longer chemical lifetime, the observed spatial heterogeneity in CH_4 highlights the importance of transport from different source regions located in India to the BoB during the summer monsoon. It is further inferred from sector-wise analysis of emissions over the hotspot region (i.e. eastern IGP) that high CH_4 emissions are due to rice cultivation, waste treatment and livestock (Page: 10, Lines 338-339). The relevant discussion is also suitably revised in the manuscript (Page 10, Lines 328-343).

The correlation between presented in situ CH4 measurements with retrievals from AIRS satellite instrument was found to be statistically insignificant (not shown) which further highlights a need of reporting in situ measurements from this region (Page 10, Lines: 339-343).

Can the authors comment on the main in source of the increased ozone (e.g. anthropogenic / biogenic emissions).
We performed additional model simulation by switching off anthropogenic emissions in the model domain. As shown in revised Fig. 4, the spatio-temporal variations in O_3 over the BoB are mainly controlled by the regional anthropogenic emissions over the South Asia. On average, O_3 mixing ratios over the BoB are predicted to be reduced of about 14 nmol mol^{-1} in lack of anthropogenic emissions in South Asia. The manuscript is suitably revised to include the new analyses and related discussion (Fig. 4, Page-11, Lines 375-379).

What levels of NOx are seen in the model?
Mean NOx mixing ratios along the ship are calculated to be 135 ± 90 pmol mol^{-1}.

The comparison of meteorological parameters from the model does not add much to the analysis and the authors should consider removing it (which helps reduce the number of figures).
As suggested by the reviewer, we removed the comparison in the revised version of the manuscript.

Could the authors also compare model data to the measurements at the surface sites? This would help assess how well the model predicts the air coming into the region and whether this contributes to any discrepancies in the data after emissions and processing.
Detailed evaluation of WRF-Chem simulated ozone over surface sites in India has been conducted by Kumar et al., (GMD, 2012b). A comparison of O_3 measurements at Thumba with model showed a good agreement (R^2=0.6) and mean values compared typically within 1-standard deviation. We have shown surface O_3 is simulated within the 1-sigma variation at another stations (Gadanki) in southern India during monsoon (Ojha et al., 2016, Fig-8). This information is now provided in the revised manuscript (Page: 11; lines: 369-374).

Section 5.3: It seems that much of the data here has had to be removed due to contamination from the ship exhaust. This causes a large gap in the diurnal average where there is no data between 0600 and 1300, a time of particular interest for photochemistry. Because of this the authors should consider removing this analysis.
Following reviewer's suggestion, this section is removed in the revised manuscript.

Section 5.4: Figures 11 and 12 seem to essentially show the same thing – could the authors combine them somehow.
Suggestion is incorporated (Please see Fig. 8).

Section 5.5: The seasonal variation is investigated by examining data from a series of previous publications of measurements in the region, presented in table 3. The analysis here is good, however I find table 3 hard to interpret. Could the data presented as a figure?
The data of table 4 (seasonal variation) is already shown in Fig 16 (which is now Fig. 12 in the revised manuscript)

Minor comments:
Both 'O_3' and 'ozone' are used throughout the text. The authors should pick one and stick to it.
"O_3" is now used throughout the revised manuscript.

Line 151: How were the analysers calibrated? A few lines of detail and references should be given here.
Suggestion is incorporated (Page-5, Lines 168-171 and 181).

The authors should try to avoid excessive use of the terms 'we' and 'our' when describing the results.
Excess use of "we" and "our" is avoided in revised manuscript.

Figure 4 is very hard to interpret – could the authors find a clearer way of showing air mass origin for the different positions on the cruise track?
To make the figure clearer now only representative trajectories (instead of all) are shown. Following suggestion of Reviewer #1, a symbol is added along to trajectories representing a time difference of one day. (Please see Fig. 3)