

Interactive comment on “Variations in O₃, CO, and CH₄ over the Bay of Bengal during the summer monsoon season: Ship-borne measurements and model simulations” by Imran A. Girach et al.

Anonymous Referee #2

Received and published: 15 August 2016

This manuscript presents measurements of O₃, CO and CH₄ 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:

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

Printer-friendly version

Discussion paper



too many figures). 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 is 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.

Section 5.2: In general I feel this section could be expanded. Why has CH₄ data not been investigated with the model here? From figure 5 it seems that there is reasonable agreement between the observed CH₄ and residence time over land of the air so it would have been interesting to see how well the model reproduced the CH₄. In general CH₄ data is often overlooked in the paper, even though the dataset seems reasonably complete and CH₄ is mentioned in the title. If the authors are not confident in the CH₄ measurements then they should be removed. Can the authors comment on the main source of the increased ozone (e.g. anthropogenic / biogenic emissions). What levels of NO_x are seen in the model? 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). 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.

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.

Section 5.4: Figures 11 and 12 seem to essentially show the same thing – could the authors combine them somehow.

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

[Printer-friendly version](#)[Discussion paper](#)

figure?

Minor comments:

Both 'O3' and 'ozone' are used throughout the text. The authors should pick one and stick to it.

Line 151: How were the analyses calibrated? A few lines of detail and references should be given here.

The authors should try to avoid excessive use of the terms 'we' and 'our' when describing the results.

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?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-595, 2016.

Printer-friendly version

Discussion paper

