Interactive comment on **"The IPAC-NC field campaign: a pollution and oxidization pool in the lower atmosphere over Huabei, China"** ***by J. Z.*****. Ma et al.: Reply to Anonymous Referee #2**

We thank the anonymous referee for his/her constructive comments. Below the referee comments and author responses.

Referee comments

Ma et al. 2011 reported measurements and analyses from the IPAC-NC field campaign in Huabei, China in spring 2006. The first part of the paper described details of the campaign. The second part of the paper analyzed the measurements. Their analyses showed that lower troposphere over the Huabei area is highly polluted, and that some pollutants show local maximum concentrations near the top of the mixing layer. The severe pollution impacted the local HOx chemistry, which the authors simulated using a box model. They found the oxidation CO and SO2 to be important sources of HO2, which in turn forms ozone. They concluded that the severely polluted lower troposphere over Huabei acts as an oxidation pool over Eastern China. The thesis of the paper fits well with the scope of ACP. The IPAC-NC measurement campaign is well designed and the measurements are of great interested to the community.

I recommend that the paper be rejected at this stage. The paper in its current form is disorganized. The authors addressed many scientific issues but in a disconnected fashion and important details are missing. The paper is also very long. I recommend that, after major revisions, the authors re-submit the present materials as two or three separate papers: the first paper as a general description of the aircraft campaign (or simply cite previous papers, e.g., Ma et al., [2010]) and a second paper focusing on HOx chemistry in detail based on the aircraft measurements.

I also recommend that the authors hire a professional editing service to correct grammatical mistakes and to make the papers more readable.

Author Responses

We thank the referee for positive considerations of our field experimental and modeling work. We also agree with the referee in that too many scientific issues were addressed but in a disconnected fashion in the original manuscript. As suggested by the referee, we have removed the parts on comparisons between regional modeling and satellite retrieved tropospheric NO_2 and on secondary organic aerosol formation. A detailed description of the chemical box model, which was given in Sect. 3.4 of the original manuscript, has been moved to Sect. 2.2 of the revised manuscript. With such a revision, the manuscript should be shorter and better organized. We would rather like to still keep the measurement analyses and modeling work together in one manuscript, and this may help readers to understand the main points of this research, i.e., the regional pollution characteristics (by measurements) and oxidation capacity over the Huabei region (by modeling), in a connected way. We have also improved the use of English language in the revised manuscript.

Referee comments

Major issues:

1. Throughout the paper and particularly in Section 2, the way the authors describe the geography is problematic. They use province, city, and road names unfamiliar to the readers (e.g., Beijing, Tianjing, Tangshan, Taiyuan, Hebei, Shanxi, 4th Ring Road, etc). Moreover, they introduce multiple Chinese names to describe geographical areas, which are very confusing and seem unnecessary to the reviewer (e.g., "Jing-Jin-Tang", "Jing-Jin-Ji"). I would strongly recommend not using these Chinese names. I would also recommend that a regional-scale map be added, on which major cities and areas be marked out.

Author Responses

It is true that some readers may be unfamiliar with these names, but we have no other names more suitable to replace them at the present time. If we translated the Beijing into Northern Capital and Tianjin into The Emperor's Harbor according to the meanings of these Chinese names, people would not understand what we mean. Even in the English maps, these cities are given by their Chinese names in pronunciation, i.e., Beijing,

Tianjing, Tangshan, Taiyuan, Hebei, Shanxi, etc. The 4th Ring Road can also be found in the English maps of Beijing. We give the latitude and longitude values of these cities in the text. As suggested by the referee, we have marked out the positions of Beijing, Tianjin, Tangshan and Shijiazhuang cities and the Bohai Gulf in Figs. 1 and 3-4 of the revised manuscript. The position of Beijing and the Bohai Gulf indicated in the map can help the reader to find the position of Huabei in China or Asia. Using a larger regional scale map could certainly help the readers to find the position of the region, but it might not be easier for them to compare model results (Figs. 2-4) and aircraft flight tracks (Figs. 5-6) with emission distributions (Fig. 1). The names "Jing-Jin-Tang" and "Jing-Jin-Ji" are used to avoid repeat long names or sentences in the same section. We have replaced "Jing-Jin-Tang" with "the larger Beijing, Tianjin and Tangshan area (Jing-Jin-Tang)" in the legend of Fig.2.

Referee comments

2. Similarly, the author assumes that the readers are familiar with the topography of this area (Page 27708, lines 1_2). Please find a way to present this better.

Author Responses

An introduction of 'Jing-Jin-Tang' is given in the first sentence of this section: "The IPAC-NC campaign was performed in the larger Beijing, Tianjin and Tangshan area (Jing-Jin-Tang in Chinese) with some flight tracks extending towards the Bohai Gulf". As suggested by the referee, we have marked out the positions of Beijing, Tianjin, Tangshan and Shijiazhuang cities in Figs. 1 and 3-4 of the revised manuscript.

Referee comments

3. The authors included several model analyses that should either be deleted, or described in detail as part of a full-blown paper dedicated to the topic. For example on Page 27708, the authors presented a passive tracer simulation using the GRAPES model to demonstrate the confluence of pollutants at 925 hPa. No details (and no citation) were given about the GRAPES model. It would be better to simply delete this analysis and describe the idea base on prevailing wind. Alternatively, the authors could cite previous papers demonstrating the confluence of pollutants at 925

hPa.

Author Responses

We have added detailed descriptions of the chemical box model to Sect. 2.2 of the revised manuscript. Please see Page 7, Line 28 to Page 9, Line 6: *"In this study we investigate the radical chemistry and ozone production using a box model based on the NCAR Master Mechanism (Madronich and Calvert, 1989;1990;Ma et al., 2002b), ····· Since most long-lived species were fixed, the concentrations of radicals and other organic products could attain a steady state during the last few hours with changes less than 1%."* We have also given a full name of the GRAPES model and several important citations in the revised manuscript. Please see Page 6, Line 25-26: "(Global/Regional Assimilation and Prediction Enhanced System; Wu et al., 2005; Chen and Shen, 2006; Xue et al., 2008)". The convergence of air masses over the Huabei region is a well-known weather phenomenon in China, and GRAPES can predict the prevailing wind pattern well with an advanced assimilation system. To our knowledge, however, no previous studies have demonstrated the convergence of pollutants over the region as done in this study. By adding a passive tracer to GRAPES, we show that the pollutants emitted from several important urban and industrial centers in different areas accumulate in the lower atmosphere over the region.

Referee comments

4. Section 2.2: I do not see the point of this comparison of OMI and the RCTM model. If the author's intention is to validate emission inventories, they should do so in a separate paper, where details of OMI retrieval and the RCTM model are included. However, the accuracy of the emission inventory is not relevant to the aircraft measurements presented in this paper. I would recommend that the authors delete this section or briefly describe the major sources of NOx by citing an emission inventory.

Author Responses

As suggested by the referee, we have deleted this comparison of OMI and the RCTM model. Instead, we have presented the regional distributions of NO_x and their source attributions in the PBL as simulated by our RCTM in the revised manuscript. Applying

the tagging method in RCTM combined with an emission inventory should give more valuable information than merely citing an inventory.

Referee comments

5. Page 27710: the locations of the surface sites should be marked in a regional-scale map.

Author Responses

We have marked out the positions of the rural site together with Beijing, Tianjin, Tangshan and Shijiazhuang cities in Figs. 3-4 of the revised manuscript. This may help the readers to locate the positions of the surface sites easily.

Referee comments

6. Page 27711: aerosol measurements are not important to the HOx analyses. However, if the authors choose to include the aerosol content, then the measurements should be described in more detail.

Author Responses

Measured aerosol particle size distributions are used in the calculations of HOx loss rate, as described in Sect. 2.2 and Sect. 3.4 of the revised manuscript. Here we just give a general description of instruments used for aerosol measurements. In the last sentence of this paragraph, we state "*Detailed descriptions of the instruments and aircraft sampling system can be found in Wang et al. (2008) and Ma et al. (2010)*".

Referee comments

7. Figures 4 and 5 should be combined and presented with better geographical context (e.g., regional scale, better distinguished coast lines, etc).

Author Responses

It is difficult to combine the two figures. We have marked out the position of the Bohai Gulf in Figs. 6 (Fig. 5 in the original manuscript). This, together with labeled latitude and longitude values, should give helpful geographical information to the readers.

Referee comments

8. Section 3.1: The detailed description and comparison of the measurements at the Beigongda and Xin'an sites seem unnecessary in this paper, since the surface sites merely provides measurements at the lowest level. Detailed discussion of the surface measurements should be presented in a separate paper. Some of the comments about the surface measurements are also questionable (Page 27713, lines 21_23; page 27716, lines 1_5).

Author Responses

We would like to still keep the description and comparison of the measurements at the Beigongda and Xin'an sites. These discussions should help the readers to learn the regional pollution characteristics in Huabei, which is one of the two main points of this article. Since surface measurements persisted during the campaign, the diurnal variation patterns of pollutants could provide valuable information on chemical and dynamic roles in the urban and rural areas. The part of discussions on toluene/benzene (t/b) ratio (including page 27716, lines1_5 as pointed out by the referee) have been deleted in the revised manuscript.

Referee comments

9. How robust is the SO2 local maximum signal at 0.4 km? The local maximum is largely due to lower concentration at the surface, which is based on ground-based measurement at Xin'an. How representative is that as an average surface concentration for the flight area? Also, the authors mentioned that the minimum altitude that the aircraft can fly is 0.4 km. What is the data altitude range from which the average concentration at 0.4 km is calculated?

Author Responses

As shown in Fig. 3, the Xin'an station is located at the center of the larger Beijing, Tianjin and Tangshan area, and can represent an average surface area of polluted rural areas of the region. The station is also located in the center of the areas where most flights took place. The same time period is selected to calculate surface SO_2, NO_x and CO concentrations. The local maximum at 0.4 km for SO_2 is significant compared to the local maximums at the surface for NO_x and CO. We use altitudes<0.6 km to calculate the average concentration at 0.4 km.

Referee comments

10. Section 3.4: the NCAR box model is a major tool in this work and should be described in more detail.

Author Responses

As mentioned above, we have added detailed descriptions of the model to Sect. 2.2 of the revised manuscript.

Referee comments

11. Page 27719 and Figure 10: The size distributions of aerosols are already presented in Ma et al. (2010). Would it be sufficient to simply cite that paper and summarize the key findings here?

Author Responses

In this manuscript, we present the size distributions of aerosols in a different way from Ma et al. (2010), i.e., the average size distributions (X-axes) as a function altitude (Y-axis). Moreover, in Ma et al. (2010) no plot on the size distributions of aerosol surfaces was shown. Therefore, we would like to still keep Fig. 11 (Fig. 10 in the original manuscript), and this may help the readers to learn the potential heterogeneous role of aerosols over the region in a quantitative way.

Referee comments

12. Page 27720_27721: What is the point of comparing OH measurements over cities of different latitudes and in different seasons?

Author Responses

We should admit that OH measurements were very sparse until now. Here we just give a review of previous studies so that the readers can get an idea of OH levels measured in different regions.

Referee comments

13. Page 27723, lines 21_24: The relative importance of VOC oxidation to OH reactivity depends on local VOC emissions. The season and latitude of the MCMA

and PRD measurements should be clearly stated.

Author Responses

We have added the season and latitude information in the revised manuscript (Page 20, Line 26-27).

Referee comments

14. Page 27728_27730: The analysis on aerosol condensation rate is a separate scientific question and deserves a dedicated paper. I recommended deleting this part to make the present paper more focused. Also, the analysis on SOA formation is problematic, since the semi-volatile oxidation products of VOCs leading to SOA formation are largely unknown. It is not clear how these calculated here. What model is used? Moreover, an important SOA precursor is monoterpene, which is not accounted for here.

Author Responses

We have removed this part as suggested by the referee.

Referee comments

15. The statement that Huabei "acts as an oxidation pool for Eastern China" would imply that a large fraction of pollutants emitted in Eastern China are transported to Huabei to undergo photochemical reactions there. I do not see evidence of this. I would recommend that the statement be revised to indicate that the photochemistry in the lower troposphere over Huabei is strong.

Author Responses

We mean that the pool is located in Eastern China, instead of pollutants from outside are transported to undergo photochemical reactions in Huabei. As suggested by the referee, we have changed the sentence to "*Our results indicate that the lower atmosphere over Huabei is not only strongly polluted but also acts as an oxidation pool with pollutants undergoing very active photochemistry over this part of China*" in the revised manuscript (Page 2, Line 3-5).

Other minor comments:

Referee comments

1. Page 27702, lines 21_23: How can the formation of SOA add to the loadings of mineral dust?

Author Responses

We mean that the low volatile gaseous organic species can condense on dust aerosols. This sentence has been deleted as all the parts on second OC formation have been removed in the revised manuscript.

Referee comments

2. Page 27711, lines 5_8: How are VOC concentrations measured?

Author Responses

We have given a description of method in the revised manuscript. Please see Page 10, Line 18-25: "*NMHC species were detected using a cryogenic pre-concentrator (Entech Instrument 7100A, SimiValley, CA) and a gas chromatograph (Hewlett Packard 6890) equipped with two columns and two detectors. The C2-C4 alkanes and alkenes were separated on a nonpolar capillary column (HP-1, J&W Scientific) and quantified with a flame ionization detector (FID). The C5-C12 hydrocarbons were separated on a semi-polar column (DB-624, J&W Scientific) and quantified using a quadrupole mass spectrometer (MS, Hewlett Packard 5973). Detailed description of such GC/FID-GC/MS analysis method can be found in Liu et al. (2005; 2008)*".

Referee comments

3. Page 27711, lines 18_19: "2 L min-1" instead of "2 l min-1"

Author Responses

We used "2 L min-1" before, but the editor office prefers to use "2 l min-1".

Referee comments

4. Page 27716, lines 19_20: There was no description of "an instrument problem" in Section 2.3, only a statement about the CO instrument taking longer to equilibrate.

Author Responses

We just meant such problem (longer equilibration time). We have deleted this sentence in

the revised manuscript.

Referee comments

5. Page 27725, line 27: What is the meaning of "calc/obs"? Please revise.

Author Responses

We have changed it to "calculated-to-observed" in the revised manuscript.

Referee comments

6. Page 27725, lines 28_29: "run" is too colloquial. Consider changing to "simulation".

Author Responses

We have changed it to "simulation" in the revised manuscript.

Referee comments

7. In text and in reference list: Wrong citation format: "Zhang et al. (2008a)" and "Zhang et al. (2008b)" should be "J. Zhang et al. (2008)" and "L. Zhang et al. (2008)".

Author Responses

While "J. Zhang et al. (2008)" and "L. Zhang et al. (2008)" are the citation format for JGR, "Zhang et al. (2008a)" and "Zhang et al. (2008b)" should be used here according to previous papers that have been already published in ACP. We will check this issue with the ACP editors.

Referee comments

8. Figure 2: the arrows on the streamlines are not readable. Please magnify.

Author Responses

We have enlarged the arrows in Fig 2 of the revised manuscript.