Atmos. Chem. Phys. Discuss., 11, C13431–C13435, 2011
www.atmos-chem-phys-discuss.net/11/C13431/2011/

Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

Received and published: 20 December 2011

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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.

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.

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

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

ACPD
11, C13431–C13435, 2011

Interactive Comment

analysis and describe the idea base on prevailing wind. Alternatively, the authors could cite previous papers demonstrating the confluence of pollutants at 925 hPa.

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.

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

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.

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

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).

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

ACPD
11, C13431–C13435, 2011

Interactive Comment

concentration at 0.4 km is calculated?

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

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?

12. Page 27720∼27721: What is the point of comparing OH measurements over cities of different latitudes and in different seasons?

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.

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.

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.

Other minor comments:

1. Page 27702, lines 21∼23: How can the formation of SOA add to the loadings of mineral dust?

ACPD

11, C13431–C13435, 2011

Interactive Comment

2. Page 27711, lines 5∼8: How are VOC concentrations measured?

3. Page 27711, lines 18∼19: “2 L min-1” instead of “2 l min-1”

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.

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

6. Page 27725, lines 28∼29: “run” is too colloquial. Consider changing to “simulation”.

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)”.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27701, 2011.