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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 #1

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Ma et al. presented ground and airborne measurements of atmospheric pollution in the Northern China region. As the authors discussed in the introduction section, Not only in regional standpoints but also in global standpoints, atmospheric chemistry of the Northern China region should be well characterized, considering its impacts towards air-quality and climate in regional and global scales. However, published in-situ measurement results so far, especially very close to the emission regions have been very limited. In that perspective, Ma et al.'s comprehensive ground and airborne measurement dataset should be widely available to the atmospheric chemistry community. This purpose is very well fit with the aims of Atmospheric Chemistry and Physics and this special issue. Based on the measurement dataset, authors conducted constrained

box model calculations to understand regional oxidation capacity, which I believe a scientifically interesting work. However, the ways to present the results and comparisons with previous studies can be far much better than as presented in the manuscript is right now. Therefore, I recommend for authors to reorganize presentations of analysis results for publication to ACP.

Major Points for Reconsideration

1) Overall, the authors tried to address too many scientific questions with a limited dataset. For example, in section 2.2., the authors discussed NO2 from modeling and satellite products. The results, as presented in Figure 3, clearly showed that the modeling results significant underestimated the satellite observation results. The reasons could potentially suggest problems in many different aspects of modeling both chemistry and emission characterizations, which can be easily summarized as a separate research paper. However, the authors only discussed a few sentences about the differences. Moreover, these emission-regional model products were not even discussed in the results-discussion section. Therefore, the insufficient regional modeling and satellite intercomparison discussion as current is appeared only distraction for developing ideas to the main points of this article. I strongly urge authors to remove this part. In addition, I suggest for the authors not to discuss about regional secondary organic aerosol forming potential in this paper. The dataset, presented in this work is too limited to discuss aerosol chemistry. On the ground, there was no aerosol measurement. In addition, the airborne measurement suite only contains aerosol physical parameter measurements. Moreover, the gas-phase measurement dataset does not contain any oxidation products such as sulfuric acid or oxygenated organic compounds (rather only hydrocarbons). These limitation on the measurement dataset can potentially cause significant uncertainty in discussion on secondary organic aerosol formation. Last but not least, the box-model, presented in this study is for gas phase chemistry not for multi-phase chemistry. In this context, I suggest that the authors should more focus on predicting oxidation capacity in the North China region and discuss about its implica-

tions.

2) All over the text, the authors compared the model calculated values with the previously reported actual measurements. Thoughtful care should be taken in the comparisons between measurement and model products. This is especially true for OH that have shown significant discrepancies between model and measurements. Moreover, the authors compared with the model derived OH profile in the heavily polluted North China region and measured OH profiles from relatively clean North Pacific and Gulf regions and extremely clean the rain-forest region. Especially the argument, derived from these improper comparisons, appeared in the bottom page 27721 can mislead readers. The authors cannot simply argue that pollutants are "efficiently" oxidized due to high model derived OH concentration. The whole argument either should be eliminated or supported by more comprehensive chemical transport model calculations.

3) The whole discussion about NO2 and NO2* is very confusing in all over the manuscript. Obviously, the discussion about potential interference from other NOy species to the NOx channel for an instrument with a thermal converter is correct and the significance of the interferences should be considered when NO2 was measured by a thermal converter equipped analyzer. However, just introducing the idea about potential interference is not enough considering the scope of this study. This is especially important when one compares datasets from an urban and a rural area just like this study. In urban areas, near NOx emission sources, a thermal converter probably works just fine. However, in rural environments away from the source regions, a significant amount of NOy potentially interferes NOx measurement with a thermal converter. I suggest that the authors should revisit the measurement and the modeling datasets and compare what are the distributions of the NO2/NOx ratios. By comparing the rations in two different datasets, more quantitative information of the potential interferences from NOy to the NOx channel can be presented. The discussion about the distribution of measured NOx should be followed after this evaluation.

Specific issues to be addressed

Page 27702 Line 15-16 I am not sure how meaningful it is to discuss difference of 1.4e6 molecules cm-3 in OH concentration in model calculation results. We are dealing with $\sim$ 40 % of differences in very reactive species that actual in situ measurement has an analytical uncertainty of more than 30 %. If the authors would like to make this as a main finding on the paper, they should justify this point.

Page 27703 Line 11 Officially, the continent of America is divided by North, Central and South and Mexico is in North America as far as I know. Double check with the official geographical region!

Page 27707 Line 3 "associated with high OH": How high is really high? The OH levels from the model calculations don't seem particularly high compared with previous measurement and model calculation results.

Figure 1. If readers are not familiar with geography in China, this map is little tough to read. Use a regional scale map!

Page 27710 Line 7 "East 4th Ring Road" If readers are not familiar with Beijing, probably they cannot get an idea about proximity to the city center. Find a batter way to present this

Line 12 Is this Xian? or Xinan?

Line 22 It is not clear whether NO and NO2 are detected by two separate instruments or one instrument was used for NO and NO2 measurements. If an one channel instrument was used for the airborne measurements, the authors should describe about how the data gap was handled while one channel was devoted to measure either NO or NO2.

Page 27713 Figure 6.It is very odd that the dataset have significant nighttime NO and nighttime O3 at the same time. This seems to be a case both the urban and the rural sites. In the presence of ozone, usually NO is titrated into NO2 without solar radiation (e.g. JNO2 = 0). Provide justifications!

Page 27715 First paragraph: It seems that the authors were trying to identify whether

the Northern China region is NOx or VOC limited regimes in ozone production. To address these issues, a lot more care should be taken. The scope of discussion is not appeared wide enough to draw the conclusion about the ozone production regime. The authors should include modeling analysis for NOx-VOCs-ozone photochemistry.

Second paragraph: VOC analysis results should be presented as followings. 1) Along with concentration information, the authors should present reactivity scale information. Due to a wide range of reaction constants of various VOCs to OH in the atmosphere, A simple concentration comparison is almost meaningless. 2) By taking ratios of different VOCs with different lifetimes, the dataset may provide some VOC aging information. This is especially important for comparison between the urban and the rural sites.

Page 27718 Line 26: Obviously, the authors calculated NO2 using the box model. Discuss about how different between model calculated NO2 and measured NO2* (or similar between them) and why

Page 27719 Line 4: Justify why the authors assumed 1.5 ppbv of formaldehyde! This could be a very important source for OH by photolysis so a correct assumption should be warranted.

Page 27720 - 27721 As pointed out above, more careful discussion is required when the authors compared model calculated OH from this study and measured OH from previous studies. Especially, many studies have reported significant discrepancies between model calculated and measured OH.

Page 27723 Reconsider the way of categorizing altitude! I would recommend that the authors reassess data based on whether the data were collected from above or below the boundary layer.

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