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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone pollution around a coastal region of South China Sea: Interaction between marine and continental air" *by* Hao Wang et al.

Anonymous Referee #2

Received and published: 29 January 2018

This paper reports intensive field measurements at two sites over the South China Sea. The spatial distribution of ozone pollution and its favorable synoptic conditions were interpreted. The authors also tried to link, by the sea-land breeze, the transport of continental pollution to oceans and the recirculation of land-originated aged air masses from ocean to the coastal regions. The manuscript is generally well written and easy to follow. The following specific comments should be addressed before it can be considered for publication at ACP.

Specific Comments:

Page 1, Line 6: Hong Kong, China

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Discussion paper

Page 2, Lines 5-6: the authors may either spell out TC and WS, or just remove them from the abstract.

Page 2, Line 15: the word “magnified” may be not appropriate here. The ozone-laden air may be transported to a larger area over the oceans, but should not be “magnified”.

Page 3, Lines 14-19: to date, the long-term O3 trend studies were relatively limited in China. The authors should refer to the following earlier studies in Hong Kong, the PRD region and northern China.

Sun, et al., Significant increase of summertime ozone at Mount Tai in Central Eastern China, Atmos. Chem. Phys., 16, 10637-10650, 2016.

Xue et al., Increasing external effects negate local efforts to control ozone air pollution: a case study of Hong Kong and implications for other Chinese cities, Environ. Sci. Tech., 48, 10769-10775, 2014.

Page 4, Lines 24-26: a more recent study has investigated the detailed chemical features including the radical chemistry in different air masses arriving at the South China Sea.

Li et al., Oxidizing capacity of the rural atmosphere in Hong Kong, Southern China. Science of the Total Environment. 612. 1114-1122. 2018.

Page 5, Line 21-22: it is not the case that northeast monsoon prevails in late summer. The O3 episode occurring in late summer in Hong Kong is mainly related to the tropical cyclones.

Page 6, Lines 8-10: it has been known that the traditional commercial NOx analyzer may be subject to significant positive interference for the NO2 measurements, especially at the rural and remote areas like WS. The authors need state the uncertainty of the NO2 measurements and the subsequent observation-based modeling analysis.

Page 8, Lines 16-18: so the OBM was not constrained by the measured HONO and

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OVOCs, right? This may affect the accurate modeling of OH radicals and ozone formation. As the OVOC measurements were available in the present study, the authors should constrain the model with the measured OVOC data.

Page 9, Lines 19-27: it would be better if the authors could provide the time series of model simulations and observations for a direct comparison, maybe in the supporting information.

Page 10, Table 1: it would be much better if the statistics of the most abundant NHMC and carbonyl species are individually shown, instead of the bulk concentrations.

Section 3.2: this section is too long and contains a lot of general description of the typhoon, continental anticyclone, and sea-land breeze (most of them are already well known). The authors may consider to further shorten such general descriptions and mainly highlight the new results obtained in this study.

Section 3.3.3: it is not clear whether the modeling analysis was conducted for the campaign average condition or for a particular case. Furthermore, the sub-title of this section may be not appropriate as this section only talked about the simulated OH level and O3 formation, other than the atmospheric oxidative capacity.

Page 21, Lines 8-10: based on the current analysis, I don't agree that the atmospheric oxidative capacity is stronger at the coastal WS than polluted TC site. HONO photolysis is a very important OH source in polluted areas including the TC site (Xue et al., 2016), which was not included in the present study. So the OH levels should be underestimated at TC. Moreover, the lower OH levels at TC should be due to the fast radical cycling given the more abundant VOCs. I presume that the HO2 and RO2 levels at TC should be significantly higher than those at WS.

Xue et al., Oxidative capacity and radical chemistry in the polluted atmosphere of Hong Kong and Pearl River Delta region: analysis of a severe photochemical smog episode. Atmospheric Chemistry and Physics. 16. 9891-9903. 2016.

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Page 21, Lines 15-22: it is surprising that the O3 production rates at WS were much higher than those at TC, especially on the episode days, given that the NOx and VOC levels were much higher at TC than at WS during O3 episodes. What's the possible reason for this?

Section 3.3.3 and Figure 5: it would be better if the ozone production rates were expressed in ppb/h so that it can be easily compared with the observed ozone increase.

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