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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 #1

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This paper provides a detailed study of trace gases and meteorology at two sites, one a suburban site in Hong Kong (labelled TC), the other a coastal site (labelled WS), with few local anthropogenic emissions, on the edge of the South China Sea. The sites are separated by $\sim$ 40 km. Emphasis is given to ozone episodes (>100 ppbv) and near episodes, which occurred on a number of occasions, some extending over 9 days, during two $\sim$50 day periods in August, September; October, November 2013. The results are rationalised in detail using a range of modelling techniques: a zero dimensional box model study using the master chemical mechanism (MCM); the Weather Research and Forecasting (WRF) model to provide wind fields and coupled with the CMAQ model to provide an Eulerian representation of the physical and chemical processes over a wide

area and HYSPLIT to provide backward particle release simulations to understand air mass origins. The paper provides a very useful dataset and an interesting analysis. The results are discussed in terms of the interaction between marine and continental air. The episodic ozone concentrations are significantly higher at WS than at TC and much of the paper relates to a discussion of the origin of these differences, which are ascribed to weaker NO titration and to a stronger oxidative capacity at the coastal site. The main meteorological features during the episodes were tropical cyclones, with transport from the polluted Pearl River Delta Region to the sites, continental anticyclones, which again brought air from polluted inland areas and Sea Land Breezes, with alternation of onshore and offshore winds. My main concern is with the contention that the results relate to the interaction between marine and continental air, which is included in the title and pervades the text. WS is one of several islands lying close to the coast. Its important characteristic is that there are few local emissions so that NOx is low. Other pollutants, CO, SO2, NMHC show clear indications of advection of polluted air, but the concentrations are on average lower than those found at TC. The wind patterns confirm that the air is primarily, perhaps exclusively during the episodes, of continental origin. Even the SLB winds from the sea simply advect high ozone concentrations, formed in polluted air, back to the coastal region. Marine air has much less impact than is found and has been widely discussed at, say, Mace Head in Ireland or Cape Grim in Tasmania. The observation of higher ozone at WS compared with TC derives primarily, as argued, from the low emissions at WS and the consequently much lower NOx and reduced titration via NO + O3. The most telling observation is the near equivalence of the total oxidant concentration at the two sites during both episodes and non-episodes (p 20). Similar behaviour is of course found in many other locations when comparing rural and urban ozone concentrations in similar air masses. It is the absence of local NOx emissions at WS that leads to the differences; it is not specifically related to its coastal location and certainly not to marine influences. The discussion of the daily ozone profile could also be improved. âĂć The diurnal variation is superimposed on a residual night-time ozone concentration, which is substantial,

Figs 2 and S4. This might be discussed. âĂć Is the higher rate of ozone formation, shown in Fig 5b, a reflection of the high ozone concentration itself? It would be helpful to show the concentrations of OH, HO2 and RO2 vs time and also their rates of production and loss. Is the enhanced [OH] a result of increased O1D production from the higher [O3] found at WS in episodes? These plots could, if necessary, be shown in the Supplement. âĂć It would also be helpful, again in the Supplement, to see ozone, OH, HO2 and RO2 concentrations, and ozone and radical rates of formation and loss on a specific episode day. Using averages can lead to a loss of clarity and understanding. Two additional points: O1D in the caption to Fig 5 should be O1D. The English needs a good deal of attention, particularly the frequent absence of definite / indefinite articles. The paper makes a substantial contribution and should be published in ACP. The authors, though, should consider the points made above relating to the overall emphasis of the paper and the clarity of the discussion on chemical processes.

(report also included as pdf)

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-988/acp-2017-988-RC1-supplement.pdf

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