

Interactive comment on “Ozone production and hydrocarbon reactivity in Hong Kong, Southern China” by J. Zhang et al.

J. Zhang et al.

Received and published: 13 December 2006

Specific comments:

(1) We added words in the text on Page 4 stating frequent ozone episode in the autumn. (High ozone events also occurred in the summer but with a much lower frequency.)

(2) Section 2: We have added a summary of the previous work.

(3) We added the information on the detection limit for NO and pointed out other HKEPD stations use instruments that are similar to those in the US. References for details on the instruments were also added.

(4) A sentence was added in the text on Page 13 stating no significant difference between 8 November and other episode days.

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(5)The sentence has been modified to indicate the strong titration of O₃ by NO and to eliminate the speculative statement on the vertical mixing, as no observation data was able to support the statement.

(6)A figure was added in the paper to show the variation of dCO/dNO_y.

(7)Since the field campaign was carried out during the period of October to December, the biogenic emissions were not strong. A separate work (So and Wang, manuscript in preparation) discussed the biogenic VOC in summer compared to that in autumn. The peak of biogenic emissions in this region is in summer when the temperature is higher, and up to 5 ppbv of isoprene has been observed at Tai O.

(8)Biogenic VOC is not part of R-OLE. So here R-OLE is due to the anthropogenic emissions. We focus on the ozone episode days in this study. VOC concentrations at Tai O during non-episode days are generally similar to that for episode days, but with much lower concentrations compared to the episode days.

(9)We assumed the hydrocarbon concentrations above the boundary layer as zero in this study. There was no measurement of VOCs above boundary layer over HK. Aircraft study of other trace gases and aerosols (Kok et al., 1997, JGR) suggests that the outflow from the mainland was predominantly in the boundary layer. In the model calculation, however, we have considered all the possible VOC sources in the item of 'source' when we derived the hourly VOC profile.

(10)A figure was added to show the derived diurnal VOC profiles at the sites other than TO and compared with the measured VOC at TO. At TO no 24-hour average samples were collected, only measured daytime VOC concentrations are available. Therefore what we can do is to compare the derived VOC profiles at the sites other than TO with the measurements at TO. It can be seen the modeled diurnal profiles look reasonable, for instance, capturing morning traffic and continuous nighttime emissions at some urban sites.

(11)Reaction (3) occurs at night. We followed the method of Harrison et al. (1996) in determining the rate of HONO production and dry deposition during the nighttime. When the mixing height is small, generally the concentration of NO₂ and H₂O is high, and the reaction rate is high too. The opposite when the mixing height is large. We added a sentence in the text to compare our results of HONO and HCHO in early morning with some published studies in Milan and Berlin (Alicke et al., 2002, 2003).

(12) We added titration and radical scavenging by high NO being the reason for the negative RIR for NO. The NO concentration on 9 October is relatively low compared to other episode days. We don't have clear answer about a transition threshold between NO_x-saturated and NO_x-sensitive for a given VOC reactivity, but this should be investigated in future.

(13)Due to page limit, the details of the sensitivity tests are not shown in the text, but we have added a brief summary of the parameters tested and results. Although using 24-hour average concentrations to drive the OBM gives the similar RIR results as using hourly concentrations, there are still some differences. For a better understanding of daytime photochemistry, hourly data on VOC would be more desirable.

Technical corrections:

1.The phrases have been changed in the text.

2.They were meant to be subtitles.

3.We placed the OBM sensitivity to NO and CO approximations in Section 2 to better explain the influence of the data approximation on the results.

4.CO is also high on some non-episode days, but lack of favorable meteorological condition/transport pattern may be the reason for not very high ozone concentrations on these days.

5.TUV is relatively high on 7 November, but other days may have more UV but low ozone (e.g., Oct 21-22) due to very low levels of ozone precursors associated with

inflow of maritime air. Thus high UV is only one of a set of conditions for high ozone to occur.

6. We changed the title of Figure 3 to 'Diurnal variations of (a) nonmethane hydrocarbons and (b) NO observed at Tai O on the episode days.' The abnormally high concentrations in the early morning on Oct 11 and Nov 7 may be due unusual weather conditions preventing air-pollution dispersion and/or unusual nighttime transport patterns that brought more urban pollutants to TO. We have modified the sentence in the text.

7. The sentence should be 'since its extremely low reactivity though its high concentration'. We corrected it.

8. A sentence was added in the text on Page 17 to state how the 'observed O₃ increment' was estimated. Numbers (%) were added to quantify "agreement".

9. We kept only Figure 7b (now Figure 8).

10. An arrow was added to point in the direction of the major mainland source region in Plate 1.

11. We changed Figure 3 to a log scale. In this way, the NO concentrations are shown better. (The line section with values below zero was actually produced by making the line smooth. They were not actually below zero.)

12. Acronyms were defined in the captions.

13. Date was added in the caption of Figure 7 (now Figure 8).

14. A sentence was added in the caption of Figure 11 (now Figure 13) for RIR estimation.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8961, 2006.

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