Atmos. Chem. Phys. Discuss., 10, C3931–C3933, 2010
www.atmos-chem-phys-discuss.net/10/C3931/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone production during the field campaign RISFEX 2003 in the Sea of Japan: analysis of sensitivity and behavior basing on an improved indicator" *by* Z. Q. Wang et al.

Anonymous Referee #2

Received and published: 13 June 2010

Review of "Ozone production during the field campaign RISFEX 2003 in the Sea of Japan: analysis of sensitivity and behavior basing on an improved indicator" by Z.Q. Wang et al for Atmos. Chem. Phys.

In this paper model the sensitivity of P(O3) to changes in NO and VOCs is analyzed using an indicator equal to the ratio of HC-OH reactivity to NOx-OH reactivity. This indicator has the same form as one proposed by Frank et al (2001), but differs in that CO and CH4 contribute to HC reactivity.

I would like to call the authors attention to Fig. 16 of the paper that contains Fig. 14 included by S. Sillman in his review. That figure shows Ln/Q as a function of NOx and

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VOC reactivity. Points with constant Ln/Q cut across lines with a constant NOx to VOC reactivity ratio. The same would be true if NOx reactivity instead of NOx concentration were plotted. If Ln/Q is a valid indicator, then Fig. 16 shows that phi alone can not explain P(O3) sensitivity. Radical production rates are important as shown by the dependence of phi(optimum) on J(O1D) and H2O in this manuscript. The absolute concentration of NOx also has a strong effect on sensitivity. (Counter statement is on p 10562, line14-17) This has been shown in simplified calculations and has been used by S. Sillman to explain the time evolution of plumes advected in a shallow boundary layer over Lake Michigan.

Sillman, S. and P. J. Samson, Simulated ozone over Lake Michigan and the northeast corridor: identifying the differences between hydrocarbon-limited and NOx-limited regions. Presented at the 86th Annual Meeting and Exhibition, Air and Waste management Association, June 13-18, 1993, #93-WP-68B.06. I think there is also a journal article but I can't find it.

While I am sympathetic to the difficulty of writing in a foreign language, this manuscript has to be edited for proper English usage. The title contains two errors. analysis should be capitalized and based should be substituted for basing. There are too many such errors for me to list.

Results on P(O3) sensitivity are of interest and I don't dispute the importance of the NOx to VOC reactivity ratio. However, there are many difficulties in the presentation which makes it difficult for me to judge the quality of the work. I also would like to see the authors address the points raised above about the dependence of P(O3) sensitivity on radical production rate and NOx concentration. I believe that this article requires major revisions.

Specific comments It would help the reader if average concentrations and some measure of their range were specified.

I would avoid terms such as obviously

ACPD

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p 10554 The first figure mentioned in the text is Figure 4. This first figure should be first and should become Figure 1.

p10554 line 12 nitrous should be nitrous acid

p 10554 line 13 Implication is that alkenes are photolyzed.

p 10555 line 5 and Eq R10. HONO is not a stable product.

p 10562 I don't understand how the P(O3) sensitivities that are shown in Fig. 5 are calculated.

p 10563 Effect of changing individual hydrocarbons by 50% is dependent on their fractional contribution to total reactivity, which is not given.

p 10563 line 28, MACR from the sea methacrolein is an oxidation product of isoprene

p 10558. Why is it important to information on aerosol instruments?

Fig 6 x-axis label I don't understand units for delta (phi) % and ppb. Values differ by 2 orders of magnitude. If panel a is calculated for a 50% change in an individual hydrocarbon and panel b is calculated for a 1 ppb increase, this means that concentrations are of order 10 ppt, which does not seem reasonable.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10551, 2010.