

Interactive
Comment

Interactive comment on “Rates and regimes of photochemical ozone production over Central East China in June 2006: a box model analysis using comprehensive measurements of ozone precursors” by Y. Kanaya et al.

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Reply to the Editor’s comment:

Dear Authors: Two extensive reviews have been posted on this paper. Although there are a large number of questions, criticisms and comments in the reviews, both reviewers see enough merit in the paper and believe it should eventually be published in the ACP. I agree with them. Most of the questions, criticisms and comments are very perceptive and substantial. Furthermore, they are specific and constructive. I believe that

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a careful response to the points raised and a thorough revision of the manuscript would result in a much improved paper. Sincerely, - Shaw Liu

Thank you very much. We hope that we adequately answered to the comments and improved the manuscript accordingly. We thank the reviewers especially for the comments on the uncertainty in the NMHC concentrations and on the L_N/L(radical) analysis. The followings are the changes to be introduced to our revised manuscript.

1.page 1, line 23, page 16, line 19. A range is included: 58 ± 37 ppb

2.page 4, line 21. typically at 14:40 CST

3.page 6, lines 11-19. A new paragraph (as below) regarding the uncertainty in the NMHC concentrations is included, upon a comment from Referee 2. The uncertainty in the NMHC concentrations determined in these ways was roughly estimated to be a factor of 2. The benzene concentrations estimated by the method mentioned above showed similar variation patterns to those directly observed by PTR-MS after 12 June (See Figure S1 of the supplement material) and the central 80% of the ratio between the two concentrations was in a range of 0.80 – 1.67. Additionally, we found that 84% of the concentrations of important NMHCs (having large reactivity toward OH) estimated by the method above deviated from the directly measured concentrations by less than a factor of 2. The influence of the uncertainty on the estimation of the ozone production rate will be discussed later.

4.page 6, last line. “the summed reactivity of CO, CH₄, H₂, SO₂, hydrocarbons, and oxygenated VOCs” (H₂, SO₂, and oxygenated VOCs are added with the revision of Figure 1h)

5.page 7, lines 10-12. A new sentence is included upon revision of Figure 1h: In Figure 1h, the magnitudes of contributions of NMHCs and oxygenated VOCs to the reactivity of OH can be recognized.

6.page 8, line 4. “to calculate values in Table 2” is inserted.

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7. page 8, line 5. “almost in balance” was replaced by “in balance”

8. page 8, lines 15-17. (R1), (R2) and (R3) were replaced by (1), (2) and (3). The equation in page 14 is now labeled as (4).

9. page 8, lines 27-28, Ranges were added: 85 ± 43 and 60 ± 32 ppb

10. page 9, line 1. Ranges were added: 18 ± 5 and 11 ± 3 ppb

11. page 9, lines 3-4. 67 ± 41 and 49 ± 32 ppb, 58 ± 37 ppb

12. page 9, line 5: “slightly” is removed from “slightly larger” in the original manuscript. (see reply 4 to the referee 1)

13. page 9, line 10: “Our value is larger than the production rate (38 ppb day^{-1})” (see reply 4 to the referee 1)

14. page 9, lines 13-14. “The difference would be attributed to dilution and deposition, effective only for the rate estimated using the residence time.” (see reply 4 to the referee 1)

15. page 9, line 22. “in situ photochemistry is active enough to explain the ozone buildup” (see reply 6 to referee 1)

16. page 10, lines 7-11. The following sentences are inserted. The production rates for Mount Tai are significantly higher than the $F - D(\text{O}_3)$ values at other mountain stations: from -0.05 to $+0.4$ ppb h^{-1} at Jungfraujoch (3590 m a.s.l.) for midday in winter, spring, and summer (Parker et al., 2009; Zanis et al., 2003), $2-3$ ppb d^{-1} for Mt. Cimone (2165 m a.s.l.) in June (Fischer et al., 2003), and from -0.8 to -0.4 ppb d^{-1} for Mauna Loa (3.4 km a.s.l.) for four seasons (Cantrell et al., 1996).

17. page 10, line 30 – page 11, line 4. The sentences were revised upon referee 1’s comment 7. The analysis also implied that the ozone production should have been more efficient when the air mass was fresh and the NO_x/NO_y ratios (and thus the NO_x concentration for a given NO_y concentration for that air mass) was higher than that ob-

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served on the mountain (~ 0.18 as an average over the campaign period). Additionally, the importance of NO_x-limited regime indicates that the ozone pollution will be severer in the future owing to stronger NO_x emission anticipated in CEC (Yamaji et al., 2008; Ohara et al., 2007).

18. page 11, lines 5-8. A sentence regarding the impact of the uncertainty in NMHC concentrations on the estimation of the ozone production rate is inserted: Because of the dominance of the NO_x-limited condition, the uncertainty in the NMHC concentrations of a factor of 2 propagated to the uncertainty in the daily production of ozone (on the basis of F-D(O₃)) of a factor of only 1.15 (Figure 5a).

19. page 12, line 8. “H₂, SO₂,” was added.

20. page 12, lines 12-16. The explanation of Figure 7d is revised upon the change of the figure (see reply 5 to referee 2): “The fraction of the radical + NO_x reaction rates (LN) to the total radical termination rate (L(radical)) was greater than 0.5 for the VOC-limited data points (Figure 7d). In this case, the OH + NO₂ + M and XO₂ (peroxy radicals that account for additional NO to NO₂ conversion) + NO reactions contributed largely to LN.”

21. Figure 1. y-axis range for Figure 1a was modified from 0 – 150 ppb to from 40 – 150 ppb. Figure 1h now includes new series, showing the reactivity of CO, CH₄, H₂, and SO₂ shown for reference.

22. Figure 2. Tickmarks for the top and bottom axes in Figure 2a, 2b, 3a, 3b, 8a. and 8c are revised.

23. Figure 7d was replaced with one showing $\text{LN} / \text{L}(\text{radical})$ instead of $k[\text{OH}][\text{NO}_2] / \text{L}(\text{radical})$.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12965, 2009.

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