

Interactive comment on “Seasonal dependence of peroxy radical concentrations at a northern hemisphere marine boundary layer site during summer and winter: evidence for photochemical activity in winter” by Z. L. Fleming et al.

Z. L. Fleming et al.

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In reference to the referee’s general comments, it is recognised that the paper lacks some of the ancillary measurements. In a way the paper points to an interesting observation that is need of further more extensive observation and I hope that tone is struck in the paper.

The title has been changed to “Seasonal dependence of peroxy radical concentrations at a Northern hemisphere marine boundary layer site during summer and winter: Evidence for radical activity in winter”. The evidence was too circumstantial to state, though it is the most likely explanation, that it was NO₃ chemistry.

Section 2.2 (the experimental description of the PERCA) has been shortened and the reader referred to Fleming et al, 2006. Section 3.8 provides some comparison and literature context for the derived ozone production numbers and our preference is to keep it as it is. Removing elements of section 3.8 would provide an inconsistency with respect to information in Fleming et al, 2006. The conclusion section (4) has been shortened.

Specific comments

1. The abstract has been modified
2. The reactions have been added at the point indicated.
3. The referee is correct with respect to the mixed terminology, this has been corrected. The measurement uncertainty is 42%, this reflects an updated assessment over that presented in the earlier Fleming et al. (2006).
4. Text corrected to reflect this.
5. Text corrected to match correct numbers in Table 1
6. Requested text added on detection limit “between 0.2-1 pptv on a 30 minute average”
7. The gradients are 1.64 for winter and 0.22 for summer for $\text{NO}_x > 1$ ppbv. This has been added to the text.
8. We agree with the referee’s sentiment, hence our circumspection. There were no measurements of VOCs in both campaigns.
9. The text has been corrected to reflect this.
10. The data in Table 4 and Figure 10 have been cross-checked and Figure 10 replotted.
11. These data have been corrected to match Table 1.
12. There is no value for 27th January owing to a lack of all the required measurements to calculate ϕ .
13. The text highlighted was orphan text from a previous version of the paper, and has been removed.
14. Plotting the data in Figures 13a and 13b on a log-scale seems to provide no

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advantage.

15. The number should have been 0.1 ppbv not pptv.

16. The probable reason for different observations in the Weybourne 2002 data is different meteorological conditions giving a different prevailing chemical climatology.

17. The referee is strictly correct, in a sense it is twice as efficient, does not lead to twice the ozone production. As far as I can see the $P(O_3)$ does not vary as the square of the NO_x concentration.

18. The conclusion has been modified to make it more concise.

19. The tables have been corrected with respect to NO_x , NO and NO_2 . There was a glitch in the averaging software.

20. Table 2 has been corrected with respect to NO_x , NO and NO_2 , as per Table 1, there was a glitch in the averaging software. The O_3 was checked and corrected.

21. Table 3 has been corrected with respect to ozone

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