

Interactive comment on “Coastal zone production of IO precursors: A 2-dimensional study” by “L. J. Carpenter et al.”

L. J. Carpenter et al.

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We would like to thank the referee for extremely useful and constructive comments. As consequences of their suggestions, there have been several changes that we believe result in a more rigorous paper. The specific concerns are described below. Note that the page and line numbers have changed, however the replies below are in the same order as the referee's comments.

1) Terminology. We agree that "open ocean" is misleading and have replaced this term by "offshore" and "non-local". A statement has been added to the Conclusions to explain that the study only suggests emission from coastal waters, not the open ocean.

2) Variability in eddy-diffusivity. Variability in eddy diffusivity has now been included, according to the work of de Leeuw et al. (2001) during PARFORCE who examined the correlation between friction velocity and horizontal wind speed. The correlation was particularly good in marine sector air and we use their relationship $u^* = 0.09 \times$

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wind speed at 3m (Figure 6 in their paper) to define K_z . Incorporating the variability in K_z results in much better agreement between the modelled and measured CH₂I₂ concentrations using the "offshore only" flux scenario (see Section 3.3 and Figure 7), which is more sensitive to K_z variability than the tidal-only flux scenario (because of the longer fetch). So micrometeorological variability obviously plays a role in controlling the CH₂I₂ concentrations at Mace Head. This was not picked up before, but is now emphasized in the Abstract and Conclusions.

3) Internal Boundary Layer Formation. It is not possible to calculate the impact of IBL formation on our conclusions, however we provide a caveat regarding IBL formation on in Section 2.3, the Model Description, as suggested.

Minor detailed comments (all page and line numbers refer to the original manuscript):

1) P196 In 9. The box model is no longer included in the analysis (see point 9 below).

2) P199. A statement has been added as to the additional variability in roughness lengths due to tidal influence.

3) P200 In 8. We have modified the statement to "It is clear from Figure 1 that the maximum IO concentrations occurred during the day at low tide."

4) P200. The construction of Figure 2 is now described clearly. The reason for the higher values in Figure 1 is that the data points of Figure 2 are averages.

5) P200. The sentence has been modified.

6) P200 In 17. The Hebestreit work is no longer referred to, since the correlations are shown in the paper.

7) P200 In 24. The constant offset in Figure 4 shows evidence for tidal (local) sources, not non-tidal (i.e. not enough time for photolytic destruction to occur).

8) P200 In 26-27. Indeed there is not likely to be a good correlation between day-time [CH₂I₂] and TH, because the control of photolysis takes over. Only at nighttime is a good correlation with TH expected. A statement has been added to this affect for clarification.

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9) P201 In 8 and In 15 onwards. We agree that the use of a box model to infer a relationship between tidal height and flux is not appropriate. The relationship can be inferred purely from the observations. The entire descriptions of the box model methods and results have therefore been removed entirely. The relationship between tidal CH₂I₂ production and tidal height was assumed to have the same qualitative form as the night-time CH₂I₂ vs TH correlation (i.e. a linear negative correlation), when there is presumably no destruction of CH₂I₂, i.e. the correlation is driven by production processes only. Text has been added to explain this. The original statement stating an inverse flux was incorrect, the flux is parameterised in the form $\text{Flux} = A - B \cdot \text{TH}$. The text has been corrected.

10) P202 In 10. The vertical grid resolution has been changed from 10 m to 2 m, and the top of the box is now only 50m. The impact on the results is negligible.

11) P202 In 22. A different approach has been employed here. Rather than using a pair of fluxes to "match the measurements", and then independently verifying the fluxes employed, the low water tidal flux, as estimated from seaweed emissions, was used regardless of the match. The constant "offshore" flux was set to the level which reproduced the average observed concentrations of CH₂I₂. Text has been added to make it clear that it is not possible to estimate a meaningful value of the offshore flux without knowing the length of the fetch accurately. The offshore flux can only be used to show that there are non-local sources, but it is emphasised that putting a value on these non-local sources is not possible (although an upper limit based on a fetch of 5km, which was verified by measurement, is suggested). The high water flux used was the same as the offshore (non-local) flux. The combination of these fluxes was then used in the model. The agreement was analysed as is, with no attempt to "tweak" the fluxes to provide better agreement. We believe this represents a more rigorous approach. The degree of correlation between modelled and measured [CH₂I₂] is shown by the r^2 values of 0.89 (coastal + offshore), 0.78 (offshore only) and 0.75 (coastal only) - this has been included in the text. The overall levels of CH₂I₂ are somewhat overestimated by the model, however we make no attempt to estimate whether the offshore or tidal

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component of the flux should be lowered, but rather suggest that a contribution from both components is likely, with the tidal flux being several orders of magnitude higher. (see Conclusions).

12) P203 In 2. The average concentration was a mean value of 6 discrete measurements, taken during the 2 weeks after the atmospheric measurements. Text has been added to this affect.

13) P203 In 5. The quoted Henry's Law coefficient was a measured value.

14) Text has been added to the caption to Figure 4 to explain the data points and error bars.

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