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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Air-sea fluxes of methanol, acetone, acetaldehyde, isoprene and DMS from a Norwegianfjord following a phytoplankton bloom in a mesocosm experiment" *by* V. Sinha et al.

Anonymous Referee #1

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I appreciate the careful replies that the authors have put together here, and would be pleased to see more emphasis and discussion on the potential controlling mechanisms for exchange rather than reporting flux values.

A simple emission factor expressed in ng/m3 appearing within an abstract of a good journal like ACP will rapidly become gospel and find its way quickly, and without necessarily a great deal scrutiny of the uncertainties or assumptions behind it, into atmospheric models. The authors seem willing to accept that the emission value in itself obtained here may not be indicative of the real world since there is essentially no wind

- so whilst it is a should be reported, it should be given much less prominence as a finding from this work.

There remain a couple of items which in my mind need further clarification.

It is very useful that they have shown through use of Henrys law coefficients the effects of temperature on the partitioning. Perhaps what I didn't make clear in my review, is that it is the potential internal temperature changes inside the mesocosm that may be significant here and in turn be caused by sunlight (and hence show covariance with PAR). A greenhouse (which is effectively what this mesocosm is) can show a huge variation in internal temperature when compared to external temperature depending on the radiation it is exposed to. When overcast then the author's suggestion that ambient air temperature (which they have data for) approximates to internal air temperature may well be valid, but without having data to back this for days with direct solar irradiation (high PAR) I remain a sceptic. If some simple data showing no difference in internal mesocosm temperature between sunny (high PAR) and cloudy (low PAR) days could back up their assertion, then I could become converted to their hypothesis. Without this key data I still find that the issue of temperature hangs over this work as a major uncertainy.

The second point I would be keen to see explored further is whether the addition of an aquarium pump is really going to prevent the formation of a microlayer. My understanding of the physics of sea surface tension is that even under remarkably turbulent conditions the discrete layer exists between bulk and air above, and that chemical exchange between these liquid zones is not rapid. The action of bubble breaking can effectively destroy this layer, but am I right in guessing that the chamber here is stirred not bubbled?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9907, 2006.

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