

Interactive
Comment

Interactive comment on “Meteorological constraints on oceanic halocarbons above the Peruvian Upwelling” by S. Fuhlbrügge et al.

Anonymous Referee #2

Received and published: 13 October 2015

I have reviewed the manuscript, “Meteorological constraints on oceanic halocarbons above the Peruvian Upwelling”, by S. Fuhlbrügge et al. and find it in need of moderate revision before publication.

General Comments:

The manuscript is reasonably well written but at times a little difficult to follow, owing in part to acronyms and repeated recitation of numbers, but also to text that seems to ramble without a clear focus in some places. The sampling approach appears sound, the data look to be good, and the approach is interesting. I’m wondering, however, if the authors are not letting the “trees get in the way of seeing the forest”, in that in some places the detail is killing the main message.

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Using a largely meteorological approach, the authors make the case that the ocean makes only a small contribution to the amount of gas in the boundary layer immediately above it. While that may be true for any of the 400 m² boxes they use, I'm not so certain it is true for Peruvian upwelling altogether. At least, that has not been made clear to me. The point is made that most of the halocarbons over a given area are advected in, but it belies the possibility (probability?) that any elevation of concentration in a particular box coming from upwind portions of the upwelling zone contains air that has already been impacted by oceanic emissions. One could walk away from this paper thinking that the emissions of short lived halocarbons in upwelling regions like Peru are not that significant when, based on comparison with boundary layer burdens over the open ocean, it seems that they are. This is not to say that the analysis in this paper is not useful, but it does need to be put in perspective. The authors also need to discuss the tenuous nature of their assumption of steady state in a dynamic boundary layer and its implications to their conclusions.

Finally, while the authors do address the uncertainties of ocean concentrations and atmospheric mixing ratios, I'm concerned about the uncertainties introduced by the modeled components, e.g., advection, degradation, air-sea exchange, and how they might impact the authors' conclusions. These seem to be ignored. The authors should at least discuss this, if not address it quantitatively.

Specific Comments:

1. The repeated use of similar acronyms requires that the reader keep looking back at the text. This could be helped considerably with a labeled diagram of the boundary layer box and its fluxes over the 400 m² ocean surface. 2. P. 20599, line 1, replace "more" with "other" 3. P. 20600, line 13-14, delete "respectively" 4. P. 20600, lines 15-19, delete all 5. P. 20600, line 23, replace "ships" with "ship" 6. P. 20601, line 21, replace "from" with "near" 7. P. 20603, line 3, how high is the 5th superstructure, what obstacles surround it, etc.? 8. P. 20603, line 11, define "moon pool" 9. P. 20604, lines 10,11, remove sentence, place refs at end of previous sentence. 10. P. 20604,

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lines 22, 23, move “from the ocean surface to right after “launched”; make “positions” singular. 11. P. 20606, line 1, replace “relating” with “ratioing”; replace “receive” with “estimate”. 12. P. 20606, line 3, delete “respectively” 13. P. 20606, lines 1-5. This part is confusing and a good place where the diagram could be put to use. 14. P. 20606, line 23, how much of a temperature drop was it to get to 18 degrees? 15. P. 20607, lines 1-10, Could this be reduced to a simple sentence? 16. P. 20607, lines 12-18, Is this paragraph necessary? Similarly, is the next one necessary? Can the authors simply make a few statements about the meaning of these concentrations, how they relate to other areas, and how they might be useful to support their conclusions? Numbers are best placed in tables so the authors can refer to them and keep the text focused on the issues at hand. 17. P. 20608, lines 23-25, This is a strange statement in that it is obvious. MBL concentrations are always a function of air-sea differences, in situ loss, and advection. The sentence after this is equally unnecessary. 18. P. 20609, all, This tutorial discussion could be condensed to a few sentences, in my mind. If the authors are trying to explain the 15-16 December anomaly, they should focus on that, not ramble through all the rest. 19. P. 20610, lines 9-16, This is good and relevant. 20. P. 20610, lines 25 ff, This is where the authors need to insert some perspective as discussed under General Comments. Also, how much of the discussion on the following page is relevant to their main point? 21. P. 20611-12, Section 3.5. Numbers are getting in the way of the points the authors need to make. There is a table of correlation coefficients. The authors should make the important points and simply refer to the table. 22. P. 20612-20614, Section 4.0, This is where an overall perspective of upwelling contributions and an understanding of overall uncertainties would really help. The authors make some good points here; I'd just like to see them better substantiated. 23. P. 20614-20616, too many numbers here. This should be a summary of the main points the authors are trying to make with their data, where the gaps in our understanding still are, what should be done to remedy those gaps, and why. The summary should be much shorter.

** Although I chose "major revisions" above, I think we're looking at "moderate"

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revisions – more than minor, less than major.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C7888/2015/acpd-15-C7888-2015-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20597, 2015.

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