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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Emission of volatile halogenated organic compounds over various landforms at the Dead Sea" *by* Moshe Shechner et al.

Anonymous Referee #1

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This is a study of concentrations of short-lived halogenated gases from a unique area, the Dead Sea. Given the interest in these chemicals and the uniqueness of this location, this paper has the potential to be an interesting contribution. It certainly includes a thorough review of the available literature and the authors have very thoroughly considered their new results in light of previously published work. However, I'm concerned about a number of aspects of the interpretation of the measurements, which are described below. The most significant is an inadequate consideration of uncertainties in most aspects of the work. This leads to an extended discussion throughout the paper of effects that I'm not convinced are real.

In Table 2, comparisons are made between concentrations measured at these Dead Sea sites with reported concentrations in the marine boundary layer (MBL) (as medians, from Ozone Assessment Reports), and measured concentrations enhancements are taken to imply significant local emissions. But this seems an inappropriate conclusion. I would expect that the influence of the marine boundary layer on what is being measured in the Dead Sea valley is diminished by the time air moves from any distant sea (Red or Mediterranean) to this valley owing to vertical mixing within the lower atmosphere. Perhaps instead, any enhancement relative to the MBL suggests only that fluxes are non-zero in this region too, and are perhaps comparable (or larger) than suggested for the marine boundary layer and coastal ecosystems? Drawing conclusions from concentration differences in the Dead Sea area vs the MBL is tricky and not especially informative, given that concentrations are influenced by dynamics in addition to flux–this seems worth mentioning, but isn't yet in this regard. Also, why aren't MBL fluxes also shown in Figure 2?

I also find it very difficult to internalize the information given in Table 2 as presented. I'd recommend the presentation of these results, if retained, also (or instead) as a figure. Furthermore, I'd suggest that any enhancement factor should also consider the reported range in the marine boundary layer concentrations so that the reader can better understand the degree to which the Dead Sea region concentrations actually are anomalous (regardless of reason, flux or meteorology). In addition, for some of these gases there are some well documented temporal, seasonal, and latitudinal variations in MBL concentrations that aren't well considered by the "annual average for 2012". As a result, I suspect that some of the EF's (CH3Br, perhaps also CH3Cl, CHCl3, and CH2Br2) are not accurate representations.

On fluxes, the text seems to inaccurately reflect what the figure indicates once uncertainties are considered. One example: "Figure 2 presents the measured fluxes of all VHOCs studied. On average, the net fluxes of all measured species, except C2HCl3 and CH3I, were positive at most of the investigated sites", and my review of Figure

2 indicates a much lower occurrence of positive flux: only 13 of 36 panels (excluding CH3I and C2HCl3) show positive fluxes where the standard error does not encompass zero. Another example can be found in section 3.2.1, lines 417-419. It is necessary to consider the uncertainty on the average here in drawing conclusions. Furthermore, I would estimate that the standard errors are likely underestimated as a result of the fairly small number of measurements used to estimate fluxes in this work.

Are the fluxes actually associated with the ecosystems purportedly sampled by this technique? Inferences about flux from measurements as a function of height have a certain spatial influence function. Please indicate what that might be for the sampling heights you have chosen. Consideration of the C2HCl3 results (an implied sink, perhaps from elevated mixing ratios in the broader Red Sea region) may indicate that the fluxes you are deriving here for naturally-emitted gases are actually not representative of the local regions you intended them to represent. How is the reader to assess this? Also, what has determined the different heights at which samples were collected on these masts? Sampling heights in a region with local emissions should have a large, but not discussed, impact on measured mixing ratios–which are being compared among sites and to MBL results.

I find the results in Figure 3 intriguing, although not much is made of it in the text. While it may be that no generalizations are possible related to all gases, there are some interesting similarities that might be worth discussing, especially to understand if these co-variations are consistent with the discussions related to co-variations in fluxes as what was intended in Table 4. Table 4 is also very hard to extract information from... and as before, I'm concerned that any identification of positive flux amounts don't take into account uncertainties on those estimations. If uncertainties were not considered, then it seems that much of the discussion related to incidences of positive flux and rankings by chemical etc. that follows should be reconsidered.

Table 5 and 6 need a consideration of correlations that are and are not significant, given the number of measurements included in each determination. Given the small

number of samples considered here, I'd estimate that correlations of <0.1 are in fact indicative of no evidence for a correlation, not a correlation described as "low".

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