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Interactive comment on "Large methyl halide emissions from south Texas salt marshes" *by* R. C. Rhew et al.

Anonymous Referee #2

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The paper is well written and, apart from some small technical deficiencies, should proceed to publication. The corrections/amendments I would suggest are:

1. The authors suggest that depletion of bromine and chlorine in *Batis maritima* could explain the diurnal changes in methyl bromide to methyl chloride emission ratios from this plant species. It seems reasonable that this will only have a significant effect on emission ratios if the amount of emitted bromine and chlorine makes up a large proportion of these stores. This should be testable to some extent: The authors could, if they have the resources and equipment, measure the chlorine and bromine content of *Batis maritima* tissues or as an easier way they can use literature values. The chlorine and bromine content values for *Salicornia europea* published in Blei *et al.* 2010b seem suitable, but the authors should look

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also for other sources as well.

2. One sticking point in the methodology is the relative small number of measurements. As the diurnal studies were carried out only on two locations three times over the course of a year there is a good chance that other influences such as changes in the influence of salt water vs rainwater, unusual cold and anything else could easily skew any findings in regards to annual emission patterns.

3. The level of uncertainty of methyl bromide fluxes presented in **Figure 2** is very large compared to the measured changes in these fluxes. Either the statistics you present with the error bars is overly conservative or the information extracted from these measurements has little meaning. It seems difficult to have confidence in a cosine function if a straight line would fit these data just as well (when taking the uncertainty range into account).

4. Enclosure times of 16 to 30 minutes seem quite long. From previous experience I know that the concentration build-up inside the chamber can heavily skew the flux data to appear lower than they really are. This would be even more of a concern at such high emission levels. Could you outline (either in the publication or as an answer for the referee's benefit) how you derived the fluxes at time "0"?

5. In the first paragraph on page 9459 the authors discuss the possible effect of local leaf temperatures on emission rates in transparent chambers. It would be helpful to know what the leaf temperatures of naturally insolated *Batis maritima* vegetation growing outside of a chamber is. Afterall this would be the natural state of a plant and would be valuable information for possible modelling efforts.

6. Without wanting to go into a discussion on the merits of transparent vs opaque chambers I would like to query how the authors on page 9462 suggest that higher photosynthetic rates might lead to higher concentrations of secondary metabolites. With little doubt higher insolation will generally lead to higher biomass yields

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and therefore have an indirect positive effect on emissions. However, the data published here are from dark chambers which cannot prove or disprove that secondary metabolites derived from photosynthesis might not directly affect emissions as there is no photosynthesis in opaque chambers.

I hope that these suggestions seem reasonable and are easily incorporated.

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