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

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We appreciate the thoughtful and supportive review by Reviewer #1, who summarized the key findings and context of the manuscript concisely. We address first the general critique provided by Reviewer #1 regarding the limitations to the dataset, and then we address the 4 more specific comments below.

The reviewer notes that the number of field site visits were limited (the 62 flux measurements were from five field outings) and spread out over a period of several years. We acknowledge that higher frequency sampling over a shorter period of time would have provided a more continuous and complete picture of the seasonal trends in emissions. The sampling strategy in this study was a balance between addressing specific scientific questions and working within logistical constraints. The goals of the sam-

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pling strategy were at first to provide a survey of emissions from dominant plants and macroalgae, and then to characterize the diurnal cycle of emissions from the dominant emitting plant (Batis maritima). The key logistical constraint was that this project did not have any dedicated funding associated with it, and the field outings were undertaken when the collaborators could generate enough resources and time to conduct the measurements. The field site was nearly 3000 km away from the laboratory where the air samples were measured.

We ruefully acknowledge the tardiness in the preparation of this manuscript, when the last set of field measurements were undertaken over 4 years ago. In the intervening time, though, it became clearer how this work fit in the context of the rest of the literature on terrestrial methyl halide emissions, and we hope this discussion enhances its value.

Specific comments:

(1) Section 3: It is stated that the temperature inside the enclosures were measured but results are discussed in terms of outside ambient temperature. Would it not be appropriate to examine relationships with the actual temperature experienced within the enclosure?

Emission rates actually are compared with chamber air temperature, not ambient temperature (see captions in Figures 1 and S1). An omission in our text is likely the source of this confusion. We have now changed 'air temperature' to 'chamber air temperature' in the second paragraph of the discussion.

(2) P9457, L17 and P9458, L16: In the Discussion it is stated that molar ratio between CH3Cl and CH3Br emissions is $\sim$15 on average, whilst in section 4.3 it is stated that the molar ratio is roughly 40 during the day and 20 at night. The statements in the two sections do not appear to be consistent with each other.

The linear regression of CH3Cl vs CH3Br fluxes has a slope of 15, but that allows a non-zero y-intercept. This slope would be greater if forced through zero (i.e., a

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weighted average of the ratios). Because the purpose of the linear regression was to show correlation, we removed the non-essential slope value from the text. We agree that it was unclear.

(3) P9459, L10-13: Two sentences seem to repeat here. The end of the first sentence indicates that the CH3Cl:CH3Br molar flux ratio is lower at higher latitude salt marshes, and the next sentence says the same thing. Please reword appropriately.

We have removed the redundant '(higher latitudes)' from the second sentence.

(4) Technical p. 9459, L12: The in-text citation here should read Blei et al. (2010b). P9463, L13: The University of Texas MSI contribution number is missing.

These revisions will be incorporated. The MSI contribution # (1681) is added.

Interactive comment on Biogeosciences Discuss., 11, 9451, 2014.