We thank the associate editor, Dr. Rhew, for his review of our responses and for inviting a correction to our article. After reading the comment copied below in italics, we agree that he caught an oversight of ours. Our response is below the comment.

The response to a comment by Reviewer 1 brought to light the possibility of a slight bias in the methane production calculation. The reviewer writes: "L232: I seem to be missing how the methane production rates were calculated? According to the methods methane development was only measured in the headspace of the incubation vials. Although this isn't wrong, it doesn't account for the methane that is still in the sediment slurry. Are the rates reported here taking that portion of methane still in the slurry. This maybe small compared to the headspace but one could be underestimating the methane production rates. This should be clearly stated in the methods and also discussed." The authors respond: "The reviewer is correct to point out that we did not fully explain how dissolved methane was accounted for. Thank you for pointing out this oversight. We created standard bottles with the same gas-to-liquid ratio but with DI water in the liquid phase instead of slurry so that dissolved methane is accounted for in our calibration curve. This has now been explained more clearly in methods section 2.5."

According to the added text, the incubations and methane standards were both prepared in 125 mL serum bottles, with 75 mL headspace. The difference was in the 50 mL solution: whereas the incubations had 50 mL of a slurry containing 1:3 v/v ratio of homogenized marsh sediment to artificial sulfate-free seawater, the methane standards had 50 mL distilled water.

Methane solubility in distilled H2O may differ from that in a saline slurry. For a given temperature, the solubility of methane in distilled H2O is greater than in seawater (e.g., Yamamoto et al, 1976, Solubility of methane in distilled water and seawater, J. Chem and Eng. Data, 21, 1, p 78-80). Perhaps the solubility of methane in distilled H2O is similarly greater than in a saline sediment slurry. If so, then the incubation headspace concentrations calibrated to the distilled H2O-filled standards would yield an estimate of total methane that is too high (rather than too low if there was no liquid at all). For example, for a distilled H2O bottle, say 96% stays in the headspace and 4% goes into the solution. For the incubation, say 98% stays in the headspace and 2% go into the solution. Measuring the headspace of the incubation based on the distilled H2O calibration curve would then yield a total CH4 estimate of ~102% of actual. The actual correction factor may be trivial compared to other experimental uncertainties, but a potential bias should be acknowledged somewhere, even if briefly.

We do acknowledge the fact that distilled water will not have the same solubility as seawater or our seawater/sediment slurry, although we do expect this difference to be

small relative to other sources of variability in our experiments. We do agree it should be addressed in the text and have added the following sentence to the methods description for methane measurements:

“Although the solubility of methane in our slurries will not be the same as in distilled water, we expect these differences to be smaller than other sources of uncertainty and variability in our incubations.”