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Discussions

Interactive comment on "Regulators of coastal wetland methane production and responses to simulated global change" *by* Carmella Vizza et al.

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My sense from reading your responses to my comments is that there is in part a "language" issue, perhaps related to the difference between someone used to looking at methane cycling in the marine environment rather than the freshwater environment. For example although methanogens and sulfate reducers may directly compete for substrates, typically marine scientists assume that sulfate reducers will win this competition to the extent that methane production is VERY low in the presence of sulfate reducers. So technically, yes, they do coexist, but the whole idea is that one can effectively ignore methanogens until sulfate is depleted (and this ignores the possible presence of consortia where the sulfate reducers are acting as methane oxidizers). Similarly, nitrate reduction is assumed to go to completion before sulfate reduction kicks in. This is true even if nitrate levels are orders of magnitude lower than sulfate

levels (commonly true in marine systems). Your data do not really permit assessing this paradigm since there are no profiles and samples are homogenized bulk samples. Pooled concentration data really can't be used in situations where gradients are large and microenvironments exist as the microorganisms respond to the exact chemistry of their specific environment, not to pooled values. Similarly the use of pooled samples for genetic work confuses things, as the various subsamples being pooled could have different chemistries. (I also think it is confusing in the paper whether you are reporting data from sediments or overlying water column. Concentrations of all redox species will change rapidly below the sediment/water interface and be strongly effected by periodic exposure to air.)

I know that it is common practice to purge bottles with nitrogen to get methane production rates, but that does not mean that rates measured in purged bottles give you in situ rates.

To me sea level rise implies higher water levels implying inundation of wetlands, raising both water levels and sulfate concentrations. It was unclear from your paper the extent to which the wetlands you sampled were continuously submerged or whether tidal influences exposed the wetlands at low tide. I assumed from your figure that the wetlands were exposed meaning that at least some of the surface sediments periodically had oxygen added. How far into currently purely freshwater wetlands will the sea advance with sealevel change? This is likely to have a bigger effect (and totally change the ecology of the plants as well).

I don't think it makes sense to say that because acetate correlated with your results that hydrogen is necessarily unimportant. Perhaps if you had included hydrogen data these modelling results would have been different? In this, and a number of other places, more careful language would at least convince the reader that you had considered an issue.

I apologize but I don't have the time to go through the remainder of your responses in detail, and in any case it is hard to tell if you have appropriately modified the text without actually seeing it. I still feel that the paper does NOT tell us a lot about the effect of sealevel rise or climate change (actually temperature increase), but does contribute to an understanding of how increasing sulfate concentrations might change methane production (but not methane or carbon dioxide flux to the atmosphere)

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