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Interactive comment on "Evidence for microbial iron reduction in the methanogenic sediments of the oligotrophic SE Mediterranean continental shelf" *by* Hanni Vigderovich et al.

Anonymous Referee #1

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General comments: This manuscript reports the first evidence for microbial iron reduction in a methanogenic zone in an oligotrophic shallow marine environment. The subject matter is within the scope of BG. The title is appropriate, the abstract is concise and complete, and the paper is well structured. Below I highlight comments in order to further strengthen the manuscript: 1) More information is needed about the rationale for the incubation experimental design. There is no mention in the methods of how the killed-control incubations were "killed". Was Fe added to the incubations? Was this before or after "killing"? How are the authors certain that the killing mechanism did not change the Fe(III) mineralogy? Please also clarify why hematite and magnetite were added instead of the more commonly used ferrihydrite and goethite; would these forms

of iron oxides be more environmentally relevant in the methanogenic zone? Please state explicitly with citations if that is the rationale. Please provide citations for hydrogen utilization as an electron donor for hematite and magnetite reduction to justify this experiment. 2) The data for the Fe extracts seem to be missing. The methods state (lines 171-172): "The different reactive iron oxides were separated to (1) carbonate associated Fe; (2) easily reducible oxides; (3) reducible oxides and (4) magnetite." Yet the only data for iron speciation has all four of these clumped together. It would be much more informative to know the depth profiles of the four individual species instead of the sum. Why wasn't this reported?

Specific comments: Line 86-90: also shown in anoxic ferruginous lake sediment enrichments, Bray et al 2018 Geobiology "Shifting microbial communities sustain multi-year iron reduction and methanogenesis in ferruginous sediment incubation" Line 248: therefore *are* not discussed... Line 333: regardless *of* the area's... Line 342: "The H2 levels at stations SG-1 and PC-3 (Fig. 1) are relatively high (Lilley et al., 1982; Novelli et al., 1987), suggesting that there is enough H2 to sustain the iron reduction process." Need to clarify that H2 is two orders of magnitude higher at SG-1 vs. PG-3 and implications of that finding in relation to other findings. More text is needed explaining the references. Are these cited to show that H2 has been historically high at the site? Need a reference for the second half of the statement about the H2 concentrations needed for reduction of these Fe(III) minerals. Line 351-357: any evidence for or against this hypothesis in this study? Line 396: positive or negative correlation? Need to cite that supp material where this data is shown. Figure 1: Add zeroes before the decimal place on the x-axis. Increase the interval if it doesn't fit as is. Figure 2 and Figure S1: Move these to be included in Fig 1. Would enable easier comparison to have all the data together and will fit on a full-page graph oriented horizontally on page. Please add zones 1, 2, 3 like in Fig 1 to data in Figure 2 and Figure S1. Could move PG-5 depth profiles to supplementary. Figure 3: This color scheme and stretched out horizontal lines with white gaps in between is extremely hard to look at. Please fix to make it easier to see. Figure 5: I don't see a clear inverse relationship like the figure caption states. I think you can say that there is more variability in methane at low Fe(II) and that methane is low for the few data points at the highest Fe(II).

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