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Interactive comment on "Methane production by three widespread marine phytoplankton species: release rates, precursor compounds, and relevance for the environment" *by* Thomas Klintzsch et al.

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This paper presents an interesting discussion about the importance of methane production by several species of algae under aerobic conditions in the ocean. The authors' experiments are original and convincing but I think they overstate (or ignore) the extent to which this process can result in methane excess concentrations in open ocean surface water. In turn the minor role of that excess production to the atmospheric methane budget is not clearly explained. Below are some substantive criticisms and some minor corrections.

Line 17: The abstract indicates that the importance of oceanic methane production to the global methane budget is unknown but this is not discussed further in article and is misleading in any case since the ocean is known to be a very small contributor to the atmosphere. I am tired of proposals and papers that use the atmospheric methane budget to justify all studies of basic methane geochemistry. Surely it is enough to note a widespread and unexplained phenomenon which one is trying to explain mechanistically. I suggest adding a sentence or two to the introduction indicating why you are bothering to do this study and de-emphasizing how it might affect global methane budget. You are better off being straightforward and admitting that the real question is that methane is known to be produced in the oxic oceanic mixed layer and after more than 40 years no one really understands why. Give some idea of what actual flux of methane to atmosphere from ocean is thought to be. This HAS been calculated a number of times.

Line 98: Were cultures axenic? How was this determined? Sterile technique is not enough if bacteria are intrinsic to algal cultures. Bob Guillard told me this when I was using his culture collection. I personally don't think that there are anaerobic bacteria producing methane in rapidly photosynthesizing cultures, but one should be accurate.

Line 115: When calculating the amount of methane produced, was fraction dissolved included? With a large headspace, this may be small but should be mentioned. Were samples equilibrated with headspace before methane measured? The authors mention that oxygen was sometimes supersaturated, but was this relative to headspace or equilibration with ambient air?

Line 133: Concentrations (final) of added substrates should be given for comparison with natural concentrations. If possible give concentrations of these substrates in medium at start of incubation with and without addition of substrate.

Line 327: If the labelled methyl groups yield only a small percentage (less than 1%) of total methane produced where is the other methane coming from? Is this result consistent with field observations that show only a weak link if any between DMS or DMSO and excess methane in surface water? This point needs more elaboration since the question of the source of excess methane in seawater has been plagued by studies that show methane can be produced by a process but that rates are far lower than are needed to explain natural surface water values. Here is where a link to ambient DMS, DMSO or MSO concentrations should be made. I think this point is a key issue.

Line 400: Weller et al may have found a correlation between chlorophyll a and methane concentrations but there were many studies in the older literature (1970s and 80s) where no such correlation was observed. I recommend authors go back and read over some of these earlier papers and confirm that measured production rates from this study can support other previously observed methane fluxes. Also see thesis by Scranton (1977) where methane production was examined in cultures by several species including Emiliani huxleyi (called Coccolithus huxleyi in my thesis) and T. pseudonana. I observed methane production in a much less sophisticated experimental setup and concluded that natural populations of the algae I studied might be adequate to support the widespread supersaturations of methane seen in the open ocean (including in places where no dense algal blooms were observed). Perhaps your results can be compared to mine or to other studies that report cell abundances and air-sea fluxes. A citation to a downloadable copy of my thesis is below. Scranton MI (1977) The marine geochemistry of methane. Citable URI https://hdl.handle.net/1912/1616. DOI10.1575/1912/1616

Minor issues: Equation 7: There should be a factor of 1000 to convert ratios to per mille values Figure 1: Plot control values here too. Line 268: Should it be “were applied” not “where applied”? Line 308: Inoculation OF cells?

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