

Interactive comment on “Methane production, consumption and its carbon isotope ratios in the Southern Ocean during the austral summer” by N. Boontanon et al.

Anonymous Referee #1

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“Methane production, consumption and its carbon isotope ratios in the Southern Ocean during the austral summer” by Boontanon et al., Biogeosciences Discussions, 7, 7207–7225, 2010; doi: 10.5194/bgd-7-7202-2010.

General comments

Oceanic emissions of CH₄ represent only a minor contribution to the overall atmospheric CH₄ budget, however, data about the CH₄ distribution in the ocean are rare and thus emissions estimates are associated with a high degree of uncertainty. Moreover, the major formation mechanisms of oceanic CH₄ are not well-known. The CH₄ conc. and isotope ratio data set from the Southern Ocean presented is novel and might

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help to shed more light on the oceanic pathways of CH₄. Unfortunately, the data are not well presented, cited literature is outdated, some statements are heavily misleading and partly wrong and the main conclusions are not justified by the results. Therefore, I cannot recommend a publication in Biogeosciences.

Specific comments

Introduction: 1) Cited literature is outdated! Please refer to the latest IPCC report published 2007 and other more actual references. Atmospheric CH₄ conc. just started to increase again after several years of stagnation. (see e.g., Rigby, M., et al. (2008), Renewed growth of atmospheric methane, Geophysical Research Letters, 35, L22805, doi: 10.1029/2008GL036037.) 2) “However, methane produced in marine environments also contributes to atmospheric greenhouse gas concentrations . . .” Yes, but, oceanic emissions only contribute about <2% to the overall CH₄ budget. This should be mentioned, see IPCC 2007 report.

Material and Methods: 4) How many replicate samples have been taken? 5) How efficient is the stripping procedure? 6) I am missing a reasonable error estimate for the CH₄ conc. 7) For the calculation of the “atmospheric equilibrium conc. of CH₄”, I strongly recommend to use the mixing ratio from the AGAGE monitoring station at Cape Grim (Tasmania); see <http://agage.eas.gatech.edu/>

Results and Discussion: 8) First sentence: This is already a statement about the overall conclusion and, thus, should be removed here 9) Delta CH₄ is not defined 10) “As CH₄ is produced and/or oxidized by bacteria . . .”. This statement is partly wrong. CH₄ is exclusively produced by archaea. (See e.g. review by Ferry, J.G. (2010), How to make a living by exhaling methane, Annual Reviews in Microbiology, 64, 453–473.) 11) Page 7213: Indeed alternative CH₄ production pathways in the ocean have been discussed as well, e.g. zooplankton grazing (de Angelis and Lee, Limnol. & Oceanogr., 1994), from methyl phosphonate (Karl et al., Nature Geosci., 2008), from DMSP (Damm et al., Biogeosci., 2010) 12) Page 7216: Did the authors correct V (wind speed) for a height

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of 10m? 13) Page 7216: I am missing a detailed estimate and critical discussion of the uncertainties of both the CH₄ diffusion into the surface layer and the CH₄ emissions to the atmosphere. In order to compare both numbers one has to know the uncertainties. Otherwise the conclusions are only speculative at best and not justified. 14) It makes no sense to argue with an average air-sea exchange flux which is based on only three stations and shows such a high variability ($-0.09 - 0.74 \mu\text{mol m}^{-2} \text{d}^{-1}$) 15) Page 7216: "... global oceanic flux of 5-50 Tg yr⁻¹". This number is outdated. Please refer to the IPCC 2007 report or other actual references.

Conclusions: 16) "A subsurface CH₄ maximum was associated with the decomposition of sinking organic matter, suggesting a relationship between CH₄ production and plankton dynamics in the area". I am sorry, but the authors do not show any data to justify this statement. What about particle flux data? I could not find any data about plankton dynamics in the ms. 17) A basin wide extrapolation of the CH₄ emissions based on only three stations does not make any sense.

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