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Biogeosciences Discussions

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

N. Boontanon et al.

ennbt@mahidol.ac.th

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We thank the referee #1 for his/her very critical and valuable comments. Almost all of the suggestions done by him/her have been accepted as described in the following PTP response:

RC = Referee's Comments; AR = Authors' Response

Introduction:

RC #1) Cited literature is outdated!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 in-

crease again after seveal years of stagnation. (see e.g., Rigby, M., et al. (2008), Renowed growth of atmospheric methane, Geophysical Research Letters, 35, L22805, doi: 10.1029/2008GL036037.)

AR-1) We have agree with referee that our ref here is outdated. We will improve our manuscript with these suggested refs also latest data if possible.

RC #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.

AR-2) We agree that oceanic CH4 emission is less than 2%, however, we also would like to mention that oceanic CH4 emission could share some part of atmospheric CH4 due to high volume water bodies and high variation of redox conditions.

Material and Methods:

RC #4) How many replicate samples have been taken?

AR-4) The samples were taken duplicate, and the data set came from single analysis with confirmed by some duplicate samples.

RC #5) How efficient is the stripping procedure? 6) I am missing a reasonable error estimate for the CH4 conc.

AC-5,6) The efficiency of analytical experiment are well justified by compare with the working standard and atmospheric CH4 level. The stripping step is also working well, by observing of the second extraction of water sample. It shows the residual CH4 is remaining in the water less than 3%.

RC #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/

AC-7) We are agree to use the CH4 mixing ratio more closer and reliable as comment.

Results and Discussion:

RC #8) First sentence: This is already a statement about the overall conclusion and, thus, should to removed here

AC-8) This comment has been deleted.

RC #9) Delta CH4 is not defined

AC-9) The lack of delta CH4 explanations in the results are more clarify.

RC #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.)

AC-10) This statement will be improved by using the suggested ref.

RC #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)

AC-11) More discussion about CH4 production will be added to the manuscript follow those alternative pathways appear in the ocean.

RC #12) Page 7216: Did the authors correct V (wind speed) for a height of 10m?

AC-12) We used the wind speed data from the data set from *RV Tangaroa* with correction, the data collection point is approximately 10 m above sea surface.

RC #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.

AC-13) We are agreeing with the referee. We will discuss of the uncertainties of CH4 diffusion into the surface layer and the CH4 emissions to the atmosphere by using diffusing factors and also mass transfer.

RC #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 _mol mˆ-2 dˆ-1).

AC-14) Unfortunately, we had only 3 set of the data with some high variation. This result is based on our lack of data and rare information of CH4 in those areas.

RC #15) Page 7216: "... global oceanic flux of 5-50 Tg yrˆ-1". This number is outdated. Please refer to the IPCC 2007 report or other actual references.

AR-15) We have agree with referee that our ref here is outdated. We will improve our manuscript with these suggested refs also latest data if possible.

Conclusions:

RC #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 justiy this statement. What about particle flux data? I could not find any data about plankton dynamics in the ms.

AC-16) We are obtained only subsurface chlorophyll maximum (SCM) data which associated with maximum CH4 concentration layer, thus, such SCM layer, we are consider that it should related to plankton dynamics.

RC #17) A basin wide extrapolation of the CH4 emissions based on only three stations does not make any sense.

AC-17) We are agree, this estimation must be strongly remark that it is come from very

rare of data set.

Interactive comment on Biogeosciences Discuss., 7, 7207, 2010.