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## ***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.**

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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**RC #1)** Cited literature is outdated!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 CH<sub>4</sub> conc. just started to in-

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crease 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.)

**AR-1)** We have agreed with referee that our ref here is outdated.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 CH<sub>4</sub> budget. This should be mentioned, see IPCC 2007 report.

**AR-2)** We agree that oceanic CH<sub>4</sub> emission is less than 2%, however, we also would like to mention that oceanic CH<sub>4</sub> emission could share some part of atmospheric CH<sub>4</sub> 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 CH<sub>4</sub> conc.

**AC-5,6)** The efficiency of analytical experiment are well justified by compare with the working standard and atmospheric CH<sub>4</sub> level. The stripping step is also working well, by observing of the second extraction of water sample. It shows the residual CH<sub>4</sub> is remaining in the water less than 3%.

**RC #7)** For the calculation of the "atmospheric equilibrium conc. of CH<sub>4</sub>", I strongly recommend to use the mixing ratio from the AGAGE monitoring station at Cape Grim (Tasmania); see <http://agage.eas.gatech.edu/>

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**AC-7)** We are agree to use the CH<sub>4</sub> 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 CH<sub>4</sub> is not defined

**AC-9)** The lack of delta CH<sub>4</sub> explanations in the results are more clarify.

**RC #10)** “As CH<sub>4</sub> is produced and/or oxidized by bacteria ...”. This statement is partly wrong. CH<sub>4</sub> 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 CH<sub>4</sub> 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 CH<sub>4</sub> 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 CH<sub>4</sub> diffusion into the surface layer and the CH<sub>4</sub> emissions to the atmosphere. In order to compare both numbers one has to know the uncertainties.

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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 CH<sub>4</sub> diffusion into the surface layer and the CH<sub>4</sub> 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 ( $-0.09 - 0.74 \text{ mol m}^{-2} \text{ d}^{-1}$ ).

**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 CH<sub>4</sub> in those areas.

**RC #15)** Page 7216: "... global oceanic flux of  $5\text{-}50 \text{ Tg yr}^{-1}$ ". 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 CH<sub>4</sub> maximum was associated with the decomposition of sinking organic matter, suggesting a relationship between CH<sub>4</sub> 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.

**AC-16)** We are obtained only subsurface chlorophyll maximum (SCM) data which associated with maximum CH<sub>4</sub> concentration layer, thus, such SCM layer, we are consider that it should related to plankton dynamics.

**RC #17)** A basin wide extrapolation of the CH<sub>4</sub> 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

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rare of data set.

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

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