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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.

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Received and published: 1 February 2011

We thank the referee #2 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) Intro clearly shows a lack of new references. The atmospheric rise in concentrations

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with 1% per year stems from the 80th, and the references do not mention the reduced rise rates around the end of the millennium, nor the reoccurring higher rise rates since 2006, both of which has lead to several important publications over the last couple of years. The newest reference here is from 1990. From the entire publication list, 2 are younger than the data set, and my feeling is that an old manuscript or parts of a proposal have been reused without update here.

AR-1) We have agree with referee that our ref here is outdated.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.

Methods:

RC)The paper is based on three single water column profiles gathered in March 2002, of one which is rather nearshore (station 8). Possible effects of the vicinity to the land for Station 8 are however not discussed. Methane and methane stable isotopes are measured using a combination of a purge and trap system and measurement on an ir-gc-combustion-irMS method. The system seems to be really straightforward for isotopes, as the authors measure the stable carbon isotopic composition on really small samples, i.e. the oceanic background with concentration down to ~ 1 nM and stripping of only 125 ml of water.

AC) The influence of CH₄ or carbon substrates from the land are also important and possible to be the sources of CH₄ production. However, the result at st.8 show that CH₄ concentration is close to equilibrium which may results from low productivity and some CH₄ oxidation.

The analytical procedure using in this report 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) However, half of the interpretation is addressing the saturation state of the surface

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water, which thus is based only on 3 discrete water samples. These are very close to atmospheric equilibrium, and their interpretation and flux estimate is based on a residual oversaturation of 2%. This approach is not at all sound science. Again, one station is close to the shelf, and the accuracy the measurements are surely not allowing such an interpretation. This is evident alone from the fact that the uncertainty of the data and fit of the solubility coefficient in the original literature is not better than these 2%. This is why for the estimate of surface saturations close to equilibrium, state of the art method is the use of a water air equilibration system, where only the T-correction of the solubility coefficient is source of error in the equations. Re-estimating the atmospheric source strength of the Southern Ocean based on 3 surface values in one single year and season is not acceptable.

AC) We are agreeing with the referee. 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) Significance: Moreover, the result would still be of little importance, as the source strength is completely negligible in the global framework. Same is true for the isotopic fingerprint, where the authors report a value slightly heavier than atmospheric equilibrium, while the (more comprehensive) dataset of Heeschen et al. is slightly lighter

than the atmospheric value. In this regard, it has also to be mentioned that the authors refer to atmospheric value of 1,7 ppmV as reported in Quay et al. 1992 rather than looking up the atmospheric mole fraction in the southern hemisphere during the month of the investigation, which would be available from the NOAA/CMDL flask network. In brief, the update of atmospheric source strength is not state-of-the-art, not supported by the data, and not addressed in a scientifically sound way, and, even if believed, not of importance.

AC) We are agreeing to use the CH₄ mixing ratio more closely and reliable as comment in our calculation.

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RC) Interpretation: The other main point made in the manuscript is based on the interpretation of the isotopic values. From a slight positive excursion of the isotopic value in the subsurface maximum, the authors conclude that the source is isotopically heavy and thus, should be generated by acetate fermentation rather than carbonate reduction. While this might be true, the authors fail to discuss that in a lot of cases, the subsurface maximum has been reported in connection with a deviation to lighter isotopic values. Also, the methane released could be isotopically altered because of re-oxidation of a part of the methane produced in the middle of a particle during diffusion out of the particle.

Lastly, the postulated direct link between phytoplankton and subsurface methane production is not supported.

AC) We agree. This altered of CH₄ isotope could be occurring under production and diffusion out with some re-oxidation. Also, we are obtained 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. We have slightly rephrased the text, merely indicating this possibility along with the caveat mentioned by the referee.

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

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