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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.

Anonymous Referee #2

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Review of the Biogeosciences Discuss. Manuscript, 7, 7205-7225: Methane production, consumption and its carbon isotope ratios in the Southern Ocean during the austral summer; by Boontanon et al.

The manuscript by Boontanon et al seeks to shed some additional light on the role of the Southern Ocean on the methane cycle, in particular on its role as a source for atmospheric methane. Unfortunately, neither the data set nor the approach are fit for this approach. The referencing is not up to date, and the results of the 3 stations their paper is based on do not draw a coherent picture. The time of sampling is 8 years ago, and except for 2 references (one of them the paper by Heeschen et al, containing a very

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comprehensive data set of methane concentrations in the Weddell Sea), it appears as if this manuscript has been written quite a while ago.

I therefore only can recommend REJECTION OF THE MANUSCRIPT

Intro: Intro clearly shows a lack of new references. The atmospheric rise in concentrations 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.

Methods: 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.

However, half of the interpretation is addressing the saturation state of the surface 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-

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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.

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.

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.

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