Biogeosciences Discuss., 5, S149–S152, 2008
www.biogeosciences-discuss.net/5/S149/2008/

Biogeosciences Discussions

Interactive comment on "Methane emissions from the upwelling area off Mauritania (NW Africa)" *by* A. Kock et al.

Anonymous Referee #2

Received and published: 13 March 2008

General comments: Previous work has shown that coastal upwelling areas off the Arabian Sea and especially Southwest Africa can have rather high methane supersaturations. However, the importance of such upwellings is as yet uncertain, chiefly due to existing data gaps and poorly constrained variability. This paper by Kock et al reports methane emissions from the coastal upwelling areas off West Africa, and as such aids our understanding of the importance of coastal upwellings for global marine methane emissions. The paper reports high quality data from two cruises, which are summarised in clear figures. The discussion contains some further analysis of the effects of hydrography and upwelling, based on T-S diagrams and methane-SST relationship, and an estimate of annual emissions from the study area. The latter is rather brief and lacks both transparent description and critical discussion of the authors' approach,

particularly in terms of the spatio-temporal variability of the upwelling. The section describing the methane-SST 'correlation' is unclear, refers to a rather arbitrarily selected data subset and offers neither discussion nor justification of the selection criteria employed. These are my two main concerns with the manuscript. Overall, however, this is a good report that deserves eventual publication once the specific comments further below will have been addressed in detail.

Specific and editorial comments:

Study site description, seasonality of coastal upwelling.

This section provides some background on hydrography and briefly mentions that the upwelling is strongest in winter/spring and reaches furthest south in February. However, it lacks a more detailed discussion of both spatial extent and its seasonal variability, both crucial for the emission estimate presented in section 5. The authors should add this information, discuss how far offshore upwelling filaments do extend over the seasonal cycle, and how their sampling periods map onto seasonal changes.

Study site description, SACW, NACW.

I would greatly appreciate if the authors could give temperature and salinity ranges for the central water masses found in the study area. These ranges should also be indicated (e.g. as boxes) in figure 6 in order to facilitate comparison of their T-S data with water mass characteristics.

Elevated atmospheric mixing ratios, p 301, lines 8 ff

The authors suggest that these may be from coastal methane emissions. This might be one possibility. However, given that global methane emissions are dominated by continental sources these should not be dismissed so easily. Trajectories in figure 3 indicate that air masses should have experienced continental influence. Could they have passed over continental areas with elevated methane (see eg JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 112, D02304, Doi:10.1029/2006JD007268, 2007)?.

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Also, given the atmospheric lifetime of methane, it seems unlikely that any differences in atmospheric methane levels between the 2 cruises could be explained by 'atmospheric decomposition' during short term transport (see statement p 302, lines 4-6). This should be revised.

Figure 5 and text from p 302 ff, line 25 ff, SST vs methane and 'mixing line'.

As far as I understand, the authors divided their data into three subsets: (i) upwelled water masses during P348 with SST $<19^{\circ}$C and CH4 concentrations >3.9 nM, (ii) open ocean water masses with SST $>19^{\circ}$C, and (iii) other data that don't fall into these two categories. They then use 'water masses' (i) and (ii) to derive a 'mixing line' from a linear correlation of methane concentrations with SST. I have several issues with this approach. Firstly, the authors don't justify their SST and methane criteria. Why 19°C and 3.9 nM methane as cut-off points, and why exclude data that don't fall into categories (i) and (ii)? I also don't think that using methane concentration as a selection criterion is a valid approach: the aim of the study is to relate methane levels to indicators of upwelling that are independent of methane. This leaves SST as the indicator of upwelling. A simple correlation analysis applied to the whole data set would make therefore more sense. Secondly, the term 'mixing line' used here for the SST-methane relationship is misleading, because the relationship was derived by linear regression and not by construction of a mixing line between two well defined end members. Thirdly, the authors then go on to compare their 'upwelling efficiency with SST-methane relationships from previous work. However, they dont discuss the impact their data selection has on their own 'upwelling efficiency' nor do they discuss if previous reports applied similar selection criteria to exclude data subsets. As far as I can see Bange et al. (1998) did not. Also, the figure of -1.5 nM / K from Rehder et al. (2002), Figure 5, might be misleading. The entire data set in Rehder et al. (2002), Figure 7, shows a much wider range with 0.125 nM / K as its lower limit. This section needs to be rewritten and regression lines in Figure 5 changed accordingly.

P. 303, lines 10 ff, 'T-S lines'

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Please indicate temperature and salinity ranges of S/NACW as boxes in figure 6.

P. 304, lines 6 ff, Annual emission estimate

The authors' description of their approach is rather skimp, partly tucked away in foot notes to Table 1, with no justification of the 'assumed' seasonality and areal extent of the upwelling. Clearly these are important issues that require adequate treatment in the main body of the manuscript, with key assumptions backed up by published work or perhaps SST data from remote sensing etc. Again there is an issue with data selection that also requires transparent discussion: why do they use ALL their data with SST $<$ 19°C for their upwelling flux estimate, but exclude data with methane $<$ 3.9 nM from their upwelling subset in Figure 6? Data selection needs to be explained in more detail, and the resulting methane concentration figures need to be given.

Summary

The summary is repetitive and could be deleted in order to make room for the expanded discussion required to address the comments above.

End of review

Interactive comment on Biogeosciences Discuss., 5, 297, 2008.