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Interactive comment on "Response of the Black Sea methane budget to massive short-term submarine inputs of methane" *by* O. Schmale et al.

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When I posted my short comment, it was already clear to me that the authors do not intend "to review or compare the entire Black Sea literature addressing the subject of methane". It was also obvious that the present manuscript is "different from the one published by Kourtidis et al. (2006)"; if there was any misunderstanding on this part of my comment then I wish to state here that I did not wanted to imply that there is any dublication of work or that the focus of the two manuscripts is the same.

However, what I do believe is that there is some relevance of the Kourtidis et al. manuscript to the work presented here, and I will try to make my point clear below.

Kourtidis et al. present a number of scenarios for massive short term submarine inputs

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of methane; although their focus is on the subsequent atmospheric fate of methane, they use bubble modelling to estimate the amount of methane that reaches the atmosphere. It is a matter of taste, up to this point, to judge whether the work has any relevance to the one presented here.

D.F. McGinnis is co-author in both papers. In the Kourtidis et al paper he has formulated the methane bubble model, using the McGinnis et al. (2006) bubble model in combination with the modified (for deep water conditions) bubble plume model of Wuest et al. (1992). In the present paper, p. 9124, para. 3.2, Schmale et al state "we applied the gas bubble dissolution model of McGinnis et al (2006)". Hence I infer that this part of the modelling is the same in both manuscripts; then, Kourtidis et al. focus on atmospheric fate while Schmale et al focus on fate within the water column. Depsite the different focus, Kourtidis et al. make some claims about the amount of the methane that reaches the atmosphere that appear inconsistent with similar claims made in the present manuscript:

1A. Kourtidis et al., p. 5179, simulation of 16 000 000 g/s release at 2 km depth, resulting at 30% of the emitted methane reaching the atmosphere,

1B. Schmale et al., p. 9125, simulation of 179 Tg/1 day release at 2 km depth (which translates to around 2 000 000 000 g/s release), resulting to only a "2% increase in the sea/air methane flux", although the release rate is about 100 times higher than the one in 1A.

Since both papers use the same model of McGinnis (2006), I would expect that some commenting on the apparently differing results, and a clear statement on the possible differences in the McGinnis model as used in the two papers, would be obvious for any taste.

Also, since in the present paper (p. 9127, para 4, Conclusions and Outlook) the authors conclude that "massive short term injections of methane will be effectively buffered in the Black Sea water column.....emission across the sea/air interface is strongly re-

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duced", again a result not in line with the Kourtidis et al. results (as already mentioned in 1A above), I would again feel greatly facilitated in my interpretation by a clear statement on the possible differences in the McGinnis model as used in the two papers.

Finally, in p. 9121 of the present work, we are informed that for the study of the air-sea gas exchange the exchange model of Wanninkhof (1992) was used. Since other parametarisations also exist (Liss and Merlivat 1986, McGillis 2001), a reference to the results of Kourtidis et al. regarding the use of different parametarisations for the calculation of sea/air fluxes at the Black Sea might have been helpful for the authors and the readers to assess the impact of the different parametarisations in the presented results. This, of course, is again a matter of taste.

Interactive comment on Biogeosciences Discuss., 7, 9117, 2010.