Journal: BG
Title: Anaerobic oxidation of methane alters sediment records of sulfur, iron and phosphorus in the Black Sea
Author(s): M. Egger et al.
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General comments

This paper deals with diagenetic processes in the sediments of the Black Sea which changed from a lacustrine environment to a marine system. The work focuses on AOM and its effect on the linked species and processes under these changes. This was done by producing solid phase and porewater profiles, and by diagenetic modeling.

The work is well written and easy to read, and I found it complete, serious and convincing. The authors measured, calculated and thought on almost all the possible aspects that could affect this system during these changes. This careful work enhances our understanding on AOM by iron and sulfate in marine setting in general and specifically in a complex setting. It also provides us with new knowledge on the Black Sea's limnological history. I thus suggest accepting this work pending minor comments.

The main comments to the authors:

- The model is not detailed and explained enough. You should cite less Rooze et al 2016 and provide more details here. Also, you should perform sensitivity tests for the various uncertainties. I did not have access to Rooze et al paper, but from its title I am assuming it is not on the same system so there is no overlapping. You should however upload this paper.
- The Fe2+ increase in the deep sediments could be from deep Fe-AOM as we see in lakes and coastal sediments (Sivan et al., 2011; Egger et al., 2015), however it can also be organoclastic. There may be reactivation of less soluble Fe(III) minerals in this system by other means other than methane oxidation (e.g as described by Sivan et al., 2016). You indeed mention it, however, you should refer to it as a possible option.
- The assumption that the total dissolved Fe and Mn (as measured by AE-ICP) are Fe(II) and Mn(II) is probably fine, however you should test it and show it in at least in one of the profiles in the Black sea sediments (or cite other works that did it there). You should compare the Fe(II) to Fe(total) by another method (as the Ferrozine), or compare your assumed Fe(II) to Fe(II) from the Ferrozine or another method.
- You should discuss in more detail the sulfate profile and – its apparent “diffusion” profile (linear curve) with organoclastic sulfate reduction, and the cryptic S cycle

in the upper part of this profile. You should also compare the downward flux of sulfate to the SMTZ and the upward flux that you calculated for methane and discuss it.

- The $\delta^{13}C$ of methane similarity to Yoshinaga's data is convincing and satisfactory. Interpretation/speculations regarding the profile of δD of methane should be given.

Specific comments

- L 84-85: Vivanite was found also in Lake Kinneret (Sivan et al., 2011), it can support your finding and related processes.
- L 92-93: Also propose the other option for Fe reduction.
- L 141-142: See comment regarding this method above.
- L. 151: I assume the auto analyzer was based on IR. How did you remove of the sulfide?
- L. 236-242: Clarify and explain this part in more details.
- L. 288: Show how you calculated to this saturation value and under which salinity conditions. Mark this value on the figures of methane too.
- Add the bottom water values on the porewater profiles
- L 297: Change the sentence to a more precise one.
- L. 381: Explain the other 91% based on the profile (see main comment).
- L 445-451: See the main comment regarding organoclastic Fe reduction.
- L. 566-568: You don't need this trivial sentence, your work is good and nice enough without it ☺.
- Fig. 3: No sulfate measurements in the sapropel depths ? Add saturation of methane. What could be the reaction precipitating phosphate in the upper 300 cm (hydroxyl-apatite)?
- Fig. 4: Again with the saturation of methane.

Technical comments

- L 339: Start a new subchapter.