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Interactive comment on "Anaerobic methane oxidation in an East African great lake (Lake Kivu)" *by* Fleur A. E. Roland et al.

Anonymous Referee #1

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In general I find this manuscript OK but a little bit superficial. This also concerns the English writing which is not bad but could benefit from a native speaker. This lake is very complicated in respect to biogeochemistry due to its very high gas content in the deeper layers and this is known from a lot of publications that tried to understand e.g. the methane cycle. I think there was a very weak literature study done before writing this ms. e.g.: "In freshwaters environments, AOM has been less studied and is often considered as negligible compared to aerobic CH4 oxidation 45 due to lower SO42- concentrations than in seawater (Rudd et al., 1974). However, other potential electron acceptors for AOM, such as nitrate (NO3-), iron (Fe) and manganese (Mn) (Borrel et al., 2011;Cui et al., 2015). . ..." This is not true at all since we see now many more publications in this direction (see below for examples). It would be really good if the authors would read them and include them in their arguing. Especially one about

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methane oxidation in Lake Kivu or the one on closely located Lake Tanganjika should be interesting and included.

It would be good to put your work a little bit more in perspective of what has been done before. I also think that publications nowadays dealing with the understanding of methane oxidation whether it be aerobic or anaerobic are also looking at the organisms involved by all kinds of molecular tools and I find it a little bit sad that here only the geochemistry is looked at. The sampling strategy is with taking samples at a 5 m (or 2.5 m at best) sampling resolution not of what I think should be expected in a lake where geochemical processes are running on much shorter distances. Of course this is difficult with a Niskin bottle alone and other techniques should have been probably be used. If you read through the result section 3.2 you can really not see any trends it is just a description of how different the lake was during different seasons, month and years. So I really wonder how representative those measurements are. In general methane oxidation rates are also really high compared to other lakes Table 4 and to former measurements from Lake Kivu, are they correct? This is also questioned when reading: “For example, the maximum aerobic CH4 oxidation rate of 27 $\pm$ 2 μmol L-1 d-1 observed 235 at 55 m depth in August 2014 occurred at CH4 concentrations of 42 $\pm$ 2 μmol L-1.” This is unreasonable that the methane is turned over in two days. I think methane oxidation rates in this ms. are much too high. What can we learn from Table 5? It is just the measured concentration of the electron acceptors that might be used during CH4 oxidation but this does not say that this is also the case. This gives no information on what is really happening in the Lake Kivu and hence not very useful. “SO42- consumption rates were calculated from the change in time of SO42- concentrations measured with the nephelometric method, which might not be precise enough, since the detection limit was 52 μmol L-1.” What does this mean? Does this help to explain methane oxidation rates? The discussion between line 325 and 354 leads to nowhere. There is no real explanation why molybdate introduction would in one case enhance methane oxidation and on one hand reduce it. The competition explanation is pulled down also immediately after bringing it up. So what happens with

the molybdate? There are several wrong participles used which would also make a native English speaker necessary, e.g., “Samples for sulfide (HS-) concentrations were collected in 50 ml plastic vials, after being filtered on a 0.22 μm syringe filter.” Should read filtered through (several times. . .)

In general I do not see also after reading Morana et al. 2015 what is so new about this manuscript. It describes methane concentrations and compared them to possible electron acceptors that might oxidize it. Since sulfate is occurring in sufficient amount it is the most likely electron acceptor but this is not proved (I do not judge the incubations due to their very different results in different years and seasons to be of any proof) and also mentioned by Pasche et al. 2011. The only new statement is the difference between aerobic and anaerobic oxidation in dry and rain seasons but if this justifies publication is not on me to judge.

Suggested literature: Oswald, K., Milucka, J., Brand, A., Hach, P., Littman, S., Wehrli, B., Kuypers, M.M.M., and Schubert, C.J. 2016: Aerobic gammaproteobacterial methanotrophs mitigate methane emissions from oxic and anoxic lake waters.- Limnology and Oceanography, doi: 10.1002/lno.10312. Oswald, K., Milucka, J., Brand; A.,Wehrli, B., Kuypers, M.M.M., and Schubert, C.J. 2015: Light dependent aerobic methane oxidation reduces methane emissions from seasonally stratified lakes. - PLoS1, DOI:10.1371/journal.pone.0132574. Milucka, J., Kirf, M., Krupke, A., Lam, P., Kuypers, M.M.M., and Schubert, C.J. 2015: Methane oxidation coupled to oxygenic photosynthesis in anoxic waters. - ISME Journal, doi:10.1038/ismej.2015.12. Zigah, P., Oswald, K., Brand, A., Dinkel, C., Wehrli, B., and Schubert, C.J. 2015: Molecular and isotopic insights into methane oxidation and associated methanotrophic communities in the water column of a tropical lake (Lake Kivu).- Limnology & Oceanography, Vol. 60. (2), doi: 10.1002/lno.10035. Blees, J., Niemann, H., Wenk, C.B., Zopfi, J., Schubert, C.J., Kirf, M.K., Veronesi, M.L., Hitz, C., and Lehmann, M.F. 2014: Micro-aerobic bacterial methane oxidation in the chemocline and anoxic water column of deep south-Alpine Lake Lugano (Switzerland). – Limnology and Oceanography, Vol.

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