

The paper by Guerin et al. is about temporal and spatial variation in methane concentrations and fluxes in a subtropical hydroelectric reservoir. As such the topic is globally relevant and suits well the scope of BG. As a whole the paper is quite descriptive and serves more as a case study displaying results which are hard to scale up or taken as representative for subtropical reservoirs. The paper has been already earlier under scrutiny of three reviewers and the authors have made some corrections to the original manuscript. However, after reading the comments by the reviewers in parallel with the response by the authors, I got the feeling that although some of the corrections were made, some comments, even quite crucial ones were ignored. It is also obvious that the English language of the manuscript needs to be checked and polished by a native speaker – there are quite a lot of grammatical errors and spelling mistakes etc. The sections 'Results' and 'Discussion' are also confusing since the first contains some discussion and the latter some results.

The first reviewer had pointed out that there are some problems with the topics raised in the introduction. At least some of the problems are still there. Now the very first paragraph of introduction gives e.g. the impression that it is ebullition, not diffusive fluxes which are important. In general, the results of this manuscript are not discussed properly in relation to ebullition; ebullition is briefly mentioned in Discussion and in Table 3.

It could be fruitful to formulate a hypothesis in the end of the introduction, i.e. if you think that water intake is the key issue, please mention it here.

The first reviewer also points out that more information is needed about the study site to make the life of the reader easier. The authors just reply that all the needed information can be found in earlier papers and only added the coordinates. However, I fully agree with the first reviewer that some more information must be given so that the reader of this very paper learns to know the study site and can then easily follow the text. You should also briefly explain what kind of studies you have earlier carried out in this reservoir since I was quite surprised to learn when reading the text that you have had methane EC running on the site and besides the flux studies based on EC and now on gas concentrations in the surface water in connections with some models you have also used chambers for methane flux measurements. This all was quite confusing.

I fully appreciate your sampling efforts over three years with a sampling frequency of once in a fortnight but I must admit that I agree with the earlier reviewer that you can hardly say that this is high frequency. It is surely higher than bulk of the scientists use, but is not high frequency. In our own projects we have sampled lakes for GHGs for several years with a frequency of once a week and we still do not call this approach high frequency measurements. Please change.

For the reason of the protocol based on samplings once in a fortnight I am also doubtful whether you can really claim that once a month sampling is enough. You do not know what was going on in your reservoir between your samplings. Without measurements that is impossible. What I have learnt during my decades in limnology is that when you really start to measure aquatic systems continuously, you will soon be surprised by the finding how dynamic they are. How this dynamics is reflected e.g. in gas fluxes is a different matter, but you cannot just ignore the possibility of highly dynamic character of the system. Please change your text accordingly.

The authors have put a lot of effort in statistics with the result that due to large natural variation most of the findings are not statistically significant, i.e. there are no differences between the sampling points or times. This being the case the authors still use lots of space in explaining the differences in results. I found this quite confusing.

Some more minor comments:

The title is very long; please shorten it by leaving out the name of the reservoir and perhaps the words 'at the surface'. Subtitles in the text are also often very long and could be easily shortened.

Lines 131-134. I don't believe that anchoring has not affected the results. it would be useful to give the information about the times/sites where and when anchoring was used e.g. in Supplement.

The phrase physico-chemical is used several times in the manuscript although it is obvious that the only parameters measured were temperature and oxygen. Please change throughout the manuscript.

Lines 145-146. The word 'poisoned' sounds odd. Preserved could be better. You could also provide information about the time the samples were stored before analysed.

Line 162. Is NTPC really the original reference for the method?

Lines 207-208. I found the visual inspection of vortexes dubious.

The paragraph 2.5. is very short. Although I appreciate the idea not to repeat too much information, this methane oxidation is a very crucial part of the presented work and thus, deserves more explanation.

In the results section the paragraphs 3.3. and 3.4 are also very short. Is this really all you want to say?

Lines 355-363. This belongs to 'Discussion'.

Lines 382-385. This also belongs to 'Discussion'.

Lines 395-400. At least part of this belongs to 'Results'.

Lines 430-444. Also here part of text could be in 'Results'

Line 488. The lake is Pääjärvi.

Lines 545-548. Again some 'Results' here.

List of references: Check for the names of the journals; sometimes you use abbreviations, sometimes you spell out the complete name. Also check the subscripts etc.

Table 1. What does it mean that you give water residence time in arbitrary units? I have never heard about that.