I am happy to see that these authors spent the time to do the analyses and writing of a technical note paper, as I believe it can prove very valuable for a wide audience and can improve the methods and results of many future studies. The use of certain chemicals to preserve samples for gas analysis is very common, and it is therefore really nice that there is research on the effects of the conservation by several methods. I therefore want to thank the authors of this manuscript for working on this topic, which I think is highly suitable for the Biogeosciences journal.

However, I am currently unable to assess the results of this paper, because I lack key information on the reliability of the results. I think this paper has a good potential, but the aspects that are missing are key for the interpretation of all results, so they need to be clarified before I could properly assess the quality of the whole paper.

I find it difficult that I cannot see if the actions of adding the substances to the samples itself changed something in the gas concentrations. There are differences in the gas concentrations at t0. This can be attributed to rapid effects of the interaction between the added chemicals and the sample, but it can also be due to sample contamination or degassing during handling. T0 is taken within 24 hours, but it remains unclear if this is after 1 hour for certain samples, and after 23 hours for others. There is also no miliQ or demineralized water control.
To have these issues addressed, in a reply to this comment but also in the manuscript, would give me more certainty about the results.

I would think it benefits the paper if results and discussion are separated into different sections, but I leave it up to the editor to decide on this, as I know there is also personal preference involved on that topic.

Another key issues that I would like to hear more on is why the t0 concentrations of $CO_2$ of the different treatments is that different. You write some about it, but as I see this as a major factor for many of your interpretations, I would like to have it addressed more in depth and more clearly. Why are there no error bars on the Cu and Hg boxes of $CO_2$?

Another important overarching issue is that it seems O2 significantly decreased in the samples with CuCl2. Is this indeed the case? This is of major importance, as it would suggest incomplete inhibition of microbial processes, and would affect all results for the CuCl2 treated samples.

Specific comments on certain parts of the manuscript are included below.

**Introduction**

The first paragraph is a bit odd. I think it is most important that the reader knows why it is needed to preserve the samples, not what they are used for in the end. I would put more emphasis on the processes in the bottles that will change the concentration.

I think it is important to also mention that ZnCl2 and CuCl2 are often used as toxic inhibitors. And give some details on why it seems certain researchers pick which inhibitor (or put that in the discussion).

Problems with the disposal of HgCl2 would be good to mention as well, as well as the costs associated with it.

48 + 49-50. Based on what did you decide to pick these papers as references?

75 – 81. The tone of this paragraph is also a bit odd. You state quite suddenly that $CO_2$ concentrations will be overestimated by HgCl2, while that is not really clear from the previous paragraphs. And the last line is way too firm for an introduction. The introduction should state the current state of the art, not opinions.

82 – 92. I would structure this paragraph differently. First, in neutral words (so no effective, overestimation, etc) explain what you investigated. Then state in a few sentence the key findings of your research.

**Methods**

96. Header 'study area' does not cover the content of the paragraph.
98. What is carefully collected? Explain in more detail.
100. Avoid = limit. And not gas loss, but gas exchange with the atmosphere.
107. That cannot be uniform for all Norwegian lakes, right? Do you mean that is the case for this specific lake, or do you mean this lake was like the lakes tested in that de Wit paper?
112. T=0 could be at any moment during the first 24 hours? Or was it the same for all samples? Were they randomized for the moment of analysis, or were some treatments done first and others later?
112. Were the same bottles measured at t0, t1 and t2, or were the bottles sacrificed?
113. 'as the water samples were collected'. Which water samples? Is it not the same as the 5L bottles? Unclear.
123. Unclear what 'it' means.
129. Why were they stored cold? In general, poisoned sampled are not kept cool, as far as I know.
133. Remove 'unfortunately'.
127. Was the same amount of liquid added to all samples? Was the miliQ flushed to remove the target gasses? What was the volume of the sample bottles?
141. Sealed with what?

**Results and discussion**

Fig. 1. Please split the graphs of O2 and $CO_2$ and CH4 and $N_2O$, it is now not clear that boxes 5-8 are a different gas.
100% saturation at which temperature? That of the lake water, at the 4 degrees of the storage, or the temperature during measurement?

Table 3. This table is a bit hard to read, while it does contain very interesting information. Can you add some lines or italic or bold text or something, to make it easier for the reader to focus?
What is the ice free season number made up of? Please explain in caption.
Also write in the caption what diff is (I know it seems obvious but still good to write down).

Fig. 3. Is the lower panel a useful addition? Or can I already get the same info from the upper panel?

Fig. 4. Isn't this the exact same info as in table 3? No need to show it twice. I think the graph is much nicer, it brings across your point very clearly.

357-362. Please address why the t0 concentration in the inhibited samples was so different from the control sample, and why the range in concentrations was much larger. Is it because of sample contamination with air during the addition of the inhibitors? If this is the case, that is not necessarily a bad thing, if you then also explain that that is one of the risks of inhibitor additons to samples.

382. Do you suggest that the microbial processes are not inhibited, or that there are abiotic proceses at play?
If it's the microbial processes, then how is it possible that these microbes are not inhibited, but the ones using O2 are?

386. Please mention whether there were statistically significant changes (between timepoints or between treatments) for N2.

408. In the CuCl2 treated samples, you have both O2 consumption and $CO_2$ production. Why do you think these are not linked?

I have not provided detailed comments on the later sections, as I think it important to know more about the $CO_2$ results first, like I stated in my starting comments.