Response to Reviewer #2 Comments

At the outset, I would like to thank Reviewer #2 for conducting the review and thoroughly analyzing my preprint. I consider the Reviewer's comments to be valuable, and below I will attempt to address them. My responses are highlighted in blue font.

The paper "*Seasonal and annual variability of methane emissions to the atmosphere from the surface of a eutrophic lake located in the temperate zone (Lake Kortowskie, Poland)*" by Skwierawski is looking into the methane emissions from Lake Kortowskie for four years at five different stations, measuring emissions almost weekly. Skwierawski analyses the methane emissions in relation to a variety of environmental factors on all the data (n = 198) and on monthly average emissions using Spearman correlation, principal component analysis and linear modeling. Lastly the author uses the measured values to predict methane emissions from the lake in 2050 and 2100.
The author has collected an interesting dataset with a long time series of data. I am concerned about the placement of the chambers, as all chambers are placed within 1.5–3.5 meters of water depth, which leads the author to conclude negligible amounts of methane ebullition take place. Nonetheless, many papers are starting to conclude that ebullition occurs in the deeper parts of the lake and accounts for large fractions of the total methane emission with increasing emissions. Moreover, the analysis conducted in the paper needs a thorough check, as simple things such as intercorrelation were never considered. Additionally, I find that the extrapolation of methane emissions to 2050 and 2100 should be associated with much uncertainty. It has become a discipline in the papers on methane emissions to try and extrapolate further and wider, more often than not too much. The manuscript is also in need of more references on some of the important statements.
Overall, I find that the paper has a good-fair scientific significance due to the large amount of data collected. However, the scientific quality of the paper is fair. The statistics are missing from the manuscript, making it hard to conclude on the significance statements. Furthermore, I cannot find any information on how the data has been handled beforehand (scaled, intercorrelation, etc.). The presentation quality of the paper is fair; the language is easily understood, yet on several occasions, the reason for using analysis or the results is not revealed until later.

Thank you for this summary, and I mostly agree with the Reviewer's opinions. I will address the specific aspects in more detail later in my response, with regard to the particular comments and questions.

In general
Add statistical information when referring to correlations.
&
L133. Please indicate the method used to assess the correlation and statistical information (df, F-value, etc.).

These indicators (df, F-value) refer to the analysis of variance. The statistical indicators for correlation are r, p, and n, which I included in the presented results related to correlations. However, I will take note of this when preparing the revised version of the manuscript and will supplement the information where it is missing.

Title should reflect only diffusive emissions are considered.

I agree, I have considered this version of the title before and ultimately I agree that it would be the right solution.

Abstract
L 14. Please refrain from using abbreviations in the abstract.
L 24. "The studies" should be changed to "The results"

Thank you, I will make these changes.

Introduction
L36. Needs a reference
L37. Needs a reference

Indeed, I treated these statements as generally known information, but they require references. I will cite Collins et al. 2020 (10.1088/1748-9326/ab6039) and IPCC 2021 (10.1017/9781009157896) in this context, while also specifying the data: the methane lifetime is 11.8 years, and the global warming potential over 100 years (GWP100) is 27.0.

L45-46. I see your point, but it's counterintuitive to first state that lakes play a significant role and then state that the magnitude is difficult to estimate.

In my opinion, there is no contradiction in this statement: lakes play a significant role in the methane budget, as supported by both measurements and estimates. However, accurately estimating this role on a global scale is difficult to achieve.

L70. Belongs in the method.

I agree, but I wanted to emphasize here that the measurements were conducted using a relatively rare method, and I wanted to showcase this to the reader right from the beginning of the paper.

Figure 1. I don't understand the partitioning into partial catchments.

This is the classification commonly used in hydrology, distinguishing between direct and indirect catchments. The map shows that the southern part of the Kortowskie Lake catchment drains directly into the lake (the lake being the first receptor of water in the drainage streams), while in the northern part, the first receptor is the large Ukiel Lake. This is an important aspect of the lake's characteristics.

Methods
L130. What qualifies as a faulty measurement?

Occasionally, the device used experienced issues with smooth operation, and due to lag in the readings, it was not possible to extract a complete, uninterrupted measurement over the full 3-minute period. Fortunately, this occurred very rarely, mostly during extremely hot days with high humidity. Typically, it affected 1 out of the 5 measurements taken.

L132. The representativeness, please spell out what the measurements are representative off.

At this stage, I just wanted to verify whether my data were obtained under conditions that were representative of the weather conditions during the whole study period. And indeed, they were. The results would not have been representative if the conditions during the measurement periods had deviated from the average values observed over the study period.

L145. Why do you only use C0 and C180, when you have 216 measurements (72*3) of methane increase? You could use the linear increase.

To determine the change of concentration during the measurement period, I used the difference between the average at the beginning and the end of the measurement, as the increase in concentration was not necessarily linear throughout the measurement. Sometimes, during the

measurement, the slope of the curve changed. This occurred especially under more turbulent and rapidly changing weather conditions, which occasionally happened during measurements. The C0 and C180 results average this, typically small, but nonetheless existing variability.

L160-167. I am not convinced that this is a good method. Methane emissions are variable in space and time, which you concluded in the beginning. By removing these observations, you remove some of the noise, but the high emissions may be due to ebullitive emissions and thus very much as relevant as the low fluxes.

This approach was intended to reduce the standard deviation between individual measurements in order to obtain a more averaged mean value for the sampling date, for the purpose of processing the data over broader time intervals. However, upon reflection (and also considering the opinion of Reviewer #1), I agree that, from the perspective of interpreting temporal data, it was not the best approach. I will make this adjustment in the revised version of the paper.

L167-170. I believe that the sensor measures the changes in temperature and humidity within the chamber, so it's possible to determine the change in environmental conditions.

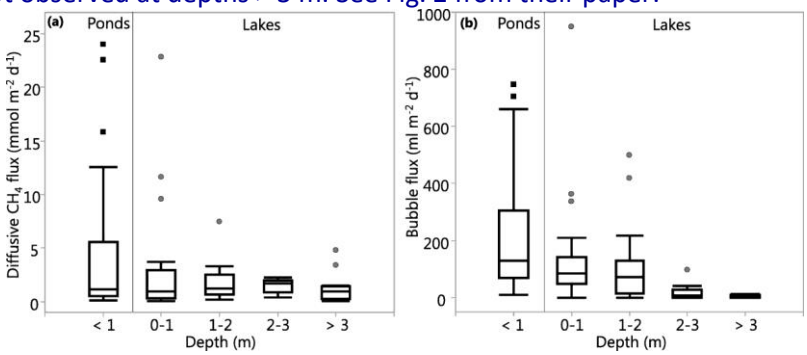
Unfortunately, the CRDS spectrometer with the measurement chamber that I used, in addition to measuring CH_4 and CO_2 concentrations, only measures absolute humidity, but unfortunately does not measure temperature.

L173. I would like you to point out in the title that you are only measuring diffusive methane emissions and not ebullitive.

Yes, I agree with that statement.

L137. As all measurements were done within 1.5–3.5 meters of water depth, you are likely to have low ebullitive emissions due to oxygen reaching the sediment, more wind disturbance and less accumulation of organic material. There might in fact be high ebullition in the deeper parts of the lake. Or in the reed belt where there is also high accumulation of organic material.

This is possible, but unfortunately I do not have data to confirm or reject such a hypothesis. However, the observations I made show that ebullition occurred only at shallower measurement sites, while at the sites furthest towards the water column, I did not observe this phenomenon even once. Similar observations for lakes are presented by DelSontro et al. 2016 (doi.org/10.1002/lno.10335), where ebullition was not observed at depths > 3 m. See Fig. 2 from their paper:



L182. Wrong parenthesis at NTU
Table 2. Use – rather than ÷ to display range. Units should be the same as the methods text even though they are equivalent.

I agree. Thank you.

Results and discussion
L216. missing mg after 14.6
L220. Also missing mg after 14.9
L224. Remove “prepared”

I will fix it. Thank you for your perceptivity.

L224-230. Remove the absolute values. As you are comparing it to your values, which are in unit area, it is irrelevant here what the total area emissions are.

The cited authors provided their data in terms of total area emissions, whereas I converted them to unit emissions based on their data. Therefore, I retained the original values from the referenced papers. If this is unnecessary, I can remove it.

L260. In my opinion, you should discard ice-covered periods, as you will have a buildup of methane underneath the ice, which will eventually be released when the ice breaks, however, by only measuring in a few locations by drilling, you will cause few areas where the methane can actually escape and thus elevated emissions here. This scenario might be the case in the instance with emissions of 24 mg m-2 d-1.

As I mentioned to Reviewer #1, I had a significant issue with how to handle the winter measurements during the ice cover period. In fact, I conducted these measurements out of scientific curiosity, and ultimately, I included these results in the dataset, treating them as 'potential emissions' for that period. Replacing these results with 'zero' values would not change the overall interpretation of the results and would only slightly affect the calculated annual average methane emissions (since n is only 14), but it also would alter the results of the individual calculated statistics in some extent. It's a shame because this was the most challenging part of the field measurements, but I understand the reservations about these results.

I would greatly appreciate a suggestion on how to handle these winter values in statistical calculations: should I leave them with zero values, or should I exclude them from the statistical analysis? Unfortunately, I find it difficult to make a decision on this matter.

L269. To my knowledge, a lot of people have tried explaining this variation, without luck. To state that the variability is attributed to weather conditions should at least be backed up further than just referring to a figure.

Thank you for this challenge, I will try to explain the observed variations in more detail in a revised version of the paper.

L307. Please explain prior how this skewness is calculated and what it means.

Skewness is a simple statistic that indicates whether the data distribution is close to normal or deviates from it. In this case, the distribution was positively skewed (positive distribution), meaning it had an "overrepresentation" of low numerical values. Skewness was calculated as one of the basic statistics in

the analyzed dataset as part of the statistical review before proceeding with further stages of data processing.

L310. Were tests made to look for intercorrelation between parameters? I would expect a high collinearity between water and air temperature.

Yes, I conducted such tests at the preliminary stage of data processing. The water temperature showed a strong correlation with air temperature (r = 0.93).

L320–322. Please rephrase, it's very hard to read this sentence.

I propose changing this phrase to:
"The conducted observations may indicate that the release of some storage emissions also occurs during the summer stagnation period, before the lake enters the autumn overturn phase."

L323–325. Need reference.
L326. Need references.

This generally refers to all papers in which methane emissions were calculated based on water concentration using the 'gas transfer velocity' equations of Liss and Slater (1974) and their subsequent modifications. There are many papers employing this method. This method does not account for wave action, but only considers the wind speed at 10 meters above the lake surface. Of course, I will add the appropriate references to support these statements.

L328. Did you calculate wave action, and if so, how? This is the first time we hear about it. It needs to be in the method section as well.

I measured the wave height directly at the measurement site. This is described in the methodology, see lines 187-191.

Table 3. I am certain many of the indicators would show collinearity which should be taken into account. What emission values are used here, is it average from each sampling period?

The table shows the correlations between methane emissions and meteorological and water indicators. I believe that the correlations between all the indicators will not be useful here. The results are based on the emission values from each measurement within the given period under consideration (years, seasons).

Section 3.2. This section is missing more discussion on the parameters, which group together with methane emissions, and why it is expected or not. What effect do the different parameters have on methane emissions? Right now, it is not used for much, or at least you do not say why you do it. I can read in the next section why you do it, but I would want to know beforehand.

I agree, in the revised version I will attempt to conduct a deeper exploration of this data resource.

L381. Yes, water temperature affects methane production, but that is different from methane emission. Water temperature also affects methane oxidation.

Of course, that is true, however, in my research, I was dealing with a black box type model and I only observed the effect of these opposing processes, in the form of CH_4 emissions into the atmosphere. This is one of the limitations of the study that I cannot avoid. From a practical point of view (i.e., in

terms of the final result – the impact on atmospheric concentration, and subsequently on the radiative balance), this final effect is probably the most important.

L411. “much” higher is pushing it here.

Well, I agree. However, in such inseparable conditions in which the lake operates (with numerous measured as well as unmeasured factors), such an R value seemed to me to be 'much’ higher.

Table 4. Please indicate the confidence interval around the predicted values.

That's a good idea, thank you for this suggestion. In the revised version of the manuscript, I will recalculate the model, taking into account the limit values of the individual components and provide the confidence interval.

L449. It should also be mentioned that the methane oxidation will increase with temperature.

That’s true, however, this was not the focus of my research. I can only comment on the final effect of these processes, which was the subject of my measurements.

L452. Add reference.

Both sentences, starting and ending on this line, are supported by references.

L467. B coefficient? I guess it is the slope.

The coefficient b is the regression coefficient (or more precisely, the unstandardized regression coefficient). 'Slope' is another term, I believe both are correct.

Section 3.3. Please indicate when values or trends from references are done *in-situ* or in laboratory experiments.

I admit that I do not understand this claim. Of course, most studies on methane emissions are based on field research (although laboratory experiments can likely be found). Perhaps the reviewer is referring to the fact that I emphasize that my research is direct in situ? If so, I only wanted to highlight with such statements that the entire measurement process was conducted in real-time and on-site, whereas most studies rely on collecting methane samples and later analyzing them in the laboratory. I don't see any reason to elaborate on this in the results section.

L480. Unit is wrong 264.4 m-2 d-1.

Of course this is a mistake, thank you.

Data availability
To my understanding Biogeosciences requires data to be uploaded in an online repository, which I encourage.

According to the journal's guidelines, there is no such requirement. However, if my paper will be accepted for publication and the editor recommends that I can publish the raw data, and I will agree to it without any reservations.