Response to comments by referee 2

The authors present a field and lab study on the biogeochemistry of methane in peat soils as related to potential leakage of deep, abandoned oil wells. The study is fine with respect to the field and lab measurements done and the authors show they are at home in the topic of biogeochemistry of methane. The peculiar aspect of the manuscript is its framing. The authors refer to the need to know background emissions from the shallow subsurface when it comes to potential leakage from the deeper subsurface. I fully agree with this need but several remarks must be made:

We would like to thank the reviewer for the very thorough review and the positive general assessment of our study and results.

1. the title is misleading as it is very general. It should be indicated in the title that this study deals with peat soils as they have the highest potential for methane emissions (probably together with paddy rice fields)

The reviewer is right to point this out, we will change the title to better fit the presented work:

"Interferences caused by the biogeochemical methane cycle in peats during the assessment of abandoned oil wells"

2. the authors were unfortunate that no thermogenic methane was found at all. However, they stick to searching for thermogenic methane which makes the manuscript in a way forced in its scope. Section 4.2 is a peculiar read as well as other parts of the manuscript.

The reviewer is correct in the impression that the focus of the study is the evaluation of potentially leaking oil wells and possible complications in peat with high methane cycling. We were not unfortunate to not find any thermogenic methane, but we do consider the very strong interpretation of our data to be necessary. We agree with the reviewer that section 4.2 has to be reworked. We will shorten the comparison with the soil gas approach by Romanak et al. 2012 and incorporate a comparison with natural methane fluxes and other studies that found methane emissions from leaking abandoned wells.

3. the study is relevant when it comes to greenhouse gas emissions and climate change. This comes around the corner at a rather late stage in the manuscript. The scope of section 4.3 should be introduced much earlier.

We thank the reviewer to point this out and will rework part of the abstract and introduction to better highlight this part of the scope.

4. the authors studied peat soils above abandoned oil wells. This is a somewhat poor choice since oil reservoirs often have lower liquid pressures than the surrounding rocks due to the exploitation for oil. Hence, there is oil and/or water flow from surrounding rocks to the reservoir (often not completely depleted). Depending on the exact composition of the field, there may be more or less natural gas involved in oil reservoirs which has a tendency to move upwards because of buoyancy effects but this may be restricted depending on the resulting pressure field. It would have been much, much better when the researcher had studied peat soils above abandoned gas wells. It seems to me that the authors are not aware of this major difference. They however should indicate this essential difference. Here, I realise that local geology plays a role in the way reservoirs may be connected or not to above as well as potential leakage of gas or oil along or through (abandoned) wells.

> *The reviewer points out an important potential difference between oil and gas wells, which the community and we are fully aware of, however earlier studies showed substantial methane emissions from plugged/abandoned oil wells (e.g., 4.6 x 10^{-2} g h^{-1} – 0.13 g h^{-1}, Williams et al. 2021). These emissions were lower than emissions from gas wells (4.1 x 10^{-3} g h^{-1} – 18 g h^{-1}, Williams et al. 2021) but still substantial. Methane emissions from oil wells were also reported in other studies (Kang et al. 2014, Townsend-Small et al. 2014, Saint-Vincent et al. 2020).*
>
> *Furthermore, oil wells in Germany are typically older than gas wells, as gas exploration started in the 1960s, and oil wells (which started in ~1860 and prospered after 1900) are thus more prone to integrity failure as material and technology were less advanced. Additionally, due to the higher number of oil fields in Germany and the production dependent higher number of wells in an oil field in general, there are 6 times more oil wells than gas wells in Germany.*
>
> *We want to point out, that this was a pilot study, with the aim to test our methodology particularly in an area with abundant biogenic methane cycling. We did not aim nor wanted to convey the impression that this study is representative for abandoned oil and gas wells in Germany, to make this clearer we omitted the "gas wells" from the title. In addition, we are currently working on an additional study to address the abandoned well situation in Germany for which we already measured and are still planning to sample multiple gas wells and more oil wells as well.*

Especially based on remarks 3 and 4, I suggest that the authors rewrite the manuscript where they present a new framework addressing both greenhouse gas emissions from peat lands and dealing with shallow biogenic methane emission when addressing potential leakage of thermogenic methane. If not, I must recommend to reject the manuscript.

> *We addressed these points and hope that especially our answer to remark 4 was able to convince the reviewer, that it is important to consider oil wells as well. In addition, our study design was established to address the question if our methodology was sufficient to detect methane emissions in the vicinity of abandoned wells and more importantly if we are able to pin point the source of such methane in a complex and organic rich setting. Another framework, for example focusing on greenhouse gas emissions from peat lands as suggested, is beyond the scope of this study and its design. This would result in shortcomings regarding the sampled parameters and for example lack of seasonality. With this in mind, we carefully balanced the selected scope and hope the*

reviewer will approve the changes to the manuscript and the resulting clearer message we want to convey.

We further want to point out, that we did not sample pristine peatlands but sampled three anthropogenically influenced peat-rich areas that had been drained decades or even centuries ago.

In addition, I have the following general remarks in addition to individual remarks annotated in the manuscript:

1. carefully check on the use of single versus plural. Subjects and related verbs are frequently not in harmony.

We thank the reviewer for pointing this out and will follow the thorough individual remarks in the uploaded pdf and we will additionally check the manuscript carefully again regarding these points.

2. the RESULTS become boring to read when presenting all kinds of numbers. These numbers should be presented in Tables (as done) and patterns should be discussed in terms of higher, lower, etc. Very importantly, it remains unclear whether the different sites differ statistically significant or not. I recommend that significance tests get added and box-whisker plots get presented (instead of means or so in tables that may be moved to supplementary material).

Indeed, the results included a lot of numbers, we will revise the marked parts and will include box-whisker plots in the result section (see below). For the statistical tests we used the Kruskal-Wallis-Test to test for normal distribution, which the methane fluxes did not show. The Mann-Whitnes-U-test was then used to compare well and reference site data. For WA-211, WA-209, WA-273, WA-264, WA-272, WA-275 well and reference sites were similar with regard to methane fluxes. WA-274 and WA-254 however showed significant differences in fluxes between well and reference sites, however, in case WA-254 the reference site showed higher emissions, and in case of WA-274 the well site a lower methane sink.

We further plan to stream-line the result section and focus more on trends rather than single numbers.

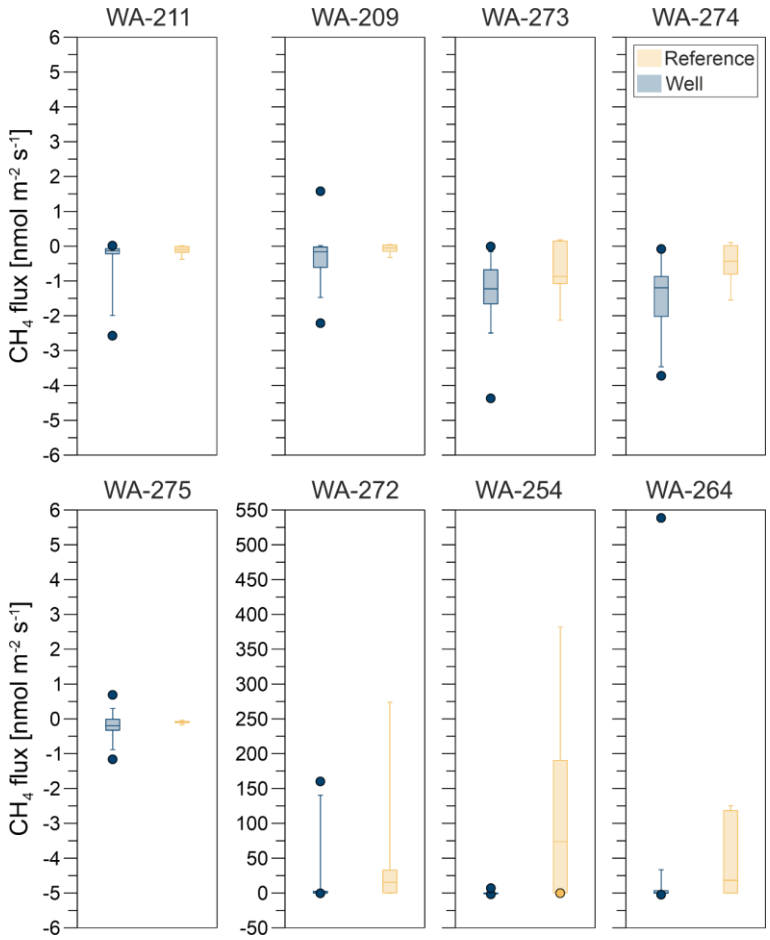

Figure 1: Box-whisker plots for the measured well and reference sites: (a, b, c, d) forest, (e) meadow, (f, g, h) peat. The underlying statistical parameters are listed in Table S9.

3. the DISCUSSION presents a lot of results which should be avoided. Move more effectively to the discussion topics.

We plan to restructure the discussion section and implement a clearer separation of results and discussion. However, in our opinion is the interpretation of data, as for example using the isotopic plots of figure 7 part of the discussion.

4. the CONCLUSIONS are poorly written. It reads more like a summary than conclusions

We will revise the conclusions to underline the implications of our study better. The mentioned comparison with Bowman et al. 2022 from the original conclusion will be moved to the discussion section 4.2. The conclusion will read:

"While it is well known that abandoned oil and gas wells can have integrity issues, respective knowledge particularly on the 20,000 cut and buried wells in Germany, is lacking. Here we provide with our multi-methodological approach first data on potential methane fluxes from abandoned oil wells to the atmosphere. We combined

emission data (positive and negative) at wells and reference areas with gas geochemical characterization of soil gas samples to investigate eight wells in a peat rich setting with three different land use types (Forest, Meadow, Peat extraction).

The Peat extraction site was the only one, which emitted substantial amounts of methane. However, in general no difference in surface methane emission rates between well and reference sites independent of site characteristics (active peat mining, drained peat vegetated with birch trees or grassland) were observed. With respect to soil gases, the three areas showed highly variable but spatially correlating (i.e. area specific) methane concentrations concurring with CO2 concentrations. The in-depth gas and isotope geochemical analysis revealed biogenic methane as source for the net emissions at the open peat site (methyl fermentation) and the meadow (CO2 reduction pathway with partial methane oxidation). These findings and the absence of higher hydrocarbons excludes thermogenic gas emissions from the plugged wells.

Furthermore, the laboratory methane oxidation rates derived from our incubated peat samples demonstrated the capacity to counterbalance reported leakage rates for buried abandoned wells in other regions. The activity of such a microbial methane filter poses the risk for false negative leakage classification. Overall the observed methanotrophy could be highly relevant for Germany as 15% of our cut and buried wells are located in areas with very organic-rich soils. However, for a comprehensive evaluation of the situation of abandoned wells in Germany further investigations are needed. Therefore, additional sampling at different sites (oil/gas wells of different age and deconstruction histories) in Northern Germany with the here introduced methodology are under way and we will evaluate the natural mitigation potential for different soil types and land uses.

In conclusion, exclusively using emission-based approaches are not suited for integrity failure assessments of buried wells as these would be susceptible to misinterpretations. We highly recommend a holistic approach for surveillance including the determination of methane emission, soil gas composition and isotopic signatures at and in the vicinity of well sites against the background of a carefully selected reference site."