

Interactive comment on “Changes of the CO₂ and CH₄ production potential of rewetted fens in the perspective of temporal vegetation shifts” by D. Zak et al.

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We highly appreciate the work of both referees since it has raised some valid and interesting points that we were glad to re-assess. We were happy about the very positive comment of Referee 1 but also about the critical points of Referee 2 which has been helpful to overcome the shortcomings of the earlier text. The text has now been revised in accordance with the suggestions and comments as listed in detail below (referee comments in quotation marks).

Anonymous Referee #1 Received and published: 3 November 2014

“This study brings valuable contribution to a better understanding of decomposition

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and nutrient fluxes processes after rewetting of fens. The results of this study can be applied in restoration planning and in post-restoration management in this type of ecosystem to influence final GHG emissions and leaching of nutrients. I especially appreciate very precise design of the whole experiment, when all potential effects of mesocosm incubation on final results were considered and discussed. The results are well discussed and compared with other studies. Implication for peatland restoration are also well described and discussed and conversion of results from mesocosm experiment to real scale is very important and useful. The conclusion are well justified by the data and nicely extend existing information”

We gratefully acknowledge this positive comment. We are very happy that the referee values the effort we have done with the experiment and to converse the results from the mesocosms to the real scale with great care. Indeed there have been some critical points detected by the Referee 2 which were very helpful to make some further improvements.

Anonymous Referee #2 Received and published: 14 January 2015

“This paper compares potential GHG emissions from litter from five macrophyte species. The experiment is fairly simple and mostly well described, and provides some useful in vitro data. However, because of the small scale and in vitro nature, there are limits to how the results can be extrapolated to the real world, and this need to be made clearer.”

We gratefully acknowledge this comment since it was our main concern during data evaluation to highlight the possible ecosystem implications as well as, to the same extent, the limitations of our results and to avoid the reader from over-interpreting our results. The referee noted the simplicity of our experiment, what we agree on. This simple approach allows (with limitations) the disentangling of the pure litter quality effect on methane emissions, but inherently bears the disadvantage that the transfer of the results to a complex ecosystems is limited. In accordance with the referee this has

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to become clearer in the manuscript. Therefore we did a very critical re-assessment of the whole manuscript to ensure that the extrapolation of our experimental results is done in a reasonable way now (see also below). We liked the statement of the last part of the comment very much and used this phrases in a slightly modified form as a kind of a directive at the beginning of the second part of the discussion: ““Due to the small scale and in vitro nature of the incubation experiment, there are limits on extrapolating results to the “real world””.

“Section 4.2 Implications for peatland restoration - Much of this section is speculation, and should be cut down considerably. It is completely unclear how the authors jump to estimations of annual net GHG exchange on an area basis. Literature values of annual biomass production are not very pertinent; it is the net balance of photosynthesis and ecosystem respiration that matters, plus the net methane emission. This should either be cut, or made explicitly clear how the estimations were done.”

This section was revised carefully. The parts related to GHG exchange were deleted. At the moment we solely focus on methane emission. Since we not measured all factors controlling in-situ methane emissions apart from the “substrate quality” we just summarized those factors instead of discussing them in detail. We still use the lab data to make an assessment of the annual methane production potential by using biomass data obtained from the sites under investigation. We believe that such an approach is reasonable to emphasize the importance of the decomposition of fresh shoot biomass from different wetland plants of the sites under investigation. In overall this part of the discussion is shortened by about half a page, the speculative statements were removed.

“Section 5 Conclusions - again, much of this is not deducible from the study described here. This should be restricted to what can be concluded from this study. Speculative extrapolation should be kept to the Discussion.”

We agree some of the conclusions were not deducible from the study. Accordingly we

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revised the conclusions and believe that no speculations are left. However, we did not move those deleted aspects in the discussion in order to avoid including any further speculations to the manuscript.

"Statistical analysis - I don't see the value of null-hypothesis testing here - the null hypothesis is not worth testing, and the sample size of $n = 3$ makes it somewhat futile. Showing confidence intervals on results would suffice. There is scope to look at statistical modelling of the GHG emissions in relation to litter composition and species, e.g. does including species in the model help explain variation in CH₄ emission? This is far more relevant than presenting p-values of differences between species. Table 5 shows results of some regression analysis, but there is scope for more here, and this would improve the paper, ideally at the expense of some of the speculation in the Discussion."

The reviewer has likewise made a good point here. Therefore instead of using hypothesis testing, we have provided confidence intervals for the results in Tables 2, 3, and 4. We have removed the significance tests from the text. The confidence intervals in Table 4 are based on the sample means of the daily measurements of gas emission rates to avoid pseudoreplication. We also agree with the reviewer that more statistical modelling of GHG emissions would be desirable, however this was not possible because (as described in the methods) the nutrient measurements were derived from pooled samples. Therefore it is not possible to do more than assess the correlation between species mean GHG emission rates and species mean nutrient content. This is what we show in Table 5 in connection with boxplots to illustrate species differences.

"There are a few more points that need clarification: GHG emissions are expressed as (for example) mg CO₂-C per g C. However, how the denominator is calculated is ambiguous: is this based on the initial mass, the final mass or interpolated between these?"

It is clarified (based on the initial mass) both in the text but also in the tables and figures.

"C. demersum seems to quite distinctly different from the other species stoichiomet-

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rically, with a very low carbon content. Firstly, it needs to be checked that such low values are actually plausible, and some reference given. Secondly, given the low C content, does it make sense to express results on a per g C basis? Do emissions from *C. demersum* appear high simply because of the low C content? Perhaps a total mass basis would be better.”

The carbon content of *C. demersum* was about 1.4 times lower than the other species, i.e. in average 343 mg/g dry mass which was in accordance with literature findings. We included this information in section 3.1: “The comparatively low carbon content of *C. demersum*, about 1.4 times less than the other plant species, is consistent with literature findings and holds also true for regions that presents tropical climate (e.g. Dos Santos Esteves and Suzuki, 2010).” We also related the emissions to dry mass and still found substantial differences.

“Figure 3 - what do the error bars represent?”

Min and max values, which is denoted now.

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