

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.

Anonymous Referee #2

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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.

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
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be cut, or made explicitly clear how the estimations were done.

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.

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.

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?

C. demersum seems to quite distinctly different from the other species stoichiometrically, 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.

Figure 3 - what do the error bars represent?

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