

Interactive comment on “Synthesizing greenhouse gas fluxes across nine European peatlands and shrublands – responses to climatic and environmental changes” by M. S. Carter et al.

Anonymous Referee #3

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General comments: Studies as the one presented here have the potential to yield new insights. The comparison of several sites that have been examined with the same experimental design is useful. However, the manuscript suffers from not being able to decide whether it is a review or a big, comparative case study. For a case study, sites and data are insufficiently presented. For a review, the adequate consideration of other articles is missing. Also, I have questions on data quality and the lack of consistent data acquisition at all sites. What is the point of reporting CO₂ efflux data from vegetated sites? I don't see why the presented sites have been selected (apart from the fact that they have been examined within NitroEurope). I do not understand why heathlands and one maquis site have been selected. Treating the non-peatlands separately may yield

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in two strong and not one incoherent article. Considering the already present extensive introduction and discussion chapters, I recommend using the presented manuscript as a basis for two review-type contributions. Please don't say “peatlands”, but “peat bogs” as fens were not examined. Specific comments: P. 3696, L.10: Isn't the potential C loss from drainage of N fertilization the bigger problem? Not all ecosystems you are presenting store a lot of C (heathlands). Maybe it would be better to also refer to the global significance of C loss from peatlands and add few sentences on heathlands. P. 3696, L.25-28: The sentence “Methane. . .Le Mer and Roger, 2001” is not necessary and can be deleted. Introduction: I am aware of the difficulties in calculating a complete C budget (including GPP) and sometimes, it is the best thing just to report CO₂ efflux. However, in that case a paragraph should be devoted to this problem arguing why it is sensible to limit CO₂ data to efflux and what the benefits of this procedure are. P. 3702, L. 1 to 7: If CH₄ and N₂O have been measured at only one collar, it is impossible to gain any idea of spatial variability. This would be a grave shortcoming and considerably decreases the significance of the presented results. On the other hand, looking at Table 3, I get the impression that GHG fluxes have been measured from several collars at each site. Please clarify this. Also, the measurement frequency is on the low end of what can be considered acceptable, especially considering the simplistic linear upscaling procedure. What does “well before” (L. 5) mean? At least how many days or weeks? P. 3704, L. 19-20: How was C stock estimated? From the supplementary material, I get the impression that it has been measured, not estimated. Is it sufficient to report the C stock in the first 20 cm in peatlands? P. 3707, L. 14: Why don't you say “. . .at the site with the highest. . .” Chapter 3.4: You are right, CO₂ fluxes are probably even lower. But this means that the real flux is not known. How is it possible to analyse that kind of data? In my opinion, the only use for that kind of data (from vegetated sites) is assessing minimum fluxes that can be used to support other arguments. P. 3710, L. 5-12: The fact that, despite not having considered other major controls of CH₄ release, you achieved such a nice temperature dependency is astonishing. Still, one wonders what the value of this finding is considering your limited dataset with a focus

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on ombrotrophic bogs. Fitting your data points to other, published data, with known plant species or water table, would help in coping with this caveat. And: You did not do a space for time substitution. Too many other environmental factors differ. P. 3711, L. 12-27: Maybe decreased CH₄ oxidation as a consequence of presence of NH₄ plays a role here? P. 3711, L.26-29: This shows that the non-consideration of “specific properties” makes new insights very difficult. Why weren’t these properties examined? P. 3712, L.16-17: Maybe looking at % water filled pore space might help. Generally, the high measurement interval hurts N₂O upscaling most. Table 2: Did you account for the different soil temperature probe depth at the sites? For GHG analyses, %water filled pore space (WFPS) is a better parameter compared to %vol. Why wasn’t WFPS used? All Figures: Nice Figures! Figures 2 and 3: The SE of what is shown by the error bars?

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