Reply to anonymous referee #2

Comment by anonymous Referee #2	**Author reply**
The study by Bulters et al. synthesized updated GHG flux data to evaluate GHG emission factors from forest organic soils in the Dfb zone, stratified by drainage status, nutrient availability, and dominant tree species, and conducted the upscaling using Latvia as a case study. The study provided a good example and moved a step forward to derive realistic country-specific emission factors which are of high importance for national GHG inventory under forest organic soils and consequently forest management strategies. The topic fell within the scope of the journal well and is interesting to the journal audience. Still, the following needs some revision to improve the quality of the manuscript.	We sincerely thank the reviewer for the review and for identifying specific points we can address to improve the manuscript. Replies to specific comments are listed below.
1. The methods section needs to be more detailed on the data synthesizing procedures and criteria.	In the revised manuscript, we will clarify that we included only peer-reviewed forest land emission factors with assigned nutrient richness status representing annualised net soil GHG fluxes in the synthesis. No additional data manipulation was performed within the present study; we synthesised emission factors that were already suitable for direct use. For some emission factors obtained from Jauhiainen et al. (2023), the original studies did not directly report annual net soil GHG fluxes. However, the necessary processing and harmonisation had already been performed by Jauhiainen et al. (2023), and these procedures are described in their supplementary material. We will also clarify the weighting procedure used to calculate mean emission factors. We followed the weighting system introduced by

	Jauhiainen et al. (2023) and retained the weights originally assigned to their data. For additional studies included in our synthesis, a weight of 1.0 was assigned because the reported values already represented annual net soil GHG fluxes and required no further manipulation. Finally, when the original source did not report the dominant tree species, we did not use those data to derive species-specific emission factors. However, we retained them in the synthesis of species-independent forest-land emission factors, provided they met the other inclusion criteria.
2. Specifically, what is the CO_2 fluxes referring to here? Is it total respiration of the forest soil (forest floor) or heterotrophic respiration?	We can clarify that the CO_2 fluxes included in the synthesis represent net soil CO_2 emissions: the sum of soil carbon loss by heterotrophic respiration and carbon inputs to the soil through litter.
3. The discussion section can be separated into several subsections to make it more structured and highlight the main points	Thank you for the suggestion; we can add subsections to improve readability.
4. The manuscript title didn't reflect emission factor, which is the core of the manuscript.	We recognise that the title can be improved. Possible options are: • "Synthesis of greenhouse gas emission factors for forest organic soils in the Köppen–Geiger Dfb climate zone" • or altenatively: "Synthesis of greenhouse gas emission factors for forest organic soils in the Köppen–Geiger Dfb climate zone with cold winters, warm summers and no dry season."
5. It's good that the authors discussed a lot on the variations of the emission factors stratified	Thank you, this is an interesting aspect to highlight. We can introduce a note in the Discussion that emission factors typically applied in the region generally

by nutrient, tree species, etc., and the rationale of different grouping options. But how does the refined emission factors in this study, compare with the emission factors used previously for GHG inventory under forest soils for the region?	average all available information across broad geographical areas, irrespective of the representativeness and composition of the underlying dataset, and are subsequently applied to areas of interest irrespective of their actual composition in terms of dominant tree species and site nutrient richness.
6. Line 228-229: the logic of the last sentence seems to be off following all the previous discussion for this paragraph.	We agree, sentence "Taken together, these results indicate that drainage status exerts a stronger control on soil CO2 fluxes than tree species composition alone" is not appropriate there and can be deleted.
Technical corrections: 1. Line 32: "CO_2", the subscript is in different format as other "CO_2". The same issue also exists for CH_4 and N_2O such as in line 134, 258, 274, 319 and 330-331. Please make it consistent throughout the manuscript. 2. Line 45: "statuses" to "status" 3. Line 126: "No relationship" between … and …? 4. Table 1: numbers "0.47" with superscripts "3", is it really corresponding to footnote 3 "All nutrient-rich deciduous sites"? Because the numbers are different from those under "Alder" and "Birch + Other" for nutrient status under "Rich" 5. Line 294: "ageing" to "aging" 6. Line 318: citation missing 7. Captions for Figure A1-A4 are not complete in terms of the	Thanks, we will correct these issues. 4: The superscript is correct for 0.47, as we applied the nutrient-rich deciduous species total average to any deciduous species at sites with any soil nutrient richness. 9: We consider the use of "–" to indicate ranges potentially counterintuitive when the maximum value is negative,

meanings of the lines and whiskers, such as medians, interquartiles… 8. Table B1 caption: unit for CO2 emissions “(kg CH ha−1 year−1)” wrong, should be “(t CO2 ha−1 year−1)”? 9. Table B1-B3: The order of rows are not arranged in any level of “Nutrient” or site “type”. Better arrange it in the order of “Nutrient” first, and in each nutrient level, follow some order of site type? In addition, the ranges are not shown properly with “−”, and the fonts are not consistent 10. Figure A5: legend for right panel, the 2 and 4 for the GHG not subscripted	