

Edits made in accordance with comments of Referee #1

Comment by anonymous Referee #1	Author reply	Edits made
The manuscript from Butlers et al. describes ghg-emission measurements from forests in the temperate climate zone especially for Latvia and surrounding countries. With the help from this collected data, the authors derived emission factors for organic soils under forests for Latvia. This emission factors are of high relevance of ghg reporting since these factors are missing for many countries. Of course, emission factors presented in this paper have to be improved, but it is a first step. Therefore, the topic of that paper is of high relevance for the readers of the journal. The authors also conclude correctly, that measurements and emissions factors for organic soils under forest for the temperate climate zone are missing.	We agree that improving emission factors is a continuous process as new measurements become available. We consider the present synthesis an important intermediate step toward moving from static emission factors towards more dynamic approaches for estimating soil GHG emissions that can better account for environmental conditions and site-specific variability.	Not needed.
The title of the manuscript does not describe the content well. It should contain that the manuscript is about to derive emission factors for organic soils under forest from ghg emission measurements. By	We originally attempted a more engaging title rather than a generic descriptive one, but we agree that the current title may not sufficiently reflect the manuscript's main focus. We can therefore revise the title to make the derivation and synthesis of emission factors	Title edited as: "Synthesis of greenhouse gas emission factors for forest organic soils in the Köppen–Geiger Dfb climate zone"

reading the current title, readers may think it is about differences of ghg emissions in winter and summer or under changing climate.	more explicit. Possible options are: • “Synthesis of greenhouse gas emission factors for forest organic soils in the Köppen–Geiger Dfb climate zone” • Or alternatively: “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.”	
Further the quality control of the used studies should be described in more detail.	We agree that the quality-control procedure can be described more explicitly: quality control followed the approach described by Jauhiainen et al. (2023), as the present synthesis builds substantially on the dataset compiled in that study where only peer-reviewed studies providing sufficient information to derive annual soil GHG flux estimates were considered. Where required, the originally reported fluxes were harmonised to annual soil GHG balances by applying supplementary procedures such as annualization of warm-season measurements, separation of heterotrophic from total soil respiration, or accounting for litter inputs. Detailed descriptions of these study-specific adjustments are provided by Jauhiainen et al. (2023), particularly in their Table S1.	For quality control, we introduced a specific section: 2.2 Data quality control and harmonisation (80-99)

	Individual estimates were weighted according to their relative reliability. A weight of 1.0 was assigned to estimates without major identified limitations, whereas a weight of 0.5 was used where uncertainty was considered higher, for example because of limited spatial replication or greater reliance on supplementary data when deriving the annual flux estimate. For annual and annualised flux data from Jauhiainen et al. (2023), the original reliability weights were retained. Additional studies compiled for the present synthesis were assigned a weight of 1.0 as the reported emission estimates were used directly without additional data manipulation. Therefore, no additional manipulation of the original data was performed within the scope of the present synthesis. We synthesised emission factors as reported in the published studies. At the time of preprint submission, part of the dataset was still unpublished and was therefore described as unpublished data. These data have since been published in a peer-reviewed article. Consequently, all emission factors included in the synthesis are now derived from peer-reviewed publications.	
In line 60 the authors mention, that some of the studies have not described vegetation. The question is,	We appreciate this comment, we can clarify this point in the revised manuscript. We did not consider the absence of	We clarified in Lines 60-63 that: "Where tree species were not reported, the data were used only to derive tree species-independent EFs

if such studies can be used to derive emission factors for organic soils under forest.	information on vegetation (dominant tree species) to be sufficient reason to exclude a study if the site was clearly identified as forest land and the reported flux estimate was otherwise suitable for inclusion. However, we did not use such estimates when deriving tree-species-specific emission factors. We included them only in the synthesis of general forest-land emission factors, independent of tree species. This allowed us to retain relevant forest GHG data without introducing unsupported assumptions about vegetation composition.	and were excluded from species-specific EF derivation”
Many studies did not give soil properties. How do deal with that? Where are the borders the authors decided not to take a study into calculation for emission factors? What quality requirements were applied to the measurement systems?	Soil-property measurements were not used as a mandatory inclusion criterion for the synthesis. Inclusion was instead based on the quality-assessment framework described by Jauhiainen et al. (2023), including whether the estimate could be clearly attributed to forest land, whether an annual estimate is provided, and whether soil nutrient status was indicated. We therefore did not exclude otherwise suitable emission estimates solely because detailed soil analytical data were not reported. We believe these concerns can be sufficiently addressed by the expanding description of the quality-control and study-inclusion procedure as described above.	In lines 69-69 we explained that “Availability of these supplementary data was not an inclusion criterion. Study inclusion was based solely on the flux and site-characterisation criteria described above, including identification of drainage and soil nutrient status.” Quality procedures are explained in section 2.2 Data quality control and harmonisation. Included studies and inclusion criteria (peer-reviewed sources, annualised net forest organic soil fluxes within Köppen–Geiger Dfb climate zone, nutrient richness and drainage status specified) are described in section 2.1 Data compilation.
Studies differ in length and frequency of measurements.	We can clarify in the manuscript that only annualised GHG flux	We explained in section 2.2 Data quality control and harmonisation

	estimates were included in the synthesis. We did not perform annualization in the present study. Where original studies reported only seasonal measurements, Jauhiainen et al. (2023) had already annualised them using peer-reviewed procedures and supporting data. Differences in study duration and temporal coverage were accounted for through the reliability weighting scheme described by Jauhiainen et al. (2023). We retained the weights originally assigned by Jauhiainen et al. (2023). For additional data included in the present synthesis, we assigned a weight of 1.0 because no data manipulation was required and the reported values represented annual net soil GHG fluxes.	that only results that characterise annualised fluxes were used in this study.
The authors should explain, which standards were applied to the different studies.	The data processing applied to the original measurements, where required, is described in the Supplementary Material of Jauhiainen et al. (2023). In the present synthesis, we did not perform any additional manipulation of the underlying data; we used the published annualised GHG emission estimates as reported in the source studies. We believe we can address this issue by elaborating on the inclusion procedure described above.	In Section 2.2,. Data quality control and harmonisation, we now clarify more clearly that no additional manipulation of the reported flux data was performed in the present synthesis. Only flux values meeting the defined inclusion and quality-assessment criteria were used. We explained the the data treatment was limited to application of the reliability-weighting system and harmonisation of units as now explained in Section 2.2.
Which flux type was considered for the CO₂ fluxes? NEE or Reco?	The data obtained from the original sources represent net soil emissions, i.e. the soil CO₂ balance (sum of heterotrophic respiration C losses and C input to soil by litter) rather than ecosystem-	In Lines 53-54 we clarified that “The addressed net CO₂ fluxes represent soil CO₂ balance: sum of soil CO₂-C loss by heterotrophic respiration and C input to soil by litter.”

	scale NEE. We can clarify this explicitly in the manuscript around line 55.	
In line 220 the authors mention that varying study design of the different studies lead to biases within the results. The question which has to be answered is, is it better to include all studies or use only the few studies which meets all quality criteria?	Only data meeting the specified inclusion and quality criteria were included in the synthesis, as described above. We believe this concern will be addressed by providing a more detailed description of the study-selection, inclusion, and quality-control criteria in the revised manuscript as described above.	Included studies and inclusion criteria (peer-reviewed sources, annualised net forest organic soil fluxes within Köppen–Geiger Dfb climate zone, nutrient richness and drainage status specified) are described in section 2.1 Data compilation. The edits made in section 2.2 Data quality control and harmonisation now clarify the quality criteria, which were most importantly that the flux characterises annual net GHG fluxes. We removed the statement that fluxes from a study in the publication process were included, as the corresponding article was published in a peer-reviewed journal during the review process. Accordingly, Kamil-Sardar et al. (2026) have been added to the studies included in the synthesis. (Line 56). Now all values used are derived from peer-reviewed studies, as mentioned in Line 52
In units' year should be abbreviated with a.	To retain consistency with our previous articles published by Biogesociences, we would like to retain year ¹ .	We retained use of “year ¹ ”.
In line 133 hyphen is used instead of small - at -4,55. Line 318 there is an error with formatting. In the reference list gases should be subscripted (e.g., N ₂ O, CO ₂ ...). The reference Latvia's National Inventory Document in line 449 is not mentioned in the text.	Thank you for pointing out these issues. We can address them during revision of the manuscript.	Corrected. Latvia's National Inventory Document is referenced in Line 390, it is now added to line 258 as well.

Edits made in accordance with comments of Referee #2

Comment by anonymous Referee #2	Author reply	Edits made
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.	Not needed.
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	We clarified in L67-68 that “Study inclusion was based solely on the flux and site-characterisation criteria described above, including identification of drainage and soil nutrient status” Above in the section 21. we clarified that we included peer-reviewed sources of annualised net forest organic soil fluxes within Köppen–Geiger Dfb climate zone with

	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.	site nutrient richness and drainage status specified. We edited Material and Methods section by splitting it into 3 subsections for improved readability on procedures. In section 2.2 Data quality control and harmonisation, we explain how it was ensured that only annualised and harmonised data were used, and we improved the description of the weighting system to clarify its role in mean EFs estimation, as explained in 2.3 Emission factor synthesis and statistical analysis.
2. Specifically, what is the CO₂ fluxes referring to here? Is it total respiration of the forest soil (forest	We can clarify that the CO₂ fluxes included in the synthesis represent net soil CO₂ emissions: the sum of soil carbon loss by heterotrophic respiration and carbon inputs to the soil through litter.	In Lines 53-54 we clarified that “The addressed net CO₂ fluxes represent soil CO₂ balance: sum of soil CO₂-C loss by heterotrophic respiration and C input to soil by litter.”

floor) or heterotrophic respiration?		
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.	Subsections are introduced.
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 alternatively: “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.”	Title edited as: “Synthesis of greenhouse gas emission factors for forest organic soils in the Köppen–Geiger Dfb climate zone”
5. It's good that the authors discussed a lot on the variations of the emission factors stratified by nutrient, tree species, etc., and the rationale of different grouping options. But how does the refined emission factors in this study,	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 average all available information across broad geographical areas, irrespective of the representativeness and composition of the underlying dataset, and are subsequently	We implemented the initial response in Lines 231-235. In addition, we introduced comparison with other EFs in Lines 250-258.

compare with the emission factors used previously for GHG inventory under forest soils for the region?	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 CO ₂ fluxes than tree species composition alone” is not appropriate there and can be deleted.	This sentence is deleted.
Technical corrections: - Line 32: “CO₂”, the subscript is in different format as other “CO₂”. The same issue also exists for CH₄ and N₂O such as in line 134, 258, 274, 319 and 330-331. Please make it consistent throughout the manuscript. - Line 45: “statuses” to “status” - Line 126: “No relationship” between ... and ...? - Table 1: numbers “0.47” with superscripts “3”, is it really corresponding to footnote 3 “All nutrient-rich deciduous sites”? Because the numbers	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,	All is corrected except: 4. where the subscript was correct ; 6. This error was due to an error in reference to the table; 9. “–” would be counterintuitive .

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 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		
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4 for the GHG not subscripted		
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