Answer to Jens-Arne Subke

Thank you for your comments and the generally positive assessment of the revised manuscript. We appreciate your thorough cross-checking and the suggested refinements, which we will implement. Please find our responses to the questions and observations in the table below.

Question/Observation	Response
The presented study has a somewhat imbalanced representation of tree species, drainage and soil types so that results have to be considered with some caution.	We agree that the distribution of dominant tree species, drainage status, and soil types is important. Accordingly, we present the results stratified by drainage status, tree species, and nutrient status (e.g. Fig 9).
Emission Factors form a significant part of the rationale of the study, taking up several paragraphs in the introduction. This is not reflected in the discussion which focusses much more on fundamental understanding of C balances, not emissions reporting. It would be better to address this by setting the scope of the study and motivation for study differently in the introduction.	The motivation of the study is indeed rooted in the necessity to improve the accuracy of organic soil GHG gas inventories in the Baltic states. We were shy of using the term EF in regard to our soil balance results; however, as also Referee #1 called attention to this, we will revise the Discussion with a note that the results can be used as EFs, and compare them to the IPCC default EFs. However, we still consider that the discussion focusing on the understanding of C balances is also warranted because EFs are not separate from these fundamentals; rather, they are derived from and guided by them.
Soil pH is cited throughout the manuscript as an important correlator of C balance. It is generally presented as a causal link of lower pH and C stocks. However, the cause of pH differences are not considered meaningfully, where conifer plantations are likely to have reduced pH due to acidic litterfall. The correlation between C stocks and pH are hence linked to vegetation more than pH being an independent driver of C stocks. This should be much clearer in the discussion (e.g. 520-525).	We agree that soil pH can be influenced by the dominant tree species, as well as the drainage status as such. We will address this in the discussion (after line 525) We would, however, like to remain cautious here as in undrained soils pH was not lowered by conifers (please see specific comment on 300).
There is also an apparent mismatch between opening arguments of conducting this study across the three Baltic states, as they share an ecoclimatic region. I agree, but found the	We agree that the study material is strongly imbalanced among the countries and thus, the insights concerning the country aspect remain limited. However, we would like to

partial focus in the analysis to separate results by country unhelpful. This is strongly biased by the distribution of vegetation and drainage across the study, resulting in limited insights, The presentation of data can be streamlined significantly by removing the "country" aspect throughout.	retain the country aspect, as demonstrating that emissions from comparable sites do not significantly differ between countries provides scientific substantiation that for comparability of GHG inventories in Baltic states, use of different C balance estimation methods are likely inappropriate. For this reason, we believe that retaining the country-level comparison adds value, even though it is not the most interesting or relevant part of the paper.
26-28: The past two sentence should be merged. What you say seems to contradict the previous statements where source and sink behaviours are presented as functions of drainage and tree species. Make it clear how different parameters influence carbon balances without causing contradictions.	We will thoroughly edit the last two sentences and will carefully check the potential contradictions while doing so.
to emphasize 105: Why does the analysis distinguish sites by country? The argument presented is that this is one ecoclimatic zone with site replication across the three Baltic states. From that rationale, national boundaries are arbitrary and the analysis should focus on environmental drivers of biogeochemical patterns. This focusses analysis and removes part of detailed results/discussion.	We fully agree that the national boundaries are arbitrary in this respect, but we need to deal with the country aspect to support the use of the same EFs across the region. The analysis distinguishes sites by country to support the argument presented. Different readers may have different expectations for the paper. We are sorry that the hard work of the referees is increased by the addition of the country aspect, but the need to deal with it comes from the regulations of the GHG inventories.
300 (Fig. 2): I am worried by the confounding effect of drainage and tree species. Table 1 indicates that undrained spruce forests were sampled, but this figure shows only deciduous forest on undrained sites. Looking at pH in particular, the observed difference ascribed to drainage status is caused by undrained sites not including coniferous forest with more acidic litter input. Comparing birch and alder forests only, there is no evident difference by drainage. Looking at Fig. 10, the pH distribution by species and drainage seem to be different to	Thank you for noticing this issue. It concerns Figure 2, where data from multiple sites - including spruce in undrained conditions - were missing. This omission does not affect any other results or figures in the article. We will update the figure. The inclusion of undrained spruce sites do not lower the mean pH in undrained conditions, as the mean pH in undrained spruce sites was 5.14. This may provide additional explanation why mean soil emissions in undrained sites were not significantly lower than in drained ones - we will add this to the discussion. In the undrained sites,

what is shown in Fig. 2 (e.g., drained/Birch has values below 4 in Fig 10, not in Fig. 10; undrained Spruce pH values shown in Fig. 10, not Fig. 2). This has to be clarified, as the discussion has to take account of these patterns and potential confounding influences.	especially, soil pH may be regulated by the inflowing water. Please note that the data points in Figures 2 and 10 do not have to match. Figure 2 presents results at the subplot level to fully reflect the observed variation, while Figure 10 shows site-level mean values. We will further clarify this in the captions.
398: This is not evident from Table 3. aGV is c. 39% of total GV (sum of aGV and bGV) in drained, and c. 49% of total in undrained.	The results in line 398 and Table 3 do not have to match. The values reported in line 398 are based on comparisons using higher-granularity data at the subplot level, whereas Table 3 presents average values aggregated by drainage status. To avoid confusion, we will further clarify in line 398 that the results are derived from biomass comparisons at the subplot level.