Answer to Anonymous Referee #1

Thank you very much for the thoughtful and thorough review. Your detailed feedback is greatly appreciated and will help us improve the manuscript. Please find responses to questions or observations made below.

Question/Observation	Response
The Reference is incomplete on p. 21	Will be revised.
The distinction of hemi-boreal is a bit confusing with other terms, such as cool temperate, Cool Temperate Mist climate region etc. and could be clarified (says on line 50 'between the temperate and boreal zones').	To avoid confusion distinguishing hemi-boreal zone and Cool Temperate Moist climate zone we will edit sentence in L86-89 as follows: "This limitation may have hindered the identification of emission-impacting factors and the ability to quantify their relationships, underscoring the need for more localized studies to address these gaps, particularly in the hemiboreal vegetation zone which partly overlaps with the Cool Temperate Moist climate region (Calvo Buendia et al., 2019) - a subregion of temperate zone as defined by the IPCC." We mention both the Cool Temperate Moist climate region and the hemiboreal vegetation zone because the former, which refers to a broader geographical area, is used in national GHG inventories to classify the Baltic States per the IPCC climate zone definitions. In contrast, the hemiboreal zone more accurately reflects the local conditions within the Baltic region. We consider it important to mention both, as the IPCC climate classification is particularly relevant for readers familiar with national GHG reporting, while the term hemiboreal vegetation zone is more commonly used within the scientific community. Highlighting this overlap helps to describe the conditions in which the Baltic States are situated for a broader audience.
It appears that there was no attempt (or success) to include a comparison of drained and undrained sites, based on the latitude and longitude data in Table S1, though there appear to be two pairs in Latvia (Fig. 1). Please clarify	We did not aim for or attempt such a pairwise evaluation in this study. The mere closeness of undrained and drained sites does not imply that they could be compared as a pair, even if they represented the same site type when the drained site was still undrained. As shown by our results, and some previous studies, there is

	variation in both soil conditions and greenhouse gas emissions among individual sites belonging to the same sites types, making pairwise comparisons questionable. Site type level comparisons are more justified. We will clarify this in the Material and Methods section.
Limited replication of site types means that categorization of type is unwarranted (lines 536 and following	We would like to clarify the interpretation of lines 536 and following. In the corresponding paragraph, we acknowledge the observed variability in soil carbon stock balances; however, based on the evidence, we consider categorization by site type to be an appropriate and ecologically meaningful approach. This assessment is supported by two key observations: first, the site types exhibit distinct and consistent differences in soil properties, as shown in Figure 2, which justifies stratification from an ecological perspective; second, this stratification is further supported by the results—on average, nutrient-richer sites acted as carbon sources, while nutrient-poorer sites showed carbon balanced around equilibrium on average, as illustrated in Figure 9c. To include the clarification in the article, we will revise the sentence on line 537 as follows: "Some uncertainty in the results arises from the inherent variation of study sites categorized into different forest site types and drainage statuses; such variation is natural and cannot be considered erroneous (see, e.g., Westman & Laiho 2000, https://doi.org/10.1023/A:1023348806857, and Ojanen et al. 2010, https://doi.org/10.1016/j.foreco.2010.04.036). However, based on the observed patterns, we consider site stratification by drainage status and site type to be an appropriate approach for interpreting soil C balance. This stratification captures key ecological differences that are relevant to C dynamics and supports meaningful comparisons across site conditions."
In the C balance, the expectation is that this measure (C tons balance etc.) converts into CO2. This maybe the case, but what about other C forms in the C cycle? Methane	We agree that methane emissions and carbon leaching, while scientifically relevant, contribute marginally to the overall soil carbon balance. However, the scope of this

would play an insignificant role in the C balance for most of the sites, given the low water table in most sites, including the undrained ones: probably up to 0.05 t C/ha/yr in the wetter sites and maybe CH4 uptake in the drier sites. Loss of dissolved organic carbon (DOC) would result from leaching of the soil, and may account for up to 0.10 t C/ha/yr additional loss, but also small to most of the soil C balance estimates that have been made.	article is intentionally limited to the soil carbon balance estimation evaluating direct CO_2 emissions as an efflux, as described in the methods section. We will add a short mention on the roles of CH4 and DOC in the Discussion.
The manuscript started with a comment on the use of Emission Factors by the IPCC and states, though no EF values were given. If the objective of the study, beyond the science of the forested systems, was to contribute to a better estimate of the variability and magnitude of EF, it would be useful to see how the authors think these study would contribute to that objective. What 'better' estimate of EF could have been made using the results assembled in the manuscript, with a lot of good, hard work over two years and standardized methods, compared to the 'guesswork' of the past?	Thank you for emphasizing the potential to elaborate further on the article's contribution to greenhouse gas emission estimates. To address this, we will expand the final paragraph of Section 3.1 Soil Carbon Balance by noting that the reported soil carbon balance values can be used directly as EFs, and compare them to the IPCC default EFs. We were a bit shy initially because these results reflect carbon balances only for the study period and are specific to the respective stands in their particular developmental stages and site conditions. They do not represent average changes in soil carbon stocks over longer timeframes, such as an entire forest management cycle. However, that is admittedly the case with all static EFs currently. We will clarify that, emphasizing that the study provides a notable contribution through both the plot-level summary and raw data on soil carbon influx and efflux. The spatial coverage of the study sites, along with the variability in stand characteristics, soil properties, and water table level dynamics, provides input for synthesising dynamic empirical soil carbon balance models that depend on drainage status, meteorological conditions, soil chemistry, and stand-related parameters.
24 It seems that the estimated changes in C do not involve + and – signs. Such as soil C removal from drained Scots pine sites was 2.77 units while C sink occurred in undrained black alder sites there was an average sink of 1.33 units. Throughout the manuscript could 'loss' estimates be given	We apologize for the inconsistency and confusion and will standardize these.

a negative sign (e.g. -2.77 +/- 0.36 units) and 'gain' estimates be given a positive sign (1.33 +/- 0.72 units. The graphs showing the 'C balance' (Fig 9 and 10) include negative values, please be consistent. The notation used in Figures also varies: for example Fig 8 has 'Carbon flux' and 9 and 10 have 'C balance' with the same units and meaning. Please standardize.	
15 the boreal region	Will be revised.
35 why not use 'faster' rather than 'higher' to describe a rate?	Will be revised to faster.
46 One of the studies was on a drained peatland used for horticultural crop production, so is not representative of the types used in the EF estimates.	We have to retain these references, as they are cited as sources used in the development of the IPCC default Tier 1 emission factors. We double checked https://doi.org/10.1029/93GB00469, bot crop and forest covers are scope of the article.
83 Jauhianen et al. was incompletely cited in the References.	Will be revised.
164 how 'small' was insignificant?	We will replace "insignificant" with "small (<5%)".
460 Basal area had the strongest correlation with C balance, yet in Fig. 10, the R2 of 0.14 was the smallest in the 6 graphs, several with p values < 0.01. Please check. It would be good to include the slope of the regression to indicate how much change in C balance was created by a change in the independent variable. For example, a reduction in pH from 6 to 2.5 (!) would result in a C balance (gain) of about 3 units. An increase in bGV of 0.1 to 3.1 units would result in a C balance of -1 to 4 units.	Since basal area showed the strongest correlation among the stand variables, it is the only stand variable presented in Figure 10. Therefore, the corresponding R^2 value does not have to be the highest. We will add slope values.