

Dear Reviewers,

We sincerely thank you both for your very helpful comments and contributions.

We think you have highlighted several important points and clarifications needed and we have now addressed your comments to the best of our ability. All our responses are written in blue text, please see below.

Yours sincerely,

Andreas Hagenbo – *on behalf of all co-authors*

List of changes

- Added a workflow flowchart (Fig. S1) illustrating the main analytical steps and methodological pathways.
- Clarified how dominant tree species were assigned within MODIS pixels.
- Added details on the predictors used in the stand-level growth and yield models.
- Expanded the description of MODIS LAI processing, including temporal aggregation and quality filtering.
- Clarified that SR16 LAI is derived from allometric equations rather than remote sensing observations.
- Added justification for using species-specific SLA values instead of MOD17 biome-level parameters.
- Revised the GPP methodology section to clarify the distinction between the MOD17 framework and our reconstruction of GPP from independently estimated NPP and respiration.
- Replaced discussion of “circularity” with a discussion of uncertainty propagation among linked carbon flux components.
- Added discussion of alternative GPP products (e.g. GOSIF-GPP, NIRv-GPP, GPP-net) and their potential advantages and limitations.
- Clarified the temporal extent of all major datasets (MODIS EURO NPP, MODIS LAI, SR16).
- Explicitly state that the study should be interpreted as a structural comparison among methods rather than a temporally explicit carbon budget for a specific year.
- Added discussion of the implications of temporal mismatch among datasets.

- Added discussion of respiration acclimation to warming and its potential implications for GPP and CUE estimates.
- Added discussion of site index as a potential confounding factor in age–NPP and age–CUE relationships.
- Clarified limitations arising from incomplete representation of fertility and vegetation type in SR16.
- Added MODIS EURO spatial resolution in arcseconds ($\approx 30 \times 30$ arcseconds).
- Corrected the sign error in the deciduous forest residual calculation.
- Corrected terminology and wording throughout the manuscript (e.g. SOC definition, autotrophic respiration abbreviation, inconsistent phrasing).
- Harmonized SLA reporting order and other methodological descriptions for consistency.
- Revised figures to improve comparability and adopted colour-blind-friendly palettes.
- Corrected several minor technical and typographical issues identified by the reviewers.
- MODIS was spelled as “Modis” in several figures. This has been corrected.

Reviewer #1

General comments:

As nicely stated in the Introduction section, the study addresses a very relevant topic, as fine-roots are an important component of total net primary production (NPP) and input for soil organic carbon (SOC). As direct measurements are challenging, commonly models are used to derive fine root biomass production and turnover. The authors examine how the varying assumptions underlying the different models influence estimates of NPP and carbon use efficiency (CUE). Again, this is a very relevant topic in terms of carbon budgeting such as greenhouse gas accounting. The authors present ways to calculate and consequently compare fine-root biomass and production and CUE from different sources (MODIS NPP, Norwegian forest resource map), which is a challenging task. This might also serve as a guide for similar studies in other countries. Overall, the study is well presented and limitations are clearly stated. The study is not always easy to read/follow, which is however more related to the complexity of the topic rather than the writing itself. There are some minor concerns regarding the calculation of MODIS GPP and some clarifications are needed for some other calculation steps in addition to some minor technical corrections. Overall I congratulate the authors on a well-designed study on a very relevant and challenging topic!

Thank you very much for your kind words!

Specific comments:

Methods

Lines 127-129: When aggregating the SR16 grid cells, how was the dominant tree species identified?

Info added, lines 129 – 130:

“Within each MODIS pixel, we calculated the proportion of SR16 grid cells classified as spruce-, pine-, or deciduous-dominated. The tree species with the highest proportion was then assigned as the dominant species for that MODIS pixel.”

Lines 132-134: Maybe some more detail regarding the stand-level growth and yield models could be added, i.e. which predictors (height, diameter?) are used to predict the annual changes in height, volume, etc.

Thank you for your comment, the predictor variables are now stated in the text, line 136 -137:

“Predictions are based on forest age, the height of the 100 thickest trees per hectare, and site index, which is defined as the average height of the 100 thickest trees per hectare at a reference age of 40 years.”

Lines 142: LAI from MODIS. MODIS provides 8-day LAI values. Which value was used or how was the 8-day data aggregated, i.e. was just the maximum LAI used or the LAI on a specific day or the mean LAI over the whole year? In case of SR16 it is LAI_{max}, correct?

We used 8-days LAI values that we aggregated. We have added a description now on how this was done. Lines 147 -151:

“LAI from MODIS was obtained from the MCD15A2H 8-day product (Myneni et al., 2021), which was the most recent operational MODIS (combined Aqua + Terra) LAI product available at the time of analysis. We used high-quality observations from the full record overlapping with the MODIS Euro NPP dataset (i.e. 2003–2012). For each of the 45 annual 8-day periods, the maximum LAI value across the 10-year record was extracted for each pixel, and these values were averaged to produce a single annual (MODIS) LAI estimate representing mean seasonal maximum conditions.”

Please note that the SR16 LAI estimate does not represent remotely sensed LAI_{max} in the same way as the MODIS product. Instead, SR16 LAI was derived from allometric equations, line 125:

“Since SR16 LAI is derived from allometric equations (Marklund, 1988), rather than remote sensing observations...”

Lines 148-150: The SLA used in the MOD17 algorithm to arrive at NPP from GPP for EvergreenNeedleLeaf ENF, DeciduousBroadLeaf DBF, and MixedForest MF are 21.1, 26.2 and 21.5 m² kg⁻¹ C respectively. Especially in case of ENF (reflecting Pine and Spruce) this value is way higher than the ones used by the authors based on literature. I.e. the authors will arrive at a different foliage biomass compared to the one that was used in the calculation of MODIS NPP (this also relates to the comment below regarding the calculation of GPP). The same, but probably to a lesser extent, is also true for the used turnover rates.

We acknowledge that the SLA values used in the MOD17 algorithm differ from the species-specific SLA values applied in this study. However, our objective was not to reproduce the internal parameterization of MOD17, but to derive foliage biomass from LAI using species-specific SLA values representative of the dominant tree species in Norway. These values (9.9, 12.4, and 27.1 m² kg⁻¹ C for spruce, pine, and birch, respectively) are based on empirical studies and better reflect species-level variation than the generalized biome-level parameters used in MOD17. We have clarified this in the revised manuscript, lines 160 - 161:

“These species-specific values differ from the generalized biome-level parameters used in the MOD17 algorithm and were selected to better represent species-level variation in Norwegian forests.”

Lines 198-247: Calculation of GPP. As the MODIS Europe dataset does not provide GPP but only NPP, the authors “employ a similar approach to calculate GPP as the MOD17 algorithm, but with some modifications”. However, they give the impression that the MOD17 algorithm first calculates NPP and then adds Ra to arrive at GPP but it is the other way round. MOD17 first calculates 8-day GPP based on the radiation conversion efficiency concept using fraction of photosynthetic active radiation (FPAR) and climatic information (but not using LAI for instance). Finally, from the annual GPP (the sum of the 8-day GPP) the respiration Ra is removed to arrive at annual NPP. So rather than $GPP = NPP + Ra$ it is $NPP = GPP - Ra$ (later in section 4.4 Methodological limitations they also state that “Remote-sensing derived GPP often relies on NPP and autotrophic respiration, which again is at least not true for MODIS GPP”). I think this detail is important, as by modifying the way how Ra is calculated (e.g. using a different SLA), the authors will consequently arrive at different MODIS GPP values than the ones that were used in the actual calculation of MODIS NPP which in turn will affect the MODIS CUE values.

Again, as MODIS Europe only provides NPP and not GPP there is a good reason for back-calculating GPP, but I think the author’s should be aware of this and that they should mention it in the text.

Thank you for highlighting these issues! Our intention was not to suggest that MOD17 derives GPP from NPP, and we agree that the original wording was imprecise. We have revised the Methods section to clarify this point. We now write this at lines 209- 214:

“The estimated tree NPP, fine root NPP and understory NPP, were summed to represent total NPP (g C m⁻² yr⁻¹), hereafter referred to as SR16-based NPP. Both SR16-based NPP and MODIS Euro NPP were used in the calculation of GPP (g C m⁻² yr⁻¹), to assess sensitivity to the choice of NPP source. We employed a C balance framework conceptually consistent with the MOD17 algorithm, but with modifications. The MOD17 algorithm estimates GPP using a light-use efficiency model and subsequently derives NPP by subtracting autotrophic respiration (Ra). Ra is estimated using a combination of biome-specific parameters from the Biome Property Look-Up Tables (BPLUT) and temperature- and biomass-dependent functions. In contrast, our approach reconstructs GPP from independently estimated NPP and Ra components...”

Now we highlight in the text what MODIS first assess GPP and then NPP and that we instead, in contrast, employ a C balance reconstruction of GPP.

Regarding the methodological section (4.4), we have also revised the wording to avoid the term “circular dependency” and instead emphasise the propagation of uncertainty through linked C flux components. The revised text reads (line 513 – 518)

“Estimates of CUE are often contentious due to the propagation of uncertainties associated with methods for calculating NPP and GPP (De Lucia et al., 2007; Jin et al., 2025; Vicca et al., 2012). GPP, NPP, and autotrophic respiration (Ra) are linked through C balance relationships and are therefore not independent. Consequently, uncertainties or parameter choices affecting one component propagate to the others, influencing derived CUE estimates. This is particularly relevant for satellite-based GPP products, which are typically derived using light-use efficiency approaches driven by fAPAR and meteorological data (Justice et al., 2002; Running et al., 2004).”

Following, this text we now also discuss alternative GPP products as highlighted by reviewer #2

Results

Figures 4 and 5: Site index seems not to be equally distributed over forest age, i.e. forests on more productive sites (higher Site index) seem to be younger than forests on less productive sites (probably due to forests on productive sites being more likely managed for timber production). Is this true and if so, do the authors think that the found relationships also reflect a Site index effect in addition to an age effect?

Indeed, the data display a degree of skewness, likely stemming from high productive sites having a higher probability of management, as you point out. As a result, we acknowledge that the relationships shown in Figures 4 and 5 may partly reflect a confounding effect of site index in addition to forest age. For transparency reasons, this is why we decided to highlight site index in the figure.

Initially, we were considering integrating out the effect of site index, but since that would artificially narrow the range of estimates in NPP and CUE, i.e. contributing to lower NPP at fertile sites and higher NPP at infertile sites, we decided to refrain from adjusting the data.

Another problem with site index is that it has limited integration in our analysis, stemming from no information about vegetation types in the SR16 data. This is the main reason why we are not explicitly drawing conclusions about site index effects on the results.

To clarify, since vegetation type is a proxy for site fertility, if vegetation type were available in the SR16 data, we could apply variable ratios of fine-root-to-foilage biomass (as listed in Table 1) to better represent variability in fine root biomass. We aim to address this in future analyses by developing a predictive model of vegetation type based on NFI data, which can then be upscaled to individual SR16 pixels.

We are grateful for you highlighting this limitation, which is now described in the manuscript, line 550 - 555:

“Variation in fine-root-to-foilage biomass ratios across vegetation types can be used to represent fertility-related differences in fine root biomass, with vegetation type serving as a proxy for site fertility. However, as vegetation type is not currently available in the SR16

dataset, we were unable to fully integrate site index and fertility into the analyses, and we therefore treat site index as a descriptive variable. As a result, incomplete representation of fertility means that some of the observed forest age-related patterns may also partly reflect variation in site index, which is linked to both site fertility and likely management-driven differences in stand age structure.”

Technical corrections:

Abstract

Lines 17 to 21: first “Mean NPP ... from 318 to 243 g C m⁻² yr⁻¹ across young to mature forests” is reported, but then for “CUE ... from 0.30 to 0.63 ... with lowest values in mature”. Probably change the second part to “CUE ... from 0.63 to 0.30 ...” for consistency.

Yes, that is better, thank you. Done.

Introduction

Line 33: “soil organic (SOC)” ... should this be “soil organic carbon (SOC)”?

Yes, carbon was missing before. This is now fixed.

Line 68: maybe add (RA) after “autotrophic (plant) respiration”

Done.

Line 91: “However, growth predictions are sensitive to CUE, ...”. The However seems to not really fit here, as this sentence is not contradicting or contrasting the previous one. Maybe just remove it and simply start with “Growth predictions are sensitive ...” or “Further, growth predictions are sensitive to CUE, as for instance ...”

Very true. “However” removed. We think the text flows better without it .

Methods

Lines 148-150: First SLA in m² kg⁻¹ is reported the order spruce-pine-deciduous, but then SLA in m² kg⁻¹ C is reported in the order pine-spruce-deciduous. Same order should be used for consistency.

Done.

Results

Figure 1: The same scale on the vertical axis should be used for all 15 panels (or at least for the 5 panels within one column) as otherwise the visual comparison of bars can be quite misleading. Maybe the color palette could be improved to be color-blind friendly.

We have revised the figure so that panels a) and b) now share the same axis scale to improve comparability. For panel c), a different scale is used because residuals are shown as root mean squared residuals, meaning all values are positive after squaring. This transformation was applied to avoid positive and negative residuals cancelling each other out, which would otherwise underestimate overall model error.

However, to adhere to your comment to the best of our ability, we have adjusted the axis range in panel c) so that the numeric range of the axis is of similar order of magnitude as panel a) and b). Specifically, the range for a and b are now both -200 to 350 and the range of c is 0 to 550.

We have also made figure 1, 2 and 6 all colour-blind friendly; instead of red, green and blue (Spruce, pine and birch, respectively) we now use dark-blue, ochre and magenta for the different tree species colours. No change done to the correlations plot as we use the Viridis colour pallet, which we understand is more colour-blind friendly.

Reviewer #2

I apologise to the editor and to the authors for my delayed response. I read the manuscript titled "Contribution of fine roots on carbon allocation patterns in Norwegian forests" by Hagenbo et al., and I have the following comments and suggestions for improvement. Overall, I find the study to be a valuable contribution to our understanding of fine-root contributions to boreal forest carbon budgets, and I appreciate the authors' comprehensive approach. However, there are some areas where clarification and additional analysis could strengthen the manuscript.

Thank you very much for your valuable input!

Major points

1 - Given the complexity of the methods, I suggest adding a clear diagram (e.g., a flowchart) that maps the different pathways for estimating foliage biomass and fine root production across the three methods. This would help readers grasp the workflow and method-specific differences more quickly. In the same direction, I also suggest restructuring the Methods section to reduce repetition of shared steps and emphasise the differences among the three approaches. For example, the manuscript could include a main workflow subsection for shared components (tree NPP calculation, understory NPP, and GPP/CUE calculation), followed by a separate subsection detailing how foliage biomass and fine root production are estimated in each method. This would reduce redundancy and improve clarity.

We have now included a flowchart illustrating the overall workflow and the main methodological steps. During the development of this figure, we initially attempted to include all the dataset-specific branches; however, this resulted in a figure that was overly complex and difficult to interpret. We therefore present a simplified version focusing on the main analytical steps from start to finish.

Since the flowchart is not space-efficient we have decided to put this into the Supplementary Information in the manuscript. This is the new figure with its caption. Please note that we have indicated by numbers the section in material and methods that the flowchart refers to:

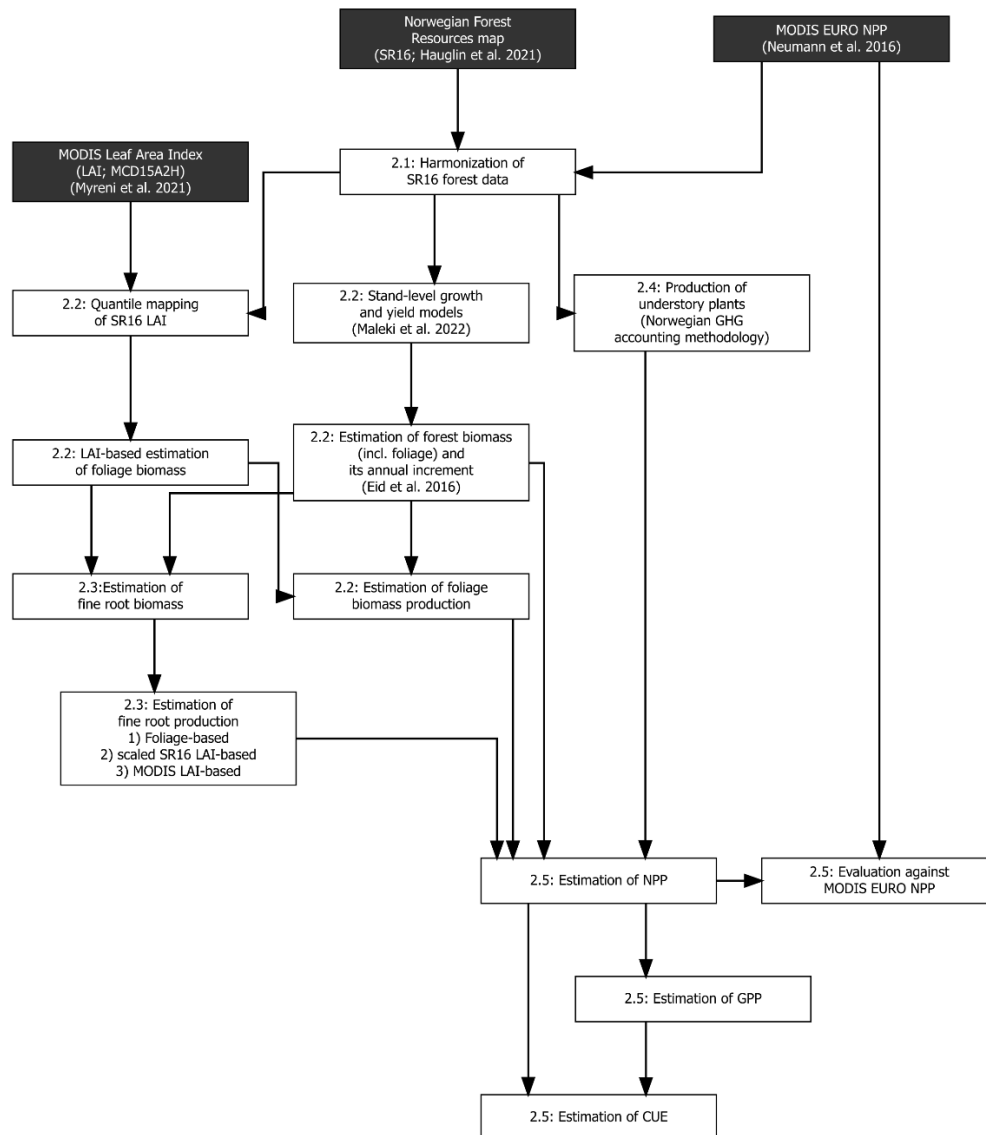


Figure S1: Flowchart of the workflow used to derive estimates of net primary production (NPP), gross primary production (GPP), and carbon use efficiency (CUE) of Norwegian forests. Filled boxes represent the main datasets used in the study, whereas open boxes indicate the different methodological steps. Numbers within the boxes refer to the corresponding sections in the Materials and Methods.

We have also added several clarifications to materials and methods and we think with these changes combined the material and methods section is easier to follow.

2 - Temporal consistency of remote sensing inputs and implications of missing respiration acclimation:

I suggest a dedicated clarification paragraph in Methods (and a short caveat in Discussion) on the temporal representativeness of all remotely sensed/model inputs. As currently described, the workflow appears to combine products from different temporal frames:

- MODIS EURO NPP comes from Neumann et al. (2016), which is a temporally continuous product for 2000-2012.
- MODIS land cover is from MCD12Q1 (2000), and LAI/FPAR is from MOD15 Collection 5.
- SR16 is based on national airborne laser scanning initiated in 2015.

This suggests the analysis is primarily a cross-sectional/fused estimate rather than a strictly year-specific carbon budget. Please state explicitly whether outputs should be interpreted as: (i) representative of a specific year, (ii) a multi-year climatological synthesis, or (iii) a structural comparison among methods independent of a single target year.

Yes, this is an important detail and we now more explicitly highlight the different temporal extents of the datasets in the methods section. However, my co-author pointed out that we align the MODIS LAI and MODIS Euro NPP better than previously described, which was incorrect,. Now we write, at line 144 -149:

“To explore and compare both approaches, we used the foliage biomass estimates from the models of Eid et al. (2016) and LAI from MODIS and from SR16, the latter derived from NFI data for 2007–2015 (Majasalmi et al. 2018). LAI from MODIS was obtained from the MCD15A2H 8-day product (Myneni et al., 2021), which was the most recent operational MODIS (combined Aqua + Terra) LAI product available at the time of analysis. We used high-quality observations from the full record overlapping with the MODIS Euro NPP dataset (i.e. 2003–2012).”

Moreover, our analysis should be viewed as a “structural comparison” between methods and we now clarify this in the methodological considering section, in the discussion. In that section we also address the implication of the different temporal extents. Line 556 -565:

“A further limitation is that the datasets used in this study are not temporally aligned. The MODIS Euro NPP data used in this study cover the period 2003–2012 and are therefore temporally aligned with the MODIS LAI (MCD15A2H) data, which were derived from observations over 2003–2013 and averaged to produce a single representative estimate. However, the SR16 forest structure data and associated LAI are based on airborne laser scanning campaigns initiated from 2009 onwards, resulting in a temporal discrepancy between the remote sensing and forest inventory datasets. Consequently, the analysis combines information from different time periods and should be interpreted as a structural comparison among alternative approaches to estimating forest C fluxes, rather than as a temporally explicit C budget for a specific year. Despite this temporal mismatch, the similarity between results derived from the two LAI-based approaches (MODIS and SR16) suggests that its influence is limited relative to the differences between the LAI modelling approaches, likely stemming from the quantile mapping applied to align the SR16 LAI distribution with that of MODIS.”

Respiration acclimation concern:

The GPP framework uses fixed/instantaneous temperature responses (e.g., $Q_{10} = 2$ for live wood and fine roots; foliage Q_{10} from the Tjoelker et al. equation), but does not appear to include explicit longer-term thermal acclimation of respiration parameters. In a rapidly warming high-latitude system, this could bias respiration upward relative to acclimated responses. The literature indicates that acclimation can dampen expected increases in respiration under warming, including in boreal/temperate trees (Reich et al., 2016; Huntingford et al., 2017; Zhang et al., 2025). Some potential implications for this study are: Maintenance respiration (R_m) may be overestimated under warmer conditions. Because GPP is computed as $NPP + respiration$ terms, GPP may be biased high if R_m is biased high. CUE (NPP/GPP) may therefore be biased low, especially in warmer subsets and potentially in age/species groups that differ in temperature regime. Apparent age/species gradients

in CUE and inferred fine-root allocation fractions may partly reflect differences in respiration parameterisation rather than solely biological allocation differences.

Suggested improvement:

- Add a sensitivity analysis with an acclimated respiration scenario (for example, temperature-dependent downregulation of reference respiration and/or acclimated Q10) and compare impacts on GPP, CUE, and fine-root C allocation.
- At minimum, quantify likely bias direction and uncertainty range introduced by non-acclimating respiration assumptions.

Thank you for raising this point. We acknowledge that plant respiration may change under a changing climate. This is related to the temperature sensitivity of respiration, which is represented in both the MODIS algorithm and our analyses. However, as you note, studies show that this temperature sensitivity itself may adjust through thermal acclimation, thereby dampening the increase in respiration expected under warming. For example, Reich et al. (2016) reported only an approximately 5% increase in respiration under a 3.4 °C warming, compared to an expected ~23% increase without acclimation.

In the present study, however, our objective was not to assess climate or warming effects, but to evaluate how different representations of foliage biomass and fine root production influence estimates of NPP, GPP, and CUE. In short, the analysis is focused on a quantitative assessment of how alternative fine root methodologies affect regional estimates of NPP, GPP, and CUE, rather than on the climate sensitivity of forests.

Incorporating acclimation would require additional assumptions about the magnitude and functional form of temperature adjustment. We recognize that there is a rather large body of work on the parameterisation and constraining of acclimation responses, before for such analyses are warranted for our tree species and Norwegian forests. While studies such as Reich et al. provide a useful starting point for modelling acclimation effects, we are also concerned about translating species-level acclimation responses (derived from individual trees) to stand and MODIS pixel scales. Because the focus of our study is not on the climate sensitivity of forests, and concerns regarding our options for robust parameterisation of respiration acclimation response, we decided to retain the standard Q10-based approach, without acclimation to maintain consistency with the MOD17 framework, and to preserve the main objective of our study rather than shifting the focus towards the impacts of a changing climate.

However, we agree that the analysis you suggest is both relevant and timely. We see clear value in exploring acclimation effects in future work, particularly in the context of climate change impacts on boreal forest carbon dynamics. Based on your comment, we have now added a short text describing this in the Discussion, line 563 -570:

“While our analyses integrate data from different time periods, they also do not explicitly represent longer-term temporal responses in respiration. Maintenance respiration was calculated using fixed temperature sensitivities (Q10), consistent with the MOD17 framework. However, experimental evidence suggests that plant respiration can acclimate to sustained warming, reducing its temperature sensitivity over time and thereby dampening the temperature-driven increase in respiration (Reich et al., 2016). Accounting for such acclimation could influence estimates of GPP and CUE and may provide insight into potential responses of forest C cycling to climate warming. However, further empirical work on

respiration acclimation across boreal tree species, and its scaling from individual trees to stand and regional levels, is needed to better constrain and evaluate such approaches.”

3 - In Section 4.4, the discussion about remote-sensing GPP products is somewhat too general. It is correct that many widely used products are linked, directly or indirectly, to model formulations that include respiration and/or NPP closure, but this is not true of all current GPP products. For example, GOSIF-GPP is a SIF-based product derived from OCO-2 solar-induced fluorescence and empirically calibrated against eddy-covariance GPP, rather than being calculated as NPP plus modelled autotrophic respiration (Li & Xiao, 2019). In addition, Wang et al. (2026) proposed GPP-net, a Sentinel-2-based deep-learning framework that estimates GPP from surface reflectance and PAR, trained against flux-tower GPP, and designed to minimise dependence on land-cover parameterisation and coarse meteorological inputs. This distinction matters here because one of the key limitations identified by the authors is the circularity introduced when GPP is reconstructed from NPP and respiration terms using a MOD17-like framework. Using external products such as GOSIF-GPP and GPP-net could therefore substantially simplify the analysis: they allow benchmarking of modelled GPP without explicitly prescribing maintenance respiration and growth respiration in the GPP calculation, reducing dependence on fixed Q10 assumptions and the associated acclimation problem raised above. In that sense, comparison with one or both products would provide a more independent test of whether the reported CUE and age-related patterns are driven by the fine-root/NPP formulation or by the respiration parameterisation used to back-calculate GPP. At the same time, I would not present these products as complete solutions, because they introduce their own uncertainties (for example, SIF-GPP transferability and gap-filling for GOSIF, and model transfer/generalisation uncertainty for data-driven products), and GPP product uncertainty can differ in northern ecosystems (Li & Xiao, 2019; Marsh & Zhang, 2022). Still, I think the manuscript would be stronger if the authors explicitly discuss these alternative GPP product classes and, if feasible, compare their MOD17-like GPP estimates against GOSIF-GPP and/or GPP-net as an additional sensitivity test.

Thank you for raising this point and for highlighting additional GPP products. These approaches are indeed interesting and relevant from the perspective of deriving more independent estimates of CUE and reducing some of the uncertainty propagation associated with MOD17-type frameworks.

In response to Dr. Pucher’s previous comment, we have revised this part of the manuscript and removed the wording related to “circularity”. Instead, we now focus on the propagation of uncertainty associated with estimating NPP from GPP, or reconstructing GPP from NPP and respiration terms, as done in our framework. We would also like to clarify that our intention was not to critique remote-sensing GPP products in general, but rather to highlight methodological limitations associated with MOD17-like approaches, including our own implementation. We acknowledge that the GPP products you mention could provide more independent estimates of GPP, and consequently CUE,

In addition to these approaches having their own uncertainties, we recognise that they also have some limitations within the scope of our study. Regarding GOSIF-GPP, it has a resolution about 5.6 km, which mean that we would further aggregate the SR16 data in our analyses, which would result in increasingly more forest stand structure variation being collapsed, making data inference more challenging. Regarding, the GPP-net model, we think this model is very interesting because of its relatively simple input requirements. Being based on Sentinel-2 data it should also be able to provide fine resolution GPP estimates, relevant for this type of our analysis. However, to the best of our understanding, GPP-net is not yet publicly available and still requires further development, as also noted by Wang et al. (2026). We therefore consider such approaches highly relevant for future analyses.

The primary objective of this study was to evaluate how alternative representations of foliage biomass and fine root production influence estimates of NPP, GPP, and CUE, rather than to derive independent CUE estimates from various GPP products. This required a consistent framework in which respiration and belowground carbon allocation could be estimated comparably across the different fine-root approaches. In addition, the use of Norwegian SR16 forest data and region-specific biomass parameterisations was essential to better represent Norwegian forest conditions, extending beyond the standard MOD17 procedure.

We therefore retained the MOD17-consistent framework to ensure methodological consistency and to maintain focus on fine-root representation and forest carbon allocation. Nevertheless, we agree that the use of independent GPP products would be an interesting direction, and we now alternative GPP products in the Discussion (Lines 519–527).

“Alternative GPP products also exist, including GOSIF-GPP derived from Orbiting Carbon Observatory-2 observations of solar-induced chlorophyll fluorescence (Li and Xiao, 2019), NIRv-GPP based on near-infrared reflectance of vegetation (Badgley et al., 2019), and more recently GPP-net, which is a predictive deep-learning model trained on Sentinel-2 data (Wang et al., 2026). These approaches are less dependent on C-balance closure assumptions and may therefore provide more independent estimates of GPP. Combining such GPP products with MODIS Euro NPP could consequently enable more independent calculations of CUE. However, uncertainty related to transferability and model performance in northern ecosystems remains an important consideration for remote-sensing-based GPP products (Li and Xiao, 2019; Marsh and Zhang, 2022). In the present study, we instead retained a MOD17-consistent framework to enable comparison of respiration and belowground C allocation across the different fine root modelling approaches.”

Minor points

Line 121: I suggest reporting the resolution in arcseconds, which in this case would be 30 arcseconds, right?

Yes, info added.

“The MODIS EURO data has a pixel size of about 30 x 30 arcseconds ($0.0083^\circ \times 0.0083^\circ$), with a spatial reference system of WGS 1984, and represents the period 2000–2012.”

Line 273: There appears to be a semantic inconsistency in the description of the residual sign. The text states that the foliage-based method underestimated production in both pine and deciduous forests, yet the reported median residual for deciduous forests is positive (+65.1). If residuals are defined as the difference between the study estimate and the reference/meta-analysis value, then a positive residual would indicate overestimation rather than underestimation. Please check the wording or the residual sign convention.

You are referring to this part:

“However, this method underestimated production for pine and deciduous forests, with median residuals of -79.8 and +65.1, respectively.”

This is wrong. Deciduous should have a negative sign. Thank you for pointing out this error. This has been corrected. We also noted that residual of biomass was expressed as a flux ($\text{g C m}^{-2} \text{ yr}^{-1}$) in the text above. This has been corrected.