

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!

Dear Dr. Pucher,

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.