# Response to Reviewers

## Long-term nitrogen fertilization increases drought sensitivity of gross primary productivity capacity in a boreal Scots pine forest

## Cover response

Dear Editor and Reviewers,

We sincerely thank the reviewers for their careful and constructive assessments. We will use the comments to strengthen the hydrological description, distinguish observed evapotranspiration (ET) from the eddy covariance tower measurements, qualify our interpretation of drought sensitivity, and clarify the eddy covariance and statistical methods. More importantly, we accepted Review1's suggestion that we revise the drought detection methodology. We will remove the P_anomaly; instead, we will use the WAI as a continuous measure of drought stress throughout summertime.

### Reviewer 1

**Reviewer comment: a. The authors have access to soil water content observations, but go at length to define a model-based Water Availability Index (WAI). The formulation is based on precipitation, a poorly describe ‘recharge’ and simulated evapotranspiration (ET). The authors do not describe the water system in the root zone. How deep is the groundwater (close to the river)? Do the trees have access to the ground water? How is the topography oriented with respect to the river, and does this cause lateral or vertical groundwater flows?**

**Response:** We agree that the original hydrological description was insufficient. Moreover, WAI is not the output of a fitted or trained predictive model, and it is a deterministic water-balance index calculated sequentially from measured precipitation and meteorological inputs using different equations and fixed literature-defined constants. No WAI parameters were estimated against SWC, so calibration fit, prediction error, and conventional model goodness-of-fit are not applicable. Continuous SWC is directly observed at a single point, but at each depth, a single fixed sensor location per site is used and may therefore reflect local soil heterogeneity. In contrast, WAI is driven by above-canopy precipitation and site-level meteorological forcing, providing a broader spatial representation of antecedent water availability. We do not refer to WAI as a direct ecosystem-scale measurement. It remains a simplified site-scale calculation that cannot resolve horizontal or vertical soil-moisture heterogeneity. The two indicators are therefore complementary. Basically, the Rosinedalsheden sites lie on a flat, deep sandy deposit, and the water table is at least 10 m below ground (Marshall et al., 2023), making direct groundwater uptake unlikely. Regarding the lateral flows, we actually don't have detailed hydrological subflow data for this site, as this is beyond the scope of our Eddy Covariance flux measurements. There is likely some lateral flow towards the river at depths of tens of meters but probably without any implication for the surface-atmosphere exchanges. Last, the orientation can be found below:

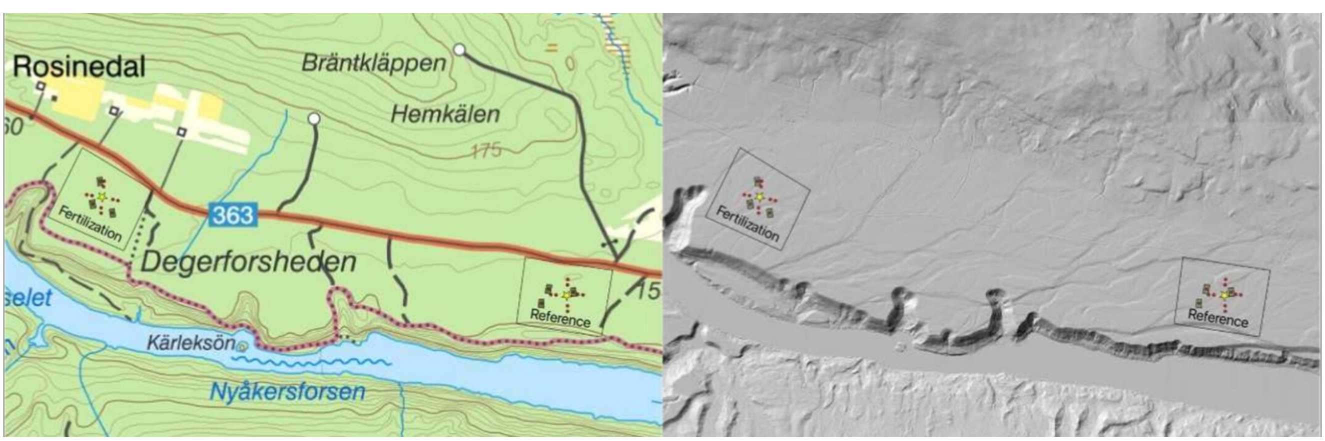


**Figure 1 Study area and its orentation to the revier (Marshall et al., 2023, Figure 1).**

**Reviewer comment: b. It is unclear if precipitation was measured at one or both sites.**

**Response:** The fertilized site has continuously observed above-canopy rainfall, while above-canopy rainfall observations at the reference site began in the summer of 2017. At both sites, the old weather stations were replaced with new ones in the summer of 2017. All missing precipitation data were filled using contemporaneous half-hourly precipitation records from the nearby SITES Svartberget–Degerö meteorological station. We will revise the Methods to clarify how site precipitation observations are derived.

**Reviewer comment: c. The authors argue that the nitrogen-induced biomass changes could cause differences in ET, but it is unclear if and how this is represented in the modelled ET.**

**Response:** Thanks for pointing out the issue. We calculated PET to estimate potential water demand from the atmosphere. We will further assess ecosystem water loss using actual ET, derived from eddy-covariance latent heat flux. We will also compare the ET from the two sites, along with SWC and WAI, to better characterize the hydrological differences due to Nitrogen fertilization.

**Reviewer comment: d. It is unclear why the authors use a model formulation for ET, while they have Eddy Covariance measurements to quantify it, which would be a much more direct quantification.**

**Response:** We will further use the eddy tower-derived ET for the statistical comparison between the fertilized and reference sites. The calculated PET was used because the WAI formulation requires an estimate of atmospheric evaporative demand. PET and EC-derived actual ET represent different quantities actually. The PET describes the evaporative demand that would occur without water limitation, whereas EC-derived ET represents the water actually transferred to the atmosphere under the actual vegetation and soil-water conditions. Thus, EC-derived actual ET cannot be described as a validation target for PET, and PET will not be interpreted as a prediction of observed ET. So, we will revise the Methods, Results, figures, and captions to consistently distinguish between EC-derived actual ET, used to quantify ecosystem water loss.

**Reviewer comment: e.They do not assess how well the WAI formulation works. The modelled WAI is quite different in shape than the Soil Water Content observations  (see figure below, based on synchronisation of Fig. 2 and Fig. S3). It seems that transitioning from moist to drought conditions occurs at different  times. It is unclear why.**

**Response:** Thanks for pointing out the asynchronous issue. The mainly reason is that Fig.2 is based on the summer period (June–August), but the Fig.S3 is an annual SWC time series, with the summer periods indicated by shading. We will revise Fig. S3 to include a summer-only representation that covers exactly the same dates as Fig. 2.

**Reviewer comment: f. In general, though, I think it is fair to say that WAI is similar at both sites and the differences, though statistically signficant, are probably within the uncertainty range.**

**Response:** We agree that statistical significance alone does not demonstrate an ecologically important difference in water availability because of the gap-filling precipitation from the nearby station can also contribute to the difference. So, we will also discuss uncertainty arising from precipitation gap filling, calculated PET, the assumed water-storage capacity, and the simplified recharge and drainage formulation. We will not interpret it as evidence of a strong fertilization effect on water availability. Additionally, EC-derived ET, observed SWC, and calculated WAI will instead be presented as complementary evidence to show the hydrological differences between sites. Last, our interpretation will be revised accordingly.

**Reviewer comment: 2. The authors observe and conclude that nitrogen fertilisation leads to a stronger drought sensitivity,**

**which would lead to an offset in carbon sequestration in drought years. I do not agree with this view. Figure 2 shows that GPP2000 is nearly always larger at the reference site. Figure 3 even suggests that the ratio between GPP2000 at both sites is constant. In my view, the scientific literature refers to an offset in the carbon uptake if drought induces such a large decrease in uptake (or an increase in release) that the beneficial effects of gradual climate warming or nitrogen fertilisation, in this case, disappear: the site turns into a source of carbon. The results in this paper show that the GPP2000 is larger at the fertilised site and that in drought situations the GPP2000 declines from a maximum to zero. But both sites follow a similar pathway and hence the fertilised site still has a larger carbon uptake. This is not what is generally meant with the term 'offset' or even 'sensitivity'. I do not imply that the authors cannot report these results. They can of course, but I object to the interpretation as such.**

**Response:** We agree that our wording was too strong. Our analysis concerns GPP2000, not the annual net ecosystem carbon balance, and it does not show that the fertilized stand became a carbon source or that its annual GPP fell below that of the reference stand. We will replace "offset" with "reduce". Our intended use of "sensitivity" referred specifically to the absolute response of GPP2000 over a common WAI range across sites. Because WAI is expressed on the same 0–100 scale at both sites, the larger decline in GPP2000 at the fertilized stand as WAI decreases from 100 to 0 reflects a larger absolute response in GPP2000 per unit decrease in WAI. This does not necessarily imply a larger proportional response. If the ratio between the two sites remains approximately constant, the relative sensitivity may be similar even though the absolute decline is larger at the fertilized stand. Thus, we will interpret the result as a drought-associated reduction in the absolute fertilization benefit, not as an offset or reversal of carbon sequestration. The fertilized stand is expected to maintain higher GPP2000 across the observed WAI range.

**Reviewer comment: 3. The authors explain the feedback mechanisms between fertilisation, growth, leaf area, photosynthesis and ET, but omit to consider the response of ET and its feedback on the water availability into account. Actually from the Fig. 2 it looks like the WAI is not that different between the sites, apart from a small offset, so the strength of the feedback may be small. The authors do not ask this research question explicitly, but it would strengthen the paper to assess the system as a whole**

**Response:** We agree. We will further assess the ecosystem feedback using actual ET derived from eddy-covariance latent heat flux. We will compare ET between the fertilized and reference sites and examine whether higher ET is accompanied by lower WAI and observed SWC.

**Reviewer comment: Line 51: 'the dry soil can even be a water competitor of plant tissues…': can you explain this a bit deeper?**

**Response**: We will clarify that when soil water potential becomes more negative than root or xylem water potential, the soil-root gradient can weaken or reverse, promoting nocturnal root-to-soil water loss (hydraulic redistribution) and increasing the tension required for uptake.

**Reviewer comment: Line 57: 'more C into … ': Do you mean relatively or in absolute terms?**

**Response:** We will distinguish allocation fraction from pool size. Marshall et al. (2023) quantified this mechanism at the Rosinedal site, where fertilization reduced total belowground carbon flux by 29% and reduced fine-root, mycorrhizal, and exudate production from 400 to 163 g C m-2 yr-1 (59%), while aboveground production increased. Their carbon-budget attribution assigned approximately 26% of the fertilization-induced increase in stem growth to greater GPP, 14% to increased carbon-use efficiency, and 60% to altered partitioning. Therefore, “more aboveground” indicates both a greater proportion of allocation and, due to increased total production, a higher absolute aboveground flux.

**Reviewer comment: Line 63: ‘However, a previous …’ : Can you go into the process behind this statement?**

**Response:** We will clarify the process that N addition partly mitigated drought effects by maintaining higher net photosynthesis and stomatal conductance, thereby improving carbon exchange under water stress.

**Reviewer comment: Line 66: Line 66: rephrase: GPP at a photosynthetic photon flux density of 2000 (units) (GPP2000) is ...**

**Response:** Agreed. We will rephrase as GPP at a photosynthetic photon flux density of 2000 umol photons m-2 s-1 (GPP2000; g C m-2 d-1) was used as an index of ecosystem light-saturated photosynthetic capacity.

**Reviewer comment: Line 67: ‘a good proxy for forest productivity’: While being a frequently used method, by adopting it, you implicitly ignore drought-imposed changes in GPP at lower radiation levels. Below, you state that summers at the site are short and cloudy. It would be good to include a statement on the relevance of light-saturated conditions relative to light-limited conditions. What percentage of the time is PAR < 2000 umol/m2/s? What happens to the alpha with N fertilization?**

**Response:** We agree and will clarify the purpose of GPP2000. GPP2000 will not be interpreted as realized daily productivity or as the exact mathematical maximum of the light-response curve. Instead, it will represent the fitted ecosystem GPP expected at a common high incident PPFD of 2000 μmol photons m-2 s-1.

Evaluating all light-response curves at the same high PPFD can reduce the influence of differences in instantaneous radiation across days and sites and allow us to compare variation in high-light ecosystem photosynthetic capacity on a standardized radiation basis. We will also acknowledge that PPFD = 2000 μmol photons m-2 s-1occurs relatively infrequently during the short and cloudy northern-Swedish summer. GPP2000 will therefore be interpreted as a standardized high-light capacity metric rather than as a measure of average realized productivity. We will also complement it with the initial light-response slope (α), which characterizes ecosystem GPP responsiveness under the more frequently occurring light-limited conditions.

**Reviewer comment: Line 67: 'By setting the light intensity …' and line 70: 'EC tower based…': I was somewhat confused, as first I thought you were referring to manual leaf-level GPP measurements, but later you refer to EC measurements.**

**Response:** Sorry for the confusion. We will clarify that GPP2000 is an ecosystem-scale metric derived entirely from eddy-covariance data and is not based on manual leaf-level gas-exchange measurements. We will introduce the EC origin of GPP2000 at its first mention and will remove wording that could imply leaf-chamber measurements or experimental light manipulation.

**Reviewer comment: Figure 1: This satellite picture is not recent. The recent satellite picture shows that the reference forest area is now only about 400 x 300 m. It would be good to be transparent about:**

**• size of the plots and footprint**

**• homogeneity of the forest beyond the footprint area (is it reasonable to expect gradients in GPP/C-uptake/release) and hence advection of CO2?**

**• management inside the plots and surrounding the plots.**

**• Please explain why you did not include a recent satellite picture.**

**Response:** The paired study areas are 13-ha plots in an even-aged Scots pine stand on a flat sandy deposit (Marshall et al., 2023). The historical image was selected because it represents land cover during the 2015-2020 analysis period. Harvest visible in current imagery occurred after the study period (surrounding shelterwood cutting northwest and northeast of reference site in March-April 2023). No logging or thinning occurred within the EC footprint during the study period; the last reported stand thinning in the plot was in 1993. The supplied multi-year footprint climatologies combine March 2006-February 2007 and January 2015-December 2019. For 94% of available half-hourly records, at least 80% of the source area was within the fertilized plot. EC measurement heights were raised stepwise as trees grew, explicitly to keep the principal fetch approximately constant and within the area of interest (Marshall et al., 2023).

**Reviewer comment: Line 100: Provide tree and EC heights, z-d, high-frequency loss, footprint/wind direction, and flux processing/partitioning.**

**Response:** We checked the complete site-specific measurement-height history. At the fertilized stand, the above-canopy EC height was 17.0 m during 1 March 2006-28 February 2007, 20.5 m during 7 July 2014-30 July 2017, 21.5 m during 31 July 2017-12 June 2019, and 23.0 m from 13 June 2019 onward. At the reference stand, the corresponding height was 18.0 m during 1 March 2006-28 February 2007, 20.5 m during 7 July 2014-2 August 2017, 21.5 m during 3 August 2017-18 June 2019, and 23.5 m from 19 June 2019 onward. The overall displacement height is around 4 meters (z-d). Both systems were raised in parallel with canopy growth, with small site-specific differences in the exact dates of change and final height. The remaining flux-processing description follows Zhao et al. (2022): 30-min covariance calculation in EddyPro v7.0.6, ICOS-style processing with double rotation, block averaging, time-lag optimization, spectral corrections, quality flags, moving-window spike screening, sigma-w decoupling filtering, MDS gap filling, and nighttime NEE partitioning. After applying all QC processes, around 43% and 48% of the CO2 flux was available at the fertilized and reference sites, respectively. The pipelines are kept constant to ensure a fair comparison between the two sites and across years. We will clarify the sensor height adjustment in our methods section.

The high-frequency loss has been corrected when running eddyPro using the built-in module. For the footprint, the tower coordinates are 64.170861° N, 19.747444° E for the fertilized site and 64.165333° N, 19.791417° E for the reference site. Based on the multi-year footprint-climatology polygons, the approximate areas enclosed by the 80% cumulative-contribution contours are 10.1 ha at the fertilized site and 6.75 ha at the reference site. At the fertilized site, the 80% footprint extended approximately 165 m to the north, 163 m to the south, 199 m to the east, and 239 m to the west of the tower. At the reference site, it extended approximately 168 m north, 134 m south, 153 m east, and 178 m west.

We will update the above information in our methods section.

**Reviewer comment: The Zhao et al., 2022 paper is written with a different goal and period, so not everything written there applies here too (is my impression). Please clarify.**

**Response:** We used the same pipeline to process the CO2 flux data; in fact, the data overlap except for 2020 (a kind of new year during that time). We will report each year available data.

**Reviewer comment: Line 102: '…2015.': Figure 2 shows data between 2006 and 2021. Please be clear about the start and end dates of the data used in the study.**

**Response:** We agree this inconsistency must be removed. The definitive analysis period starts on 1 July 2006 and ends on 31 December 2020. Our summer analyses use the June-August interval.

**Reviewer comment: Line 115: How did you estimate Rd?**

**Response:** Rd is a fitted daytime ecosystem-respiration parameter in the daily non-rectangular hyperbolic light-response model. It was estimated simultaneously with alpha, theta, and Pmax from half-hourly daytime data, rather than prescribed by nighttime respiration. We will clarify the sign convention and distinguish fitted Rd from independently partitioned ecosystem respiration.

**Reviewer comment: Section 2.4: Could groundwater dampen drought effects?**

**Response:** The water table is at least 10 m deep. Direct access by the trees is therefore unlikely, although deep unsaturated-zone storage and redistribution cannot be excluded. We will state this limitation rather than infer groundwater uptake based on proximity to rivers.

**Reviewer comment: Eq. 2: I do not understand why you apply this method on a daily basis. Droughts do not develop overnight. You need to account for precipitation deficit in the weeks/months before. Li et al., 2021 apply the method on a monthly basis to quantify climate extremes, but that may be too coarse for the short Northern-Swedish summers.**

**Response:** We agree that drought cannot be defined by a one-day precipitation anomaly. We will use WAI as a continuous drought-stress indicator across all valid summer observations from June to August.

**Reviewer comment: Eq. 4: I fail to see the rationale behind this approach:**

**• What do you really mean with recharge? In general, recharge is supposed to represent the effect of lateral ground water flows and/or the net effect of seepage, infiltration and capillary rise.**

**• why is recharge cancelled out against precipitation. Recharge and precipitation can occur at the same time.**

**Response:** We agree that “recharge” was misleading. In the WAI calculation, the term is precipitation input admitted to the finite-capacity accounting store, limited by the remaining storage deficit. It is not measured groundwater recharge or lateral inflow, and it is not canceled against precipitation. We will rename it to effective storage input and rewrite the calculation step by step.

**Reviewer comment: You list quite some arguments against using observations of SWC or water pressure or SPEI. I agree that observations can give a limited picture of soil water availability if the soil and/or water supply is heterogeneous. But you are studying quite small plots. I doubt that precipitation and evaporation are very heterogeneous. You do not write about soil Nevertheless, I think observations are the best you can get. Fig. S3 shows you have the observations and they clearly show seasonal and interannual variation. Any modeled value (e.g., WAI) can only be used as an inter/extrapolator after careful calibration. SPEI could still be interesting if based on observations of P and ET, particularly if you are studying the effect of droughts developing over the course of weeks.**

**Response:** Thanks for your comments. Here, we did not use SPEI because each tower has its own precipitation and meteorological input series. If SPEI were fitted and standardized separately for the two sites, each value would represent an anomaly relative to that site’s own climatic water-balance distribution. Such site-specific standardization could remove part of the absolute between-site difference that is relevant to our paired-stand comparison.

We therefore used WAI as the primary continuous water availability indicator because it has the magnitude and temporal accumulation of each site's precipitation and atmospheric-demand inputs on their original common water-balance scale.

**Reviewer comment: Line 157: Why not use EC ET, and how does modeled ET perform?**

**Response:** We clarify that no fitted ET model was used. PET was calculated with from measured meteorological variables and serves as atmospheric demand in the deterministic WAI calculation. We will add an analysis to compare the EC ET directly.

**Reviewer comment: Earlier in the paper, you said that N fertilisation could affect ABG and ET, which could feedback on the drought severity. Since you are studying this effect, you cannot use a model estimation which does not account for the N effects as observed. And if there are reasons to use a modelled ET, how good does the model perform, on average, and in drought conditions, with respect to the observations? Section 2.5: How good is this model prediction?**

**Response:** We clarify that the PET and WAI are calculated, so we cannot assess model performance.

**Reviewer comment: Later in the paper you show that the response of GPP to the drivers is mostly non-linear. I'd like to see how well this linear regression works, before you use the results.**

**Response:** The multivariable linear regression was not intended to reproduce the complete response shape or to serve as the primary predictive model. Its purpose was to provide a standardized summary of the direction, relative magnitude, and statistical support of associations between GPP2000 and the environmental drivers. Later, the nonlinear equation, for example, Equation (9), was used to address different questions. Similar to the light response curve, Equation (9) was used to answer how fast GPP2000 changes as WAI decreases from 100 to 0. The summary of the linear regression was list in Table S2, in the supplementary document.

**Reviewer comment: Line 169: How is VPD:WAI quantified?**

**Response:** All interaction variables were constructed after standardization. Each component variable was converted to a z-score using its analysis-sample mean and standard deviation, and the interaction was then calculated as the product of the two z-scores. We will state this for VPD × WAI and every additional pairwise interaction. We will clarify in the method part.

**Reviewer comment: Before Section 3.1, assess ET and WAI models.**

**Response:** Neither calculated PET nor WAI is a statistically fitted predictive model.

**Reviewer comment: Figure 2 caption: Explain how half-hourly EC data become daily GPP2000.**

**Response:** For each day, a non-rectangular hyperbola was fitted to the relationship between half-hourly ecosystem GPP and incoming PPFD. The fitted curve was evaluated at PPFD=2000 umol m-2 s-1. Only fits that passed goodness-of-fit ($R2 > 0.6$) criteria were kept.

**Reviewer comment: Line 226: Line 226: How can WAI be different between the sites? Eqs. 3-6 suggest that only precipitation or PET can be different between the sites. Did you measure preciptiation at both sites? If so, why do you show only 1 in Fig. 2d? Or is PET different between the sites? If so, please explain what parameters or input variables are different between the sites.**

**Response:** It is because some precipitation is gap-filled by a nearby station. The fertilized site has continuous above-canopy rainfall observations, whereas above-canopy rainfall measurements at the reference site began in summer 2017, when weather stations at both sites were replaced. Missing precipitation at either site, including the pre-installation reference-site period, was filled in from the nearby Svartberget Degerö site. Thus, WAI can differ because the sites use their own available precipitation and meteorological or PET inputs, while a common nearby record supplies missing intervals, after which each water store evolves recursively.

## R1.21 - Daily variation wording

**Reviewer comment: Line 227: Line 227: '…, the long-term …': Unclear if you are referring to the daily total or daily variation. How should I see that the daily variation is larger at the fertilised site?**

**Response:** We did not intend to state that the fertilized stand had greater day-to-day variability or variance in GPP2000. We intended to describe the relative levels shown by the daily time series: GPP2000 was generally higher at the fertilized stand, and the absolute difference between stands was most apparent near the summer seasonal peak. We will replace "long-term daily variation" with "long-term daily time series".

**Reviewer comment: Please rephrase.**

**Response:** Agreed. These lines will be rephrased.

**Reviewer comment: Line 237-238: I am not sure if I agree with this formulation of sensitivity. From fig 3 it seems like GPP2000 is larger at the fertilised site than at the reference plot. However, the response seems to scale perfectly with the GPP2000_max. Obviously, if GPP2000_max is smaller, the change between GPP2000_max and 0 gC/m2/day is smaller. But I would not call this a difference in sensitivity. I would only call it a sensitivity difference if the response curves have different shapes.**

**Response:** We agree. The revised analysis will compare normalized curves by GPP2000_max and its shape parameters.

**Reviewer comment: Line 238: 'In addition, …': Not in addition, this is the starting point. Please mention this before discussing the response..**

**Response:** We will rephrase this part.

**Reviewer comment: Figure 4: Caption: 'during dry days': As indicated before, I object to using P_anomaly defined on a daily basis as a drought indicator. Please use another metric (preferably soil moisture and or vpd observations)**

**Response:** We agree that daily precipitation anomaly is not an appropriate indicator of accumulated drought. We will therefore remove P_anomaly from the drought definition and from the associated figures and analyses. We will not preselect drought days according to daily precipitation. Instead, we will use WAI as a continuous drought-stress indicator across all valid summer observations from June to August. Although WAI is updated daily, it has accumulated hydrological conditions because the water store on day t depends on the store on day t-1, as well as on current precipitation and atmospheric evaporative demand. Declining WAI will therefore represent the progressive development of water limitation rather than the precipitation condition of one particular day. We will analyze the response of GPP2000 across the continuous WAI gradient, and use observed SWC and VPD as complementary variables.

**Reviewer comment: Line 355: I don't see this 'offset'. Figs. 2 and 3 show that GPP in the fertilised plot is always larger than in the reference plot. It almost seems like a constant ratio. Hence, even in drought conditions, GPP will be larger in the fertilised plot. (See also my general comment).**

**Response:** We agree and will revise the word "offset". The conclusion will refer to a diminished marginal benefit under drought, while explicitly noting that GPP2000 remained higher at the fertilized site.

**Reviewer comment: Line 35: There are many papers describing a decline in soil organic matter mineralisation as a result of N deposition. I am not entirely sure if this applies to artificial fertilisation too, but it would be good to be aware of it.**

**Response:** Thank you. We will revise this part.

**Reviewer commentLine 376–379: I object to this formulation of the conclusions. Yes, the response of GPP2000 is larger at the fertilized site, but only because the range in GPP2000 is larger. There is no "offset" in the sense that GPP could be so far reduced during drought that annual GPP becomes smaller than at the reference site.**

**Response:** Thank you. We will revise the conclusion, and we will compare the absolute response curves, GPP2000 normalized fit curve. A larger absolute decline will be described only as greater absolute responsiveness. We will conclude that fertilization increased drought sensitivity only if the normalized response curve differs significantly between the sites.

**Reviewer comment: If values are standardized, why is the mean not zero?**

**Response:** Thank you for identifying the ambiguous caption. SWC was not standardized separately within each site. All observations from the fertilized and reference sites were put together, and a mean and standard deviation were used: z(SWC)=[SWC-mean(SWC_all)]/SD(SWC_all). The mean is therefore zero across the combined dataset, but the mean within either individual site is not necessarily zero.

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# Reviewer 2

**Reviewer comment: Line28: adding some nuance from the classic Bonan studies on forest impacts of the climate system would be forthcoming.**

**Response:** We will add the classic Bonan studies (Bonan, 2008; Bonan et al., 1992) to highlight that the snow-albedo feedback could further offset the cooling effect by carbon sequestration in boreal forests.

**Reviewer comment: Line39: the original reference for this is probably Körner 1995. There are also a few topics muddled together in this paragraph**

**Response:** We will review the recheck and reorganize this section.

**Reviewer comment: L122: Was a value forgotten here?**

**Response:** Yes. The full sentence is “Thus, we further recomputed GPP with a fixed PPFD of 2000 μmol $m^{-2}$ $s^{-1}$ by Eq. (1)” So, 2000 is missing there.

**Reviewer comment: Was the theta parameter constrained in any way to take logical values (between 0 and 1 if I’m not mistaken)? This might not be an issue, but this model can be hard to parameterize?**

**Response:** We used 7-day moving-window light-response curve fits that bound theta between 0 and 1 and retained the outputs only fits with $R^2 > 0.6$.

**Reviewer comment: Eq 7: Was standardization necessary here? Many of these values are not normally distributed, and the linear regression model will fit regardless of normalization; the parameters will just have units.**

**Response:** We agree that standardization does not normalize distributions and is not required for linear regression. Its purpose was only to compare coefficient magnitudes across differently scaled predictors.

**Reviewer comment: In Figure 2, there was some missing data pre-2015 for the reference site, which is fine but were these periods excluded in the bar graph with the average statistics when comparing?**

**Response:** All site comparisons were restricted to paired dates, meaning that if one site is missing a period, the paired date at the other site will not be included in the statistical comparison.

**Reviewer comment: Were the eddy-covariance systems calibrated against each other?**

**Response:** Both tower systems were calibrated and maintained in accordance with the same site procedures. Every retained observation from the two towers was processed using the same established pipeline as Zhao et al. (2022): identical EddyPro calculations and corrections, quality-control rules, sigma-w decoupling filtering, gap filling, and flux partitioning were applied. This common pipeline minimizes differences in processing methods between the treatment and reference comparisons.

**Reviewer comment: How was covariance among drivers handled? Standard RF importance can be biased by correlated predictors.**

**Response:** We agree that covariance among environmental drivers, particularly WAI and VPD, may bias variable-importance estimates from the random forest model. We will further improve this part by supplementing the original analysis with conditional inference forests and conditional permutation importance. The conditional permutation procedure evaluates each predictor while accounting for its dependence on the remaining predictors, thereby reducing the importance of bias associated with correlated drivers. We repeated the conditional forest analysis 100 times with repeated sampling using independent random seeds and summarized the mean and 2.5th–97.5th percentile range of the resulting importance estimates.

# References

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