Review of '**Long-term nitrogen fertilization increases drought sensitivity of gross primary productivity capacity in a boreal Scots pine forest'** by Liang Chen et al, submitted for publication in EGUsphere, 2026

General comments

This is an interesting paper about the response of the photosynthesis rate to drought of a set of Pine forest sites in Sweden with contrasting nitrogen fertilisation. Boreal forests play an important role in the global carbon cycle and carbon sequestration on land. The boreal climate is expected (and observed) to become more variable and droughts are becoming more frequent. Most boreal forests are nitrogen-limited in their growth rates and in plantations, nitrogen fertilisation is often used to lift the limitation. Nitrogen fertilisation stimulates growth and results in larger above ground biomass and leaf area, and hence increases water use. As a result, fertilised boreal forests may more often run into water-limited situations as a result of more frequent precipitation deficit and water use. Considering the vast areas of boreal forests, fertilised or not, a potential feedback to the photosynthesis rates and the global carbon cycle is worth studying.

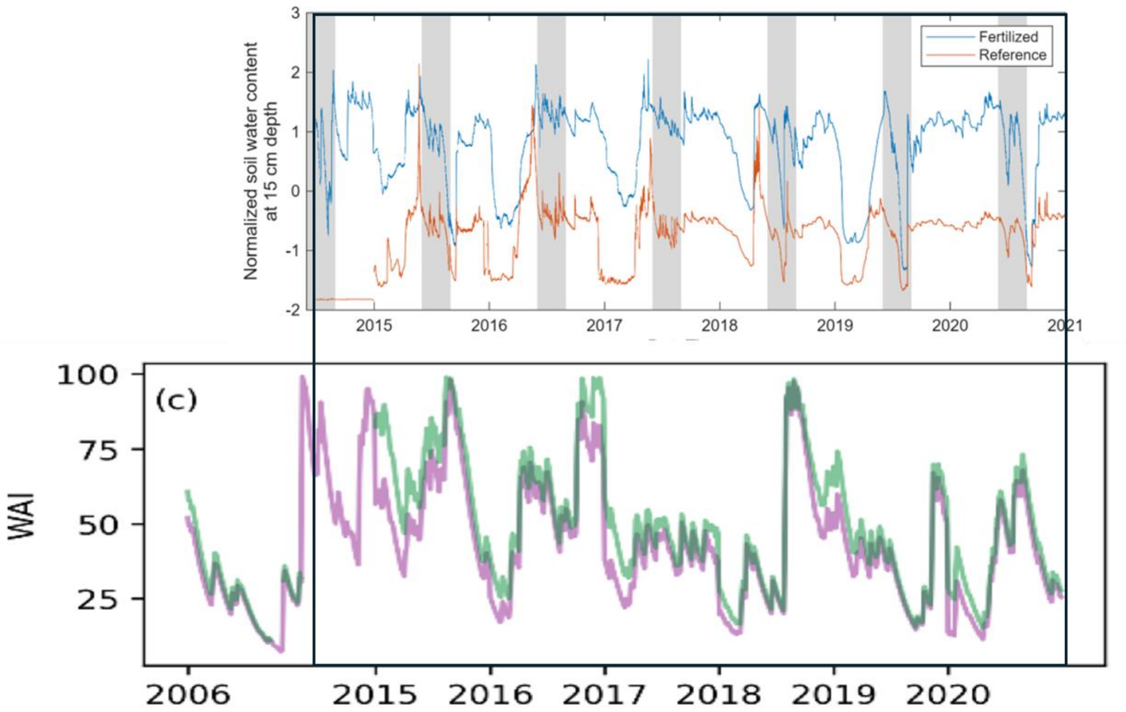
The authors avail over site observations of GPP and water availability at two neighbouring sites, one fertilised and the other not, in Sweden over multiple recent years, allowing them to study the drought feedback effects. The paper is well-written, concise and to the point. The figures are clear and serve their purpose. As such, the subject and material are worth being published about.

I have a few concerns, which refrain me from advising publication in its current form. In the detailed comments I will address these in more detail, here I will summarise them:

1. 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).
    a. 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?
    b. It is unclear if precipitation was measured at one or both sites.
    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.
    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.
    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.

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.



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.

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.

Considering that my concerns are predominantly methodological and textual, and I think the authors could address them without too much work, and that the paper addresses important, new and relevant information, I advise publication after major revisions.

Detailed comments:

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

Line 57: 'more C into ... ': Do you mean relatively or in absolute terms?

Line 63: 'However, a previous ...' : Can you go into the process behind this statement?

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

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 at the site, summers are short and cloudy. It would be good to include a statement about the relevance of light-saturated conditions with respect to light-limited conditions. What percentage of the time is PAR < 2000 umol/m2/s? What happens to the alpha with N fertilisation?

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.

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.

Line 100: '… described …': It would be good to provide some basic information to the reader to help interpret the measurements:

- what is the height of the trees, the ec system, (z-d) and do high-frequency underestimations play a role (how much?)
- what is the size of the N-treated and reference areas in relation to the footprint area? What is the wind direction distribution and did you exclude winds from the North? (It seems like the fetch towards the North is only about 500 m, but I don't know the footprint area.)
- some basic information about flux calculations and partitioning.

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.

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.

Line 115: How did you estimate Rd?

Section 2.4: You do not write about the groundwater table. Figure 1 shows that the plots are close to a river. I suspect the ground water table is not too deep to access for the tap roots of the pine trees. How does groundwater uptake dampen the drought effects of your forest plots?

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.

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.

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 heterogeneity, sloping terrain or lateral and vertical ground water flows, nor about the differences between the plots.

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 modelled 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.

Line 157: You have an EC system, why don't you use observed ET? I think that would be much preferable over using a modelled version.

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?

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.

Line 169: How do you quantify VPD:WAI?

Section 3 (before 3.1): Here I would expect a careful assessment of the ET and WAI models.

Figure 2 (caption): Which approach to GPP2000 do you use here? It is unclear to me how you upscale EC measurements of GPP to a daily GPP2000 value.

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.

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?

Line 228-230: please rephrase.

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 sensitivity difference. I would only call it a sensitivity difference if the response curves have different shapes.

Line 238: ‘In addition, …’: Not in addition, this is the starting point. Please mention this before discussing the response..

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)

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).

Line 359: 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.

Line 376 – 379: I object to this formulation of the conclusions. Yes: the response of GPP2000 is larger at the fertilised site, but only because the range in GPP2000 is larger. There is no 'offset' in the sense that GPP could be so far reduced in drought periods that the annual GPP is smaller than at the reference site.

Fig. S3: If the values are standardised, how can they not be 0 on average?