

“Nitrogen deposition and climate drive plant nitrogen uptake while soil factors drive nitrogen use efficiency in terrestrial ecosystems” by Vallicrosa et al. presents an analysis of ground-based estimates of nitrogen uptake and nitrogen use efficiency using NPP of foliage, wood (if present), and fine roots along with nitrogen concentration data for each of those tissues. The data span all the biomes, and the authors pair those measurements with plausible drivers. Climatic variables and N deposition appear to drive nitrogen uptake, whereas soil biological variables appear to drive nitrogen use efficiency. Total soil nitrogen was not predictive of either response.

I am impressed by the data collection, which seems reasonable to me. I am satisfied by the equations presented (subject to some small points below) and the measures the authors used to fill in missing data. I think the results are believable and interesting and useful.

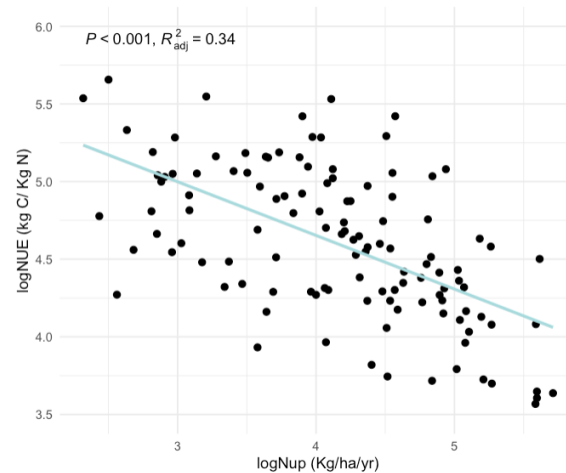
We thank the reviewer for the very positive comments and time devoted to improving our manuscript, providing deep thinking and arising excellent points. By incorporating the requested arrangements, we believe that the clarity and fairness of our study will be substantially improved. We deeply appreciate your rigorous but simultaneously kind approach in your review.

I have only two major requests. First, I can see that the Nup and NUE global predictions are not simply negatives of each other, but I can also see that there is a negative correlation between Nup and NUE. Would it be reasonable to provide a plot of these two key variables against each other? I attempted this with the supplemental data, and there was clearly a “wedge”: high Nup paired with low NUE and high NUE paired with low Nup. But intermediate values show a lot of variation. I assume the different predictors explain a lot of that variation. OK, here’s my request: please explain why a reader who casually looked at figure 3 would be wrong to conclude that climatic drives are affecting both Nup and NUE in opposite directions. It just seems weird that none of the climatic drivers rise to the top of the NUE analysis, especially when I look at figure 3b. Put another way, the interesting result that Nup is driven by climate and NUE is driven by biology warrants deeper discussion, along with ways that the “biology” might also be correlated with climate (or not).

This is an excellent point, thank you for pointing that out. What figure 1a and 2a are disclosing is what are the most important variables to explain Nup and NUE respectively. In other words, which are the variables that explain a higher portion of the variability of the dependent variable (Nup or NUE). Nonetheless, the fact that a variable it is not disclosed as important doesn’t make it uncorrelated, as you mention in your comment. As we report in the table S2 and line 442 we see some level of correlation between soil and climatic variables being 0.52 the highest one, between MAT and AM presence. Thus, aligning with your point of view, soil variables show to embed climatic factors to some extent since they are not totally independent ecologically. We see how this can create confusion to our potential readers so we will better clarify this aspect in the section 2.1 as follows: “Despite climatic variables such as MAT and MAP not appearing as important variables explaining NUE, they are to some extent represented in the soil variables. As shown in Table S2, soil variables are not independent of climatic

variables since they have some degree of correlation”. We will also remove sentences that could give opposite impressions to avoid confusion in the abstract and the main body of the manuscript.

In addition, we will include a supplementary plot (Fig. S10) showing the relationship between Nup and NUE as follows in the methodology section.



My second major request is for some caveats and discussion about the global nature of your conclusions. I work in temperate forests – the drivers of Nup and NUE within this much smaller set of points may or may not match the drivers you found globally. I think that is worth saying loudly (i.e. in the abstract as well as the discussion). In other words, I think you have found great reasons why Nup and NUE will differ between, say, a boreal forest and a tropical forest, but your analysis doesn’t say anything about why, say, a pine forest will have a different Nup and NUE than a nearby maple-basswood forest (right?). I would hate for folks who are new to this area or who are only thinking about this superficially to imagine that the strong results you found globally also apply locally. (They may, but your analysis can’t address whether they do or don’t.)

This is indeed a caveat of global approaches that needs to be responsibly acknowledged. We believe our 2.7 section is the best to include such discussion. There, we will incorporate further discussion as such: “Global scale approaches such as this one, are intended to provide broad quantifications of planetary processes assuming a scale-precision compromise. Because of that, we do not advice to use our approach to seek fine scale precision, since our results may overlook local particularities and differences between forest type, species distribution or management practices”. We will also emphasize in our abstract that our approach is valid for global scale quantifications.

Minor things:

Line 51: “we used ground-based observations...” can you say something more about the nature of these measurements?

With pleasure. We will include: “...we used ground-based observations across 159 field experiments (including above and belowground information)...”

Line 53-54: do you mean “mean temperature and precipitation”?

Yes. We will modify it as mentioned.

Line 165: Looks like the dashed line is at 0.75, not 0.8.

You are right, thank you. We will modify the footnote accordingly.

Line 165 (and 180): I don't know what "The model pseudoR2 was 0.349" means (oh, is it the R2 for the model? This can be clarified.)

Yes, it is. We realize the typo now and we will change it to GLM pseudoR<sup>2</sup> instead.

Line 173: Is "contraposition" the correct word here? "In contrast" would work nicely I think

It will be changed as you recommend

Line 244, 275: I would recommend rounding to whole numbers – three decimal places seems like a mismatch with the uncertainty in these numbers.

It will be changed to  $842 \pm 236$  Tg of N and  $110 \pm 19$  kg C per kg N in the text as you suggest.

Line 263: I'm not sure what you mean by, "Nonetheless, these efforts do not translate yet on low N deposition effects in natural woodlands and grasslands."

We agree it is not straight forward. We will rephrase it to "Nonetheless, these reductions do not translate yet into how N deposition effects natural woodlands and grasslands, showing to still be the main driver for plant Nup."

Line 269: I understand this sentence, but I think it can be rephrased more clearly

It will be rephrased as such: "Consequently, this extra input of N coming from N deposition is not being captured by biomass and enhances the N leaching associated with eutrophication, acidification, loss of biodiversity, and N<sub>2</sub>O emissions (Aber et al., 1989; Gundersen et al., 1998; Bobbink et al., 2010) exacerbating environmental problems."

Line 328: "truthfully" seems like an odd word to use here. Perhaps "more accurate"?

We will incorporate it as suggested.

Line 418: Seems strange to call this "GrossNup" since they actually aren't taking up the portion that they were able to retranslocate. What you are calling "NetNup" is what they are actually taking up. I'd suggest simplifying this and avoiding confusion by embedding the retranslocation within a single equation for "Nup".

We will rearrange it in one single equation as such:

$$Nup = (NPPlaves * Nleaves + NPPstem * Nstem + NPProots * Nroots) - (NPPlaves * Nleaves - NPPlitter * Nlitter)$$

Line 424: these are fractions, not percentages, right?

The most common units to express N in plant tissues is percentage in dry weight (%) or mg of N per g of dry biomass (mg/g). In this case we chose %. We changed it to % in dry weight to make it clearer.

Line 423, 434: shouldn't this be kg biomass/ha/yr?

Indeed. It will be modified accordingly. Thank you for pointing this out.

Line 435: These units don't seem to be working out

We will express it the same way as for the other formula