

Interactive comment on “Leaf nitrogen from first principles: field evidence for adaptive variation with climate” by Ning Dong et al.

Anonymous Referee #2

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This study sets out to predict leaf nitrogen per unit area (Narea) through a combination of leaf mass per unit area (LMA), the ratio of leaf-internal to atmospheric CO₂ (ci:ca) and Rubisco activity.

Although the study presents some interesting observations relating environmental variables to Narea and other leaf-scale traits, a major omission has been made by not showing explicitly how nitrogen per unit leaf mass (Nmass) varies in these observations. It is possible to infer some aspects of the relationships from the data presented, but it seems possible that a much simpler and perhaps stronger predictive relationship could be formulated around the simple fact that $Narea = LMA * Nmass$. This relationship is clear to the authors as they use it to calculate Narea itself from measurements of LMA and Nmass (p.5 line 24).

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The authors attempt to separate the LMA contribution to variation in Narea from a metabolic contribution, but they arrive at a summation of effects, one connected to structural variation which is tightly connected to LMA, and another metabolic component that is formulated as independent of LMA (p.2 lines 12-14, p.7 lines 4-6). My concern with this approach is that the metabolic component of Narea includes a dependence on LMA as well, since metabolic variation can be driven both by changes in the leaf tissue N concentration and by the number of layers of mesophyll cells and the thickness of each layer.

Without explicitly showing how Nmass is related to the environmental factors explored here, it is not clear how the current study moves the field forward from the relationship suggested by Niinemets and Tenhunen (1997) between V_{cmax} and Narea.

There is also a potential incongruency in the calculation of irradiance as a function of canopy leaf area, while asserting that the leaves measured were from the sunlit canopy. If truly sunlit leaves were used, then the relevant irradiance would be the top of canopy values. Perhaps this is just a matter of defining what sunlit leaves means for species that exist only in the understory of mixed species canopies. In any case, I am concerned that the irradiance used for sunlit leaves of the dominant trees in these relationships is not the correct one.

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