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Interactive comment on "C_3 plants converge on a universal relationship between leaf maximum carboxylation rate and chlorophyll content" *by* Xiaojin Qian et al.

Anonymous Referee #1

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"C3 plants converge on a universal relationship between leaf maximum carboxylation rate and chlorophyll content" X. Qian et al.

Qian and authors used linear regressions to evaluate a combined dataset of leaf level chlorophyll content and maximum carboxylation rates from roughly one dozen species of C3 plants. They demonstrate that chlorophyll content explains roughly 65 percent of the variation (spatial and temporal) in measured Vcmax. The authors use this regression to argue for the existence of a 'universal' relationship between chlorophyll content and Vcmax and intimate that such a relationship could be applied globally to derive robust measurements of Vcmax.

My primary concern about the manuscript has to do with the claim of 'universality', which probably comes with little surprise. To start, the number of species considered by their analysis is quite limited. For trees, their dataset only considers four temperate broadleaf species, of which two (P. grandidentata and P. tremuloides) are incredibly closely related. What about gymnosperms? Does this hold for douglas fir and pinyon pine? Or evergreen oaks? Various C3 grasses? Arid shrubs? Any claim to universality must have a far more diverse collection of underlying species considered by the study.

The claim to universality is further undercut by the actual amount of data collected per species. Some of the shrub and vegetable species have fewer than ten measurements. The discrepancy is visually highlighted in Figure 4, where there are only a handful of 'vegetable' and 'shrub' data points. As a result, the overarching relationships are driven by the vastly larger "(Temperate Broadleaf) Tree" and "Crop" datasets. This discrepancy actually goes to undercut the overall message, as the slope of the Vcmax-chl relationship definitely doesn't look the same for the vegetable data (NRMSE $\sim$ 50 percent). Furthermore, both these datasets seem to have been previously published to highlight the strength of the Vcmax-chlorophyll relationship (Croft et al 2017; Qian et al 2019). While it's fine to combine previously published datasets to derive new insights, the authors here could do a better job of framing how the combination datasets allows for a new advance. As presently constructed, the manuscript implicitly suggests that the strength of the Vcmax-Chl relationship is a mostly novel finding.

I was wondering if the authors might not do a little more work to expand their dataset further still. Table 2 lists a number of previous studies that have explored the Vcmax-Chl relationship. Have the authors considered combining their data with the vcmax-chl data they have collected? It could be interesting to more thoroughly and exhaustively combine datasets in a statistical framework to understand how things like phylogeny (species/genus), leaf habit (evergreen/broadleaf) and anigo/gymnosperm affect the Vcmax-chl relationship. My suspicion is that the results would point toward a fairly consistent Vcmax-chl relationship, but would do so in a framework that more holistically appreciates the wide array of C3 plant types.

The authors also some logical jumps that weaken their overall argument. The first is relatively minor. In the introduction the authors indicate that the consistency of the Chl:Vcmax relationship is stronger than the N:Vcmax relationship and use this as the basis for focusin on Chl in the main body of the text. I was hoping that the authors would revisit this claim in their analysis. It would be nice to establish that the Chl:Vcmax relationship is i) strong and ii) stronger than alternatives. Again, this is a relatively minor point, but one that would make any claim to universality much more convincing.

The second logical jump is slightly more important. Throughout the introduction, discussion, and conclusion that authors make the repeated claim that establishing a strong Chl:Vcmax relationship might enable mapping Vcmax at the global scale using remote sensing. This is a huge jump. From my reading, the data analyzed here is leaf level data. Satellites see canopies – not leaves. How sure are we that leaf-level relationships hold at the canopy scale? I am especially reminded of the back and forth between Ollinger et al. 2008 and Knyazikhin et al 2013, both published in PNAS. Ollinger put forth an approach for measuring canopy nitrogen content, while Knyazikhin argued those "spectra-nitrogen" relationships were more than likely driven entirely by variations in canopy structure. How likely are the critiques of Knyazikhin to apply to remote sensing measurements of chlorophyll? Certainly I do not expect the authors to have all the answers to questions like these, but it seems inappropriate to ignore such concerns entirely. Reliably estimating just about anything from remote sensing requires consistency (in a physical sense) between what satellites measure and what we measure on the ground. In my mind, the remote sensing world has repeatedly undercut its credibility by avoiding, as opposed to embracing, issues of scale.

- Ollinger et al 2008. "Canopy nitrogen, carbon assimilation, and albedo in temperate and boreal forests: Functional relations and potential climate feedbacks" - Knyazikhin et al 2013. "Hyperspectral remote sensing of foliar nitrogen content"

I will make a final note concerning citations. Overall, the citations tend to skew toward the more recent (e.g., 2017 or newer). There were also several instances where the authors cite a paper that discusses a topic (e.g., L160 citing Croft et al 2017 in reference to b6f/NADPH), as opposed to citing a more direct paper that focuses on the topic. I would encourage the authors to careful revisit their citations to make sure the appropriate literature is cited.

Minor Notes

L50 inevitably? This confused me.

L166: "Adjusting the concentration of leaf chlorophyll pigments is one of the most effective mechanisms by which plants regulate light absorption." Such a claim could use a citation. This is certainly an interesting point, but I am not familiar with the literature that supports this line of argument.

Why do the authors use NRMSE? One of the main reasons to use RMSE is that it has units that make sense. Given that all the analyses are in terms of Vcmax, it seems more informative to use RMSE.

Figure 4: Please change the axes so the observed values are on the y-axis. This makes it so the intercept term is interpretable in terms of the linear relationship between the two variables.

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