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Discussions

Interactive comment on "Wide Discrepancies in the Magnitude and Direction of Modelled SIF in Response to Light Conditions" *by* Nicholas C. Parazoo et al.

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General:

Parazoo et al. compare seven SIF-enabled TBMs against empirical SIF and GPP data from a subalpine evergreen coniferous forest. The models, which had SIF retro-fitted, share some common concepts but on the other hand differ widely in terms of other concepts, with corresponding impacts on simulated SIF. The authors describe the differences compared to the empirical data and discuss these in terms of the differences in model structure.

Interest in the adding SIF capabilities to TBMs is largely driven by the recent availability of global SIF satellite products which provides promising avenues for additional constraints on carbon cycling, especially for GPP. Given that this research field is still in its infancy, I think the scope of this study, even though limited to a single site and a few weeks of peak-vegetation period data, is justified. The manuscript is well written and I think the authors do a great job in navigating the reader through the complexity of the investigated TBMs without getting lost in the many aspects these models differ.

I have only really very few detailed comments (see below) and only one major comment, that is that I was wondering whether the model comparison would profit from adding simulations with the original SCOPE model. This model is some sort of golden standard for SIF modelling (in fact many of the investigated models have gleaned from SCOPE in one way or the other) and I could imagine that SCOPE simulations might provide a good benchmark for the investigated TBMs, which given their scope need to weigh complexity against realism. Even though SCOPE is much more complex in terms of the treatment of canopy radiative transfer and gas exchange, running it with pre-scribed meteo inputs and adjusting a few key parameters should be easy to do.

Detailed comments:

l. 60: and theoretical models suggest a non-linear response at leaf-scale (Gu et al. 2019)

l. 84: a needle is anatomically a leaf

l. 102: not so much at leaf-scale really

l. 103: the FLOX is missing in the list of tower-mounted spectrometer systems

Fig. 1: calling a 3-year average a climatology is a bit of a stretch in my view – maybe just refer to this as the 2015-2018 average?

l. 165-174: how representative are these measurements for the larger footprint of the flux tower?

l. 229: one sentence on the effects of complex terrain, for which NR1 is famous, on NEE and inferred GPP?

l. 260: wouldn't that be the Ball-Berry-Woodrow (BBW) model?

l. 261: and this simply the Leuning model?

Table 1: what is the difference between big-leaf and single layer models? Where do two-leaf big-leaf models fall into?

l. 573: sunlit/shaded leaf area fractions

l. 803-810: what are recommendations for model structure with respect to APAR?

l. 816: might refer to new approaches such as stomatal optimisation based on xylem hydraulics (Eller et al. 2020)

l. 821: here I would think we also need more data from a wider variety of plant species under in situ conditions, especially all kinds of stress, ideally combining active and passive chlorophyll fluorescence measurements

l. 833: for perspective - do the authors dare to say something about what they would expect from a similar model comparison for a well-watered high-LAI crop?

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