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Interactive comment on "Description and validation of an intermediate complexity model for ecosystem photosynthesis and evapo-transpiration: ACM-GPP-ETv1" *by* Thomas Luke Smallman and Mathew Williams

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Smallman and Williams describe a daily, canopy-scale, coupled photosynthesis-ET model, which they categorise as "intermediate" complexity. They ask how computationally efficient their simpler model is and whether this approach can adequately simulate the complex TEM. They also discuss potential research applications of this model.

The bottom line is that I think this paper is interesting, thorough and I can already

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Discussion paper

see many future applications - it warrants publication. However, I think the current presentation sells it a little short.

For example, the paper starts out by make strong claims for the need for a computational efficient model - fine. But if that is the desire, why not simply use a big-leaf/2-leaf, coupled A-ET model? Or even more simply, a linear approximation (e.g. Best et al. 2015J. Hydrometeor., 16, 1425–1442), or something between those two approaches involving machine learning? My guess, is that the reason is that speed isn't the ultimate objective. So being told that the new model is ~2200 times faster than SPA, to me, isn't all that interesting. I bet I could make a simpler model than this which is quicker still. I'm not arguing that the choices made by Smallman and Williams aren't perfectly legitimate (although "fewer parameters than typical leaf-scale stomatal models" - is that actually true?); however, they aren't simply with speed in mind.

I would like to see more text devoting to the justification of their intermediate model. What is the underlying performance expectation here ("ACM-GPP-ET and SPA simulated GPP and ET fluxes at 59 FLUXNET2015 sites with substantial skill")? And how is the reduction in complexity affecting performance? My sense as a reader is I don't really know the answer to this question after reading the results. In Fig 3, the simpler model produces an R2 > 0.81 for all four fluxes. How much more would this have been degraded by simplifying the model further? Or phrased another way, which of the key assumptions are responsible for this performance? Knowing that might be really insightful for model development and a broader audience than users of the ACM/SPA models. It might also help identify areas where the emulator model could be improved further to be more mechanistic.

Finally, the authors clearly see model-data fusion as a means to calibrate such a model. Therefore, I wonder about some of the choices in terms of mechanism (see point about plant hydraulics).

Introduction ————

- Pg 2, line 23: This statement about TEMs being expensive (slow) requires some quantification. It is not my experience that standard TEMs, which simply solve coupled C-water fluxes are actually all that slow. No doubt a model such as SPA (that the second author works with) is undoubtedly slower than most TEMs, but as a blanket statement?

- Pg 2, line 24: the text around issues to do with model data fusion ignores recent advocates of emulators (e.g. Fer, et at. Biogeosciences, 15, 5801-5830, 2018.).

- Pg 2, line 27-30. Whilst this is a valid argument, I wonder what the evidence is that use of a daily model is less biased than a sub-daily model reliant on a weather generator? This text also ignores a number of papers that have attempted to approximate sub-daily behaviour without the need for a weather generator (e.g. Sands, P. J. (1995). Australian Journal of Plant Physiology 22, 603-6 14.).

- Pg 2, Line 31: Agreed and I note that the second author has already produced such a model, ACM. I suggest some text at this point to discuss this and how the proposed approach differs is warranted. Presumably, the distinction is the coupling of the carbon and water cycle and I suggest it is worth including the history of ACM in the discussion on Pg 3.

Methods: ———

- What is the link between eqn 1, Pn and gc, eqn 2? Surely Pn should depend on gc? In fact, the final eqn for GPP, number 14, which is dependent on gc makes sense, but what is the connection to eqn 1 and where is this explained?

- Where are the equations for gs and gb? I actually see these are included below eqn 57 for gb. It would be worth telling the reader this at the point gs and gb are introduced.

- Page 8, line 5: why is the reference temp 20 and not 25 degrees?

- How is the isothermal net radiation estimated?

- Why add in the complexity of the optimisation (sec 2.6) and/or the plant hydraulic resistance (2.9)? Surely this isn't more efficient than a simpler bucket type soil model? Further, in 2.9, the plant hydraulics resistance actually makes little use of commonly measured traits (e.g. p50). I wonder if this isn't quite a disadvantage moving forward. What would any proposed optimisation scheme be calibrating against?

Results: ——-

- I feel like the start of the results could benefit from a sentence introducing what is happening again. At this point, the manuscript is 22 pages long and although the CARDAMON stuff was introduced in the methods, I suspect you could forgive the reader for being a bit lost. My suggestion would be to re-read 5.1 standalone and see how clear it is for a reader, I would suggest it could be revised.

- Pg 22: Do the authors have thoughts on why the model is underestimating peaks in transpiration as simulated by SPA?

- Pg 23: "However, ACM-GPP-ET marginally out-performs SPA at most sites and for ET in particular." - how should this be interpreted by the reader? My interpretation is that the simpler model, which is a calibration ought not to out perform SPA and if it does so, it does so for the wrong reason. This warrants some comment.

- I didn't find the comparison in Fig 5 particularly insightful. Lumping all the sites means that we don't learn anything. Where does the model perform best, worst? What does this performance tell us about the underlying mechanisms? In that sense, Figure 7 is more useful and perhaps 5 could be omitted?

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