Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-200-RC1, 2019

Geoscientific Model Development
Discussions

Interactive comment on "P-model v1.0: An optimality-based light use efficiency model for simulating ecosystem gross primary production" *by* Benjamin D. Stocker et al.

Anonymous Referee #1

Received and published: 15 September 2019

I have a mixed feeling for this review. On the good side, this is incredibly well written. All figures and analysis are highly professional. On the other side, this manuscript degraded the elegance of optimality hypothesis.

I fully understand the original paper on optimality hypothesis (Han et al 2017) was not perfect. It had much room for improvements. But the way to improve in this manuscript is not attractive in my view with following reasons.

1) AET/PET was used for aridity index to consider drought effects. The authors used SPLASH model. If AET can be modeled so well, then GPP must be modeled well too as they are both tightly correlated via stomata conductance. Therefore, in my humble

opinion, bringing AET to consider drought effects in GPP estimates are logically odd. The key motivation of this study is to add soil stress function into P-model which leads better prediction of GPP, but that added soil moisture function appears decoupled from stomata conductance in the framework of optimality hypothesis. So in a physiological sense, it is not any more optimal model. Bringing stomata conductance from SPLASH would be one option although it is ugly... but the assumption of using AET/PET is that stomata conductance is correct.

2) There has been a series of papers that proposed global GPP maps with evaluations against fluxnet database. Many papers which were cited in this manuscript already evaluated model performance across scales from site level to the global land, daily to seasonal to annual scales. When I agreed to review this manuscript, I expected what would be global GPP, and how it varies in space and time from P-model. Site level evaluation for seasonal scale does not convince me about the overall performance of this model. In my past experience, I could match the modeled seasonal variations of GPP with fluxnet GPP extremely well; but in that case, global GPP values and interannual variation/trends were weird. I mean the authors should test the revised P model across different scales. Current evaluation is not enough.

3) The authors have incorporated an empirical soil moisture stress function to down-regulate LUEopt. I understand why the authors introduced soil moisture stress function after the 1st author's fantastic papers on drought and fLUE. However, I think the introduced soil module is too heavy given the elegance of optimality hypothesis. It is a typical soil bucket model which requires soil properties and rainfall. To scale up P-model globally, the key barrier will be this soil module- they are too uncertain and P-model will be coupled with a heavy hydrological model like SPLASH. We know microwave remote sensing based soil moisture only captures top soils.

4) The improved model still showed poor performance in capturing interannual variations of GPP. That's disappointed given the introduction of temperature and soil moisture terms.

5) Overall framework of revised P-model is almost identical to MODIS LUE model. MODIS GPP model downregulates LUEmax via temperature and VPD. Recent papers proposed a universal LUE max, or pixel based LUEopt that varies with time. That is the current status of MODIS GPP model. Then the revised P-model is almost following same direction; incorporating temperature and soil moisture to reduce LUEopt. Although the processes differ between two models in terms of f(temp) and f(water), overall philosophical framework appears very similar. That is the reason that I wrote "degradation of elegant P-model" in my general comments. If optimality hypothesis does not reflect temperature and water stress well, that indicates the optimality hypothesis is incorrect. Decoupling stomata conductance from added soil moisture function is a drawback in the framework of optimality. I would wish the authors incorporate temp/water effects into optimality theory in a more elegant way. The current way is too MODIS LUE style....

6) Current model evaluation is not enough. I strongly recommend testing the revised model at global scale across MODIS years. For example, Keenan et al (2017) showed recent increase of global GPP via P-model. Does the revised P-model still support this finding? Or does new modules of soil moisture and temperature reduce global GPP? I request this as P-model was already published so the authors may move many lines about original P model description to Appendix. The novelty of this model must be evaluation across diverse scales.

Only a few specific comments follow as the manusript is so well written. - L118: What was beta in Wang et al (2017a)? - L370: MODI -> MODIS

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-200, 2019.