Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2020-36-AC1, 2020

Interactive comment on "A daily, 250 m, and real-time gross primary productivity product (2000–present) covering the Contiguous United States" *by* Chongya Jiang et al.

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Received and published: 1 October 2020

Thank you for reviewing our manuscript "A daily, 250âĂĽm, and real-time gross primary productivity product (2000–present) covering the Contiguous United States". We have tried our best to address your comments and to improve our manuscript.

1. The authors have wrapped three separate innovations into a single paper. First, a new approach to estimating PAR from MODIS. Second, a new approach for estimating C3/C4 fraction. And third, combining those two new products with estimates of NIRv to derive GPP.

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We greatly appreciate your positive summary on our innovations.

2. For the PAR modeling, either more information is needed about the "four machine learning approaches" or the authors need to provide the code used to fit these models. Each algorithm has a multitude of adjustable parameters. How were these parameters determined, what are their values, and how might future researchers modify and/or improve upon the approach? It is also unclear to me how "uncertainty" is calculated. As written (L160), it appears uncertainty is represented in terms of model-model disagreement, as opposed to model-data disagreement. This seems inappropriate, though I know that the approach has been used in other parts of this literature. Trying to wrap my head around the uncertainty terms was complicated by the fact that Figure 2 reports PAR in W m2, while Figure 3 uses MJ m2 d1. Using common units throughout would be helpful. We totally agreed with your points.

For the modeling, we have added the following text to the manuscript: "We used Scikit-learn, a free software machine learning library for the Python programming language, to build the models. All the four algorithms were automatically optimized by tuning their hyperparameters using five-fold-cross-validation on their training dataset." For the uncertainty, we suppose product uncertainty information can be categorized into two types: theoretical and physical (Fang et al., 2012). While physical uncertainties indicate the departure of product values from hypothetical true values and are obtained through independent validation studies, i.e., model-data disagreement, theoretical uncertainties are caused by uncertainties in the input data and model imperfections, e.g., model-model disagreement, and are usually estimated by individual product science teams. The unit in Figure 2 is a typo, and we have corrected it to MJ m-2 d-1 in the revised version, which is consistent with Figure 3 and commonly used in light use efficiency studies. Thank you for pointing it out! Fang, H., Wei, S., Jiang, C., & Scipal, K. (2012). Theoretical uncertainty analysis of global MODIS, CYCLOPES, and GLOBCARBON LAI products using a triple collocation method. Remote Sensing of Environment, 124, 610–621. https://doi.org/10.1016/j.rse.2012.06.013

3. Some other minor issues on the PAR estimation. Equation 9 uses the term “fPAR”, which in this literature often means ‘fraction of photosynthetically active radiation absorbed by plants.’ (e.g, their equation 4). It’s quite confusing to distinguish between fpar and FPAR. Finally, the authors might consider a supplemental figure showing patterns of PAR/SW âĔŸAĔĞT this is a fairly well-studied, physically grounded ratio. It might also help to show that the approach works just fine in semi-cloudy (e.g, when there is more diffuse PAR) conditions.

We have followed all your suggestions in the revised manuscript. We have changed “fPAR” to “pPAR” (proportion of PAR in SW) to avoid misleading. We have added a PAR/SW ratio figure in the supplementary. We have also added a supplemental figure to show the error distribution of as a function of atmospheric transmittance (tSWR) to demonstrate that the approach works fine for any sky conditions.

4. For C3/C4, how are these uncertainties propagated into the final reported GPP estimates? From Figure 6b, it seems uncertainty is often quite high (e.g., > 40 percent). Accounting for this uncertainty seems important. For Figure 6 and 7, it could be helpful to change “Reference” to “CDL Reference” or something of that sort. At first, I was confused by the difference between “Reference” and “Ground Truth.”

We agree that the uncertainties in C4 crop fraction is large (e.g., > 0.4) in some areas in Figure 6b. However, the metric RMSE is sensitive to extreme values, and it is different from misclassification rate (0.4 does not mean 40%). For a pure pixel of a corn/soybean rotation field, the RMSE = 0.39 if three out of 20 years is misclassified, i.e., misclassification rate = 0.15. For example, in Figure 7b, predicted time series match well with the CDL reference in most cases, but the RMSE is 0.40. We use RMSE instead of misclassification rate because C4 crop fraction is a numerical variable. To avoid misleading, we have added this clarification in the revised manuscript, and added RMSE values in Figure 7 to provide an intuitive sense of this uncertainty. We have also replaced “Reference” by “CDL reference” following your kind suggestion.

5. Perhaps most importantly, the authors mostly ignore the poor performance of their approach at evergreen needleleaf sites. Figure 8D indicates that SANIRv is not a good predictor of daily iPUE at ENF sites. The result is mentioned in L374, but relatively little discussion is offered for why this might be the case or what should be done about it. From Figure 11b, it seems that combining uncertain estimates of iPUE with PAR somewhat alleviates the poor performance within ENF, though the ENF site with an R2 of less than 0.2 stands out.

We totally agree with your point. We have added more interpretation on the poor performance in ENF: "This relatively weak iPUE $\sim$ SANIRv relationship is expected because evergreen needleleaf forest tends to allocate resources for leaf construction and maintenance at large time scales and does not have much flexibility to change canopy structure and leaf color as a response to varying environment at small time scales (Badgley et al., 2019; Chabot and Hicks, 1982). Previous studies found that changes in xanthophyll cycle instead of chlorophyll concentration or absorbed PAR explained the seasonal variation of photosynthetic capacity in evergreen needleleaf forest (Gamon et al., 2016; Magney et al., 2019). Therefore, SIF was suggested by some studies as a better proxy of photosynthetic capacity in this ecosystem (Smith et al., 2018; Turner et al., 2019), though satellite SIF has coarser spatial resolution, shorter temporal coverage, and larger temporal latency, and lower signal-to-noise ratio than SANIRv."

For the ENF site with the lowest R2 (US-Me1, Figure R1), we found it was likely a SANIRv data processing issue. First, this site has strong seasonal variation in NIRv and thus was not detected as ENF using our algorithm. Second, our soil correction uses 20 years data to derive soil background NIRv. At this site, NIRv data in 2004 – 2005 winter was much smaller than 20 years average, possibly due to poor growth conditions during that period as well as the contamination of snow or cloud. As a result, our algorithm over-corrected soil effect, leading to a lot of 0 SANIRv in that winter, which in the end caused low R2 in GPP. However, we considered this site-year as an extreme

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case, which does not influence the overall quality of the product.

Figure R1 (see the attached figure). Comparison between AmeriFlux (black dots) and SLOPE (red curves) daily GPP at the US-Me1 site, which shows the lowest R2.

Badgley, G., Anderegg, L. D. ., Berry, J. A., & Field, C. B. (2019). Terrestrial Gross Primary Production: Using NIR V to Scale from Site to Globe. Global Change Biology, (April), 1–10. https://doi.org/10.1111/gcb.14729

Chabot, B. F., & Hicks, D. J. (1982). The ecology of leaf life spans. Annual Review of Ecology and Systematics. Volume 13, 13(1), 229–259. https://doi.org/10.1146/annurev.es.13.110182.001305

Magney, T. S., Bowling, D. R., Logan, B., Grossmann, K., Stutz, J., & Blanken, P. (2019). Mechanistic evidence for tracking the seasonality of photosynthesis with solar-induced fluorescence. Proceedings of the National Academy of Sciences, (27). https://doi.org/10.1073/pnas.1900278116

Gamon, J. A., Huemmrich, K. F., Wong, C. Y. S., Ensminger, I., Garrity, S., Hollinger, D. Y., . . . Peñuelas, J. (2016). A remotely sensed pigment index reveals photosynthetic phenology in evergreen conifers. Proceedings of the National Academy of Sciences, 201606162. https://doi.org/10.1073/pnas.1606162113

Smith, W. K., Biederman, J. A., Scott, R. L., Moore, D. J. P., He, M., Kimball, J. S., . . . Litvak, M. E. (2018). Chlorophyll Fluorescence Better Captures Seasonal and Interannual Gross Primary Productivity Dynamics Across Dryland Ecosystems of Southwestern North America. Geophysical Research Letters, 45(2), 748–757. https://doi.org/10.1002/2017GL075922

Turner, A. J., Köhler, P., Magney, T. S., Frankenberg, C., Fung, I., & Cohen, R. C. (2020). A double peak in the seasonality of California's photosynthesis as observed from space. Biogeosciences, 17(2), 405–422. https://doi.org/10.5194/bg-17-405-2020

6. A related, global scale effort to relate NIRv to GPP [Badgley, Anderegg, Berry,

Field, GCB2019] that the authors cite, identified 'deciduous' vs. 'evergreen' as being a critical parameter for model performance. Recognizing the difference in scales of the two analysis and the authors' stronger focus on C3/C4, it still feels necessary for a richer discussion of the performance of the model at ENF sites, especially given that the ultimate goal of the manuscript is to distribute a GPP dataset that researchers from across disciplines might find useful. From the analyses, it seems individuals working in agricultural contexts might find the data more reliable than those working in ENF systems. These caveats should be clearly flagged for the reader and the authors might benefit the research community by offering some discussion about what they think is going on and what future efforts might address such uncertainties.

(Badgley et al., 2019) separated 'evergreen' from 'deciduous' because they found "differing slopes for evergreen, deciduous, and crop ecosystem types". In our study, however, we found ENF has similar slope (c = 3.35) with all C3 PFTs (c = 3.46). Therefore, we decided not to separate them. We have made a richer discussion of the performance of the model at ENF sites (see response 5). In addition, we have further highlighted the limitation in the end of our Conclusion: "However, caution should be used in the interpretation of GPP seasonal trajectory in evergreen needleleaf forests because of relatively poor relationship between SANIRv and iPUE, and GPP magnitude in southwestern US grasslands because of the ignorance of fraction of C4 grasslands."

7. Finally, I find it perplexing that the manuscript lists five authors but the underlying data product only lists two authors. The author contributions indicate that author G.W. helped develop the SLOPE model and that authors B.P. and S.W. were involved in refining/interpreting how the model works. Doesn't the resulting dataset and the citations it might one day receive rely on the contributions of these three authors as well?

We considered model development, paper writing, and product generation as different works. Therefore, paper and product have different author lists.

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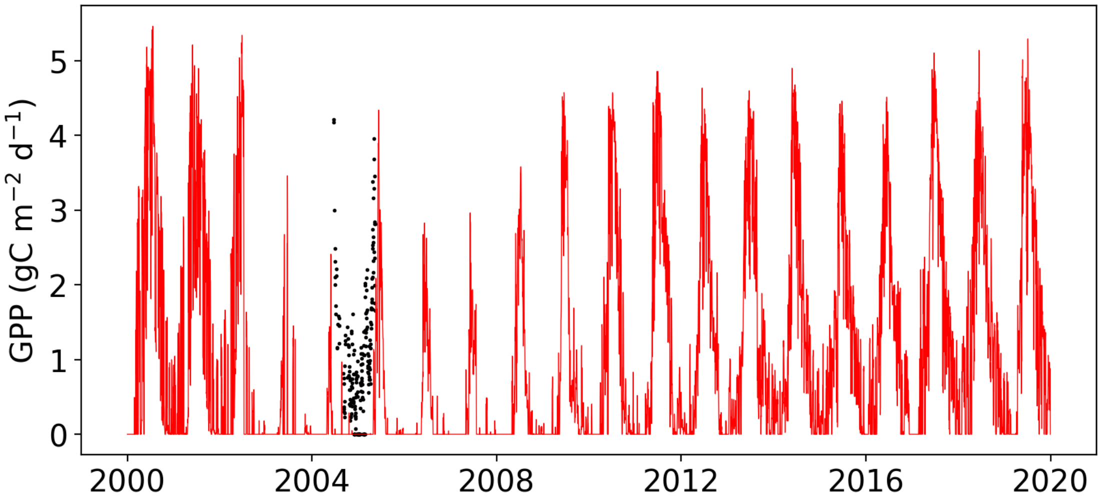

Fig. 1. Figure R1. Comparison between AmeriFlux (black dots) and SLOPE (red curves) daily GPP at the US-Me1 site, which shows the lowest R2.