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# *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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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. This manuscript details a new SateLite Only Photosynthesis Estimation (SLOPE) gross primary productivity (GPP) product based on: 1) near-infrared reflectance of vegetation (NIRv); 2) photosynthetically active radiation (PAR); and 3) C3/C4 fractional cover. The new product explains 84% of the spatial and temporal variance in GPP obtained from 50 Ameriflux eddy covariance flux tower (EC) sites. Critically, the prod-

uct includes uncertainty estimates at the pixel-level, an important advance over most existing products.

We greatly appreciate your positive summary on our manuscript.

2. Overall, I feel the paper is well written and the data product is of significant value, but perhaps mostly as an improved proxy for cropland productivity. The authors make importance advances over previous efforts by incorporating satellite-based NIRv and PAR and removing dependence on reanalysis-based weather data. The use of C3 and C4 fractional cover is appropriately done for croplands, but importantly it does not appear appropriately handled for natural ecosystems. The work adapts the commonly used light use efficiency framework, but removes all biophysical constraint logic (e.g., response to temperature, water, nutrient limitation), and instead makes the assumption that NIRv adequately captures these constraints. While this might be a fair assumption for herbaceous and deciduous dominated ecosystems, it is likely problematic for natural evergreen dominated ecosystems. Additionally, independent of vegetation type, it is unclear if NIRv is capable of capturing changes in LUE such that $CO_2$ fertilization effects are accurately represented in this product. Further, the authors utilize a classification that separates C3 and C4 vegetation functional types, however, their input data does not separate natural C3 and C4 grasslands, which is likely problematic for western US ecosystems (which are under-represented by eddy covariance flux towers, and thus the product is not well evaluated across these regions). In my view, these critical issues need to be fully addressed before this manuscript can be considered for publication.

Thank you for providing these deep insights. We totally agree with all of your points. First, SLOPE is developed to generate a high-spatiotemporal-resolution real-time GPP dataset with a reasonable overall accuracy. This objective fundamentally differentiates it from existing GPP products. In fact, we believe process-based model is the best way to quantify GPP, because it takes all influential factors (e.g., temperature, water supply and demand, radiation quantity and quality, $CO_2$ fertilization, nutrient limitation, leaf physiology, and canopy structure) into consideration. The authors have been persistently devoting to this direction (Jiang and Ryu, 2016; Jiang et al., 2020). However, we are aware that large uncertainties accumulate from both parameterizations and data, and therefore not suitable to generate a high-spatiotemporal-resolution real-time GPP dataset with a reasonable overall accuracy. Light use efficiency model removes heavy burdens from process-based model and is therefore more practical with a reasonable overall accuracy, but it also removes many useful features. Most LUE models only use information content in PAR, FPAR, temperature and humidity, without considerations of CO2 fertilization and nutrient limitation. The parameterizations of temperature and humidity effects are usually empirical, and brings in low resolution and large latency input data. SLOPE removes these uncertainties in both parameterizations and data through removing explicit biophysical constraints. Because plants adapt to environment and optimize their canopy structure and leaf physiology, NIRV,Ref is likely to capture a majority part of biophysical constraints. However, we acknowledge that where and when this strategy leads to more benefits requires much more investigations in the future.

Second, we totally agree with your point on ENF. 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.”

Third, it is true that the missing of C4 grasses is a limitation. We had a statement in the manuscript that "It is worth mentioning that C4 grassland and shrubland are not considered in this study as no nationwide high-resolution distribution data is available". 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."


Jiang, C., & Ryu, Y. (2016). Multi-scale evaluation of global gross primary productivity and evapotranspiration products derived from Breathing Earth System Simulator (BESS). Remote Sensing of Environment, 186, 528–547. https://doi.org/10.1016/j.rse.2016.08.030

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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

3. Line 50-60: Some valid points are made here. However, for the CONUS region in particular, there have been previous advances that already address many of these limitations. In particular, Robinson et al. (2018) utilized high quality weather data interpolated from dense weather station networks across the region and improved landcover data from the National Landcover Data Layer (NLCD). This is a much more appropriate data product to compare the new product against and, if it's available, I recommend this comparison.

Thank you for your kind suggestion. First, we have changed "50 km" to "> 10 km" in the revised manuscript. Second, NLCD still suffers from large time lag issue. Third, we cannot agree more on the importance to compare different products. We suppose comparison should be made in a comprehensive manner, as the authors did in the past (Jiang et al., 2016). However, this is too much for this manuscript and also is not encouraged by this journal. We plan to do that in a separate study.

4. Sections 3.1 and 3.2: I commend the authors for their work to provide robust uncertainty estimates for PAR and SANIRv, and subsequently SLOPE GPP estimates. This is an important advance over most previous efforts. Is the uncertainty due to PAR,

SANIRv, functional classification, and statistical fit included as separate data layers with the final product? This would be a useful addition for understanding pixel-level uncertainty. Also it would be useful to see a map that shows the dominant source of uncertainty at the pixel level, which would highlight potentially how uncertainty varies by region.

We really appreciate this highlight on our novelty. However, we are aware that the uncertainty definitions of different components are quite different: âĂć PAR: "Four different PAR estimations are then obtained by Eq. (9), and their ensemble mean and standard deviation are considered as the final estimation and uncertainty, respectively". âĂć SANIRv: "SANIRV is supposed to be smooth within a short time period, therefore, the standard deviation within the ±3-day temporal window is calculated as uncertainty". âĂć C4 crop fraction: "The RMSE between predicted and reference CDL C4 fraction is calculated as uncertainty". âĂć Slope coefficient: "The RMSE between SANIRV-derived and AmeriFlux iPUE for C3 and C4 are calculated as uncertainties of cC3 and cC4, respectively". Although we integrated them to provide a quantitative GPP uncertainty, the attribution to different components and the analysis of their relative importance may not make enough sense. Rather, we suppose investigating the spatiotemporal patterns of GPP product uncertainty makes a lot of sense. However, this requires a comprehensive analysis, as the authors did in the past (Fang et al., 2013), which is too much for this manuscript.

Fang, H., Jiang, C., Li, W., Wei, S., Baret, F., Chen, J. M., . . . Zhu, Z. (2013). Characterization and intercomparison of global moderate resolution leaf area index (LAI) products: Analysis of climatologies and theoretical uncertainties. Journal of Geophysical Research: Biogeosciences, 118(2), 529–548. https://doi.org/10.1002/jgrg.20051

5. Section 3.2: There is very low variance in SANIRv for evergreen vegetation (Figure 5), which results in the lowest correlation with EC GPP data. There are also a few ENF sites with very low correlation (Figure 11). This has been a well-known issue of vegetation reflectance-based indices, such as NIRv. Below are a number of papers that

discuss this issue and all indicate SIF could be a major improvement. I recommend at minimum that this issue and ways forward, such as downscaling TROPOMI SIF (Turner et al., 2019), be included prominently in the discussion of this paper.

Thank you for your suggestion. Please refer to our response 2.

6. Section 3.3: There are dynamic mixtures of C3 and C4 species throughout the natural ecosystems of the Western US. As far as I can tell this analysis and it's reliance on NLCD data is unable to account for these important ecosystems since NLCD does not represent C3 and C4 grasslands. These are also regions where EC sites are not well represented and thus the product 1) does not accurately capture; and 2) is not well constrained or evaluated across these ecosystems. At minimum, this needs to be pointed out very clearly throughout the methods and discussion of this paper. Alternatively, and maybe more appropriately, it seems the authors present an advanced cropland productivity product and perhaps natural regions including C3/C4 grasslands and evergreen forests should be masked out. It is my view that the current work has major limitations for accurately representing natural ecosystems.

Thanks for pointing out this issue. We were aware of the paper you suggested. However, the dataset and model generated in that paper was limited in a small region and we were unable to apply it to the whole CONUS. We had a statement in the manuscript that "It is worth mentioning that C4 grassland and shrubland are not considered in this study as no nationwide high-resolution distribution data is available". 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. The authors cite Badgley et al. (2019) as justification for separating the model based on only C3 and C4 functional types. Yet, that paper also indicated that the

best model fit between NIRv and GPP included separation between deciduous and evergreen ecosystem types as well. Based on this, the authors should include further justification for their model framework.

(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.

8. I recommend a map of the flux sites utilized overlaying the NLCD / CDL data utilized. This would highlight that the product has not been evaluated across important western US ecosystem types including dry herbaceous, shrub, and evergreen forests. Also for Table S1, one of the only dryland evergreen forest sites utilize (NR1) is reproduced twice and it’s unclear if years 2000-2007 or 2000-2014 are utilized in the analysis.

Thank you so much for point out this fault. Yes, we double-counted this site (US-NR1). We used two different GPP datasets, AmeriFlux L4 and FLUXNET2015. We gave priority to FLUXNET2015 and removed the overlapped sites from AmeriFlux L4. We have remade this table as well as all results. We have 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.”

9. Consider dropping the NASA Blue Marble background from all maps. This is unnecessary and potentially distracting.

We have followed your suggestion and remade all maps.

10. Why use a soil adjusted NIRv? One of the advantages of NIRv is that is naturally isolates a pure vegetation signal (Bagdley et al., 2019).

Because NIRv is still influenced by soil signal. We had a statement on this concern:

“When vegetation is absent, iPUE is zero and NIRV,Ref should be zero too. However, this is not true in reality as >99.9% soils have positive NIRV,Ref values according to a global soil spectral library (Jiang and Fang, 2019), and the correction of NIRV,Ref for soil is needed for better performance at low vegetation cover (Zeng et al., 2019).” By correcting the soil effect, the intercept term can be removed from the iPUE $\sim$ NIRV,Ref relationship.

11. I recommend including in the discussion whether this model is capable of capturing $CO_2$ fertilization effects on vegetation productivity. Previous work has suggested that LUE models may not have the capacity to fully capture this important and rapidly changing driver of GPP. Pointing out this potential limitation is important to ensure appropriate data usage by the community.

This is a great point. We have added one sentence in Conclusion: “Although the SLOPE product has been generated from 2000 to present, caution should be used in the interpretation of long-term trend because the SLOPE model, as many other LUE models, does not explicitly consider the $CO_2$ fertilization effects on vegetation productivity.”

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