Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2020-36-RC2, 2020
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Open Access Earth System Science Data Discussions

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.

Anonymous Referee #2

Received and published: 6 July 2020

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

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

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Discussion paper

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.

Major Comments: 1. 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.

Robinson, N.P., Allred, B.W., Smith, W.K., Jones, M.O., Moreno, A., Erickson, T.A., Naugle, D.E., Running, S.W. Landsat 30 m and MODIS 250 m derived terrestrial primary production for the conterminous United States. 2018. Remote Sensing in Ecology and Conservation DOI: 10.1002/rse2.74.

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2. 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.

3. 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.

TS Magney, DR Bowling, BA Logan, K Grossmann, J Stutz, PD Blanken, et al. 2019 Mechanistic evidence for tracking the seasonality of photosynthesis with solar-induced fluorescence. Proceedings of the National Academy of Sciences, 116, 11640-11645

Smith, W.K, Biederman, J.A., Scott, R.L., Moore, D.J.P., He, M., Kimball, J.S., Yan, D., Hudson, A., Barnes, M.L., MacBean, N., Fox, A., Litvak, M.E. Chlorophyll Fluorescence Better Captures Seasonal and Interannual Gross Primary Productivity Dynamics Across Dryland Ecosystems of Southwestern North America. 2018. Geophysical Research Letters DOI: 10.1002/2017GL075922.

AJ Turner, P Köhler, TS Magney, C Frankenberg, I Fung, RC Cohen. 2019. A double peak in the seasonality of California's photosynthesis as observed from space. Biogeosciences 17, 405-422

4. Section 3.3: There are dynamic mixtures of C3 and C4 species throughout the

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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.

Yan, D., de Beurs, K.M. 2016. Mapping the distributions of C3and C4 grasses in the mixed-grassprairies of southwest Oklahoma using the Random Forestclassification algorithm. International Journal of Applied Earth Observation and Geoinformation 47, 125-138.

5. 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, G., Anderegg, L.D., Berry, J.A. and Field, C.B. 2019. Terrestrial Gross Primary Production: Using NIRv to Scale from Site to Globe. Global Change Biology DOI:10.1111/gcb.14729.

6. 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.

The below referenced paper covers the major challenges associated with extrapolating algorithms across drylands without appropriate evaluation.

Smith, W.K., Dannenberg, M.P., Yan, D., Herrmann, S., Barnes, M.L., Barron-Gafford, G.A., Biederman, J.A., Ferrenberg, S., Fox, A.M., Hudson, Knowles, J.F., MacBean, N., Moore, D.J.P., Nagler, P.L., Reed, S.C., Rutherford, W.A., Scott, R.L., Wang, X., Yang, J. 2019. Remote sensing of dryland ecosystem structure and function: Progress, challenges, and opportunities. Remote Sensing of Environment 233, 111401.

Minor Comments: 1. Consider dropping the NASA Blue Marble background from all maps. This is unnecessary and potentially distracting. 2. 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). 3. I recommend including in the discussion whether this model is capable of capturing CO2 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.

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