

Biogeosciences Discuss., editor comment EC1  
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## Comment on bg-2021-49

Paul Stoy (Editor)

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Editor comment on "Extreme events driving year-to-year differences in gross primary productivity across the US" by Alexander J. Turner et al., Biogeosciences Discuss.,  
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Dear Dr. Turner,

I am now in receipt of a third review, which I pasted below. Please also consider this Referee's comments in your response, which I am looking forward to reading.

Sincerely,

Paul C. Stoy

**Manuscript:** Extreme events driving year-to-year differences in gross primary productivity across the US, by A. Turner and coauthors

**Summary:** The authors take TROPOMI SIF retrievals from 2018 and 2019 and correlate them with Ameriflux GPP estimates to calculate a relationship between SIF and GPP. They then downscale this relationship to 500m resolution using NIRV to calculate GPP over CONUS using a 16-day moving window. They find a small difference in total CONUS GPP between 2018 and 2019 (4%), and determine that 28% of this variability can be explained by 4 precipitation-associated events, in Texas, the Midwest, South Dakota, and California.

**Review:** There is nothing explicitly wrong with this paper. The methods are sound, and the results are reasonably explained by the data. That being said, my overall impression of the paper is that there is nothing novel or new here. The linear relationship (over large spatiotemporal scales) between SIF and GPP has been in the literature for a decade. That we can also see it in TROPOMI is not a dramatic finding. Previous work (e.g. MODSIF) has also downscaled SIF data. We also know that GPP is suppressed in drought. Again, a whole body of work. Vegetation response to drought as expressed by reduced SIF has also been previously reported. The suppression of early-season GPP due to Midwest flooding was in a paper last year. Using a new dataset to confirm previous results is not exactly groundbreaking. After reading the paper, my reaction was "well, yeah." Nothing new here,

sort of a 'me too' paper, confirming previous results with a new dataset.

The title of the paper states that extreme events drive year-to-year variability in GPP. Between 2018 and 2019, the amount of IAV explained by the 'extreme' events emphasized is just a little over 25%. What about the other 75%? Are there other events that were not included in the study and would therefore increase the fraction of variability due to extreme events? Why were they not included? Or is the majority of IAV explained by smaller anomalies (that don't qualify as extreme) in precipitation, humidity and/or temperature? If the latter, then the paper title is demonstrably false. The claims made are qualitative, and after reading the paper I was not convinced that the title was a true statement.

I can't in good conscience recommend a paper for publication just because they didn't do anything 'wrong'. I think there is a responsibility on the authors' part to present new, innovative and useful information to the community when submitting a paper, and I don't think that bar has been cleared here.

I find myself wondering about the word 'extreme'. It's in the title (and not defined in the paper), so let's think about it a little bit. How might one define 'extreme event'? Is there a quantitative metric? I'm thinking about drought specifically, since 3 of the 4 'extreme' events involved precipitation deficit. There are multiple products that can be used to define drought (or high precipitation that might lead to flooding), such as the University of Nebraska-Lincoln Drought Monitor, Standardized Precipitation Index, or Palmer Drought Severity Index that are easily available in gridded form. It would not be difficult to define 'extreme' using one (or more) of these products and then correlate the regional differences in GPP with them. The title of the paper could then be confirmed or refuted quantitatively.

2018-2019 differences in GPP could be stratified by drought/excess precipitation metric, and the annual difference in GPP could be quantified in terms of fractional contribution by 'extreme' (greater than +/- 1 standard deviation in the index chosen?) and fractional contribution by anomalies that don't qualify as extreme. I'd be interested to see that. One might also look at these differences on a seasonal and/or PFT-level basis. Does 'minor' (not extreme) variability in eastern CONUS drive overall CONUS variability because mean GPP is so much larger than in more arid regions (the west)? That's an interesting question as well.

My formal recommendation for the paper is rejection, but I think with a little more effort the datasets the authors have produced can be used for research that has real value. If that effort can be put forth, I think resubmission would be entirely appropriate.

### **Specific Comments:**

- Page 4, L30-31: "...by assuming that GPP scales linearly with PAR..." This statement is just plain wrong. There is a very large body of observational work that demonstrates it. The idea that the GPP/PAR relationship is nonlinear is the basis of every terrestrial biosphere model, from light-response models like VPRM or CASA to enzyme-kinetic models such as CLM, ORCHIDEE or SiB. The authors of the paper know this, so I'm assuming that there are assumptions behind this statement. They need to be explained, and the statement justified (much) more than just a statement of fact. It is not.
- There are multiple references that are incorrectly formatted.
- The annual GPP was given (0.6-0.7 PG C), but comparison to other products would be helpful. It is well-known that GPP simulations (global or regional) vary by a factor of two or more, so it would be informative to know where the GPP product generated here sits in that spectrum, not just in terms of annual total but seasonal/regional comparison as well.