Biogeosciences Discussions

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Comment on bg-2021-354

Christian Frankenberg (Referee)

Referee comment on "Sun-induced fluorescence as a proxy for primary productivity across vegetation types and climates" by Mark Pickering et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-354-RC1, 2022

The manuscript by Pickering et al provides an in-depth comparison of downscaled Solar Induced Chlorophyll Fluorescence from GOME-2 with upscaled GPP estimates from FLUXCOM-GPP. The paper is in general well written and certainly of interest to the community. I do have some higher level comments that I would like the authors to address before it can be accepted.

- The authors compare down-scaled SIF with FLUXCOM GPP. Downscaled SIF using from GOME-2 can include two sources of error: I) GOME-2 retrievals are known to be somewhat less accurate than say OCO-2 and TROPOMI and II) The downscaling itself might introduce errors. Given that we have more than 2 years of TROPOMI data, I don't understand why a simple test of downscaled GPP with "original" TROPOMI SIF data can be performed. This would help evaluate the robustness of the product used.
- Please always provide the reference wavelength for SIF (which is wavelength dependent) and clearly state whether it was length-of-day corrected or not.
- The dataset by Koehler et al wasn't used but that decision is not well motivated (or described). What "bias" are the authors talking about? Statements like these really need to be rigorous, right now it is rather sloppy.
- To me, there is some circularity in the interpretations. Most importantly, the authors state that: "Proving this technique at a global scale provides evidence for the use of high-resolution SIF in monitoring the resilience of local ecosystems to environmental fluctuations, an area of growing importance as extreme weather events become more frequent and more severe". This statement is far reaching but it is actually based on just a comparison with FLUXCOM GPP, which implies that FLUXCOM GPP has the same potential (and could be provided in near real time as well). Thus, it is unclear what SIF could do that FLUXCOM (or other pure remote sensing products) can't. The interesting cases would be those in which the products disagree but the author's statement is based on the agreement in the IAV between the two.
- Some (if not all?) of the variables analyzed (VPD, radiation) are also included as driver variables for FLUXCOM. It is thus unclear whether we are learning something new. The authors could do the same analysis as in Figure 10 but for FLUXCOM-GPP as well to evaluate whether the drivers (or limitations) between the datasets are identical or not. Only then would we learn something in my mind, right now a lot of the analysis is

somewhat phenomenological.

Some minor comments:

Line 54: Please cite some of the original works on SIF and GPP as well (e.g. Joiner et al and Frankenberg et al).

Line 58: Frankenberg and Berry don’t really talk about water availability. Maybe rather about a lower dynamic range in SIF yield vs GPP yield once stress kicks in.

Line 79: Please add citations for those data-products

Lines around 156: Lower bias, higher level of agreement: Please be more concrete, this could be anything. It is important to differentiate absolute biases (scaling factors), which are trivial from worse agreement as seasonality is not well captured. Also, this statement shows that there is considerable uncertainty in GOME-2 itself, thus it would be important to know whether the authors would draw different conclusions if they had chosen another data product.

Line 184: I really don’t understand why the authors are working at 0.05 degrees rather than just aggregating everything to the native FLUXCOM resolution. Is there any good reason to introduce potential interpolation errors. My guess is the reason is convenience but please prove me wrong.

Line 237: What is true though is that if SIF is zero, there certainly is no GPP (but not necessarily the other way around). Thus, there is a biophysical reasoning behind that assumption. Maybe the linearity assumption is the one that could be questioned?

Figure 3: The IAV correlations are surprisingly good. It would be VERY interesting to compare the SIF-GPP slopes derived intra-annually from those inter-annually.

Figure 6: Please use higher resolution for the final version (or vector graphics)

Line 454: “high VPD correlates with high cloud cover”. I must be reading this wrong, it doesn’t make sense and the causality of the sentences here is somewhat strange. Large scale atmospheric dynamics drive cloud cover and humidity, hence also VPD, temperature and solar radiation. There are feedbacks but it reads as if VPD is in the driver's seat here, which it isn’t

Line 469: Again, this statement requires caveats.

Line 481: “Purity” maybe state “quality”?

Line 503: Given the low dynamic range of tropical GPP, this is not surprising. So the question is whether the lower correlation is just due to the lower dynamic range in the presence of noise or something else?

Line 581: See above, these statements can’t be made without explicitly re-stating the assumptions or caveats.