Biogeosciences Discussions

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Comment on bg-2021-354

Russell Doughty (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-RC3, 2022

Pickering et al. used a downscaled GOME-2 SIF dataset and FLUXCOM GPP to investigate the SIF-GPP relationship and its drivers for different land cover types.

The paper is well written, and I certainly commend the authors for undertaking such a broad global analysis. I would recommend publication, but first I would really enjoy seeing the authors' discussion on some tougher questions facing the SIF community regarding downscaled products and our interpretation of the results.

Major points

Do comparisons among products tell us anything?

I am skeptical of analyses that compare products and interpret the results as containing empirical information or insights into their relationships. For instance, downscaled SIF is not SIF and FLUXCOM GPP is not GPP. To me it seems their relationships would be very sensitive to or determined by their respective errors. I do commend the authors for describing SIF as downscaled SIF and GPP as FLUXCOM GPP in the text and the figures, but does their relationship really tell us anything about SIF and GPP? Many other papers written with these products or similar products do not often make this distinction, and simply interpret downscaled SIF products as SIF and GPP products as GPP. So, thanks to the authors for being more diligent.

On this note, I think it is a fair question to ask why a downscaled product was used - why not use the raw TROPOMI data. Or use them both and discuss how the results differ. If you repeated this analysis with gridded TROPOMI SIF data, would you get the same results? We have demonstrated ways to use ungridded (Doughty et al. 2019 PNAS) and gridded TROPOMI data (Doughty et al. 2021 JGR) for such analyses.

Downscaled SIF products

A couple of comments regarding the downscaled SIF products. This is certainly not intended to be a jab at the SIF-LUE product, but I think there are a couple of issues with most of these products that have not really been addressed yet.

First is that have shown in my JGR 2021 paper that there is a very weak or often no correlation between VIs and SIF in the tropics, and I have found the same to be true when using TROPOMI SIF and TROPOMI surface reflectances. However, the downscaled products use VIs or surface reflectance, along with machine learning or environmental scalars such as we use in LUE models, to predict SIF.

How sound is it to predict SIF with surface reflectance or VIs in the tropics when they lack a correlation? SIF is affected by physiological processes that do not affect leaf/canopy optical properties - so is it really safe to assume that we can use reflectances to predict SIF? This question is particularly important for the tropics since they are such a strong driver of annual and intra-annual GPP and XCO2.

Second, do the downscaled products actually reproduce the SIF signal? The downscaled SIF products were produced before we had a sizable amount of TROPOMI data, but now we have four full years of TROPOMI data. Ideally, platforms with more coarse spatial and/or temporal resolutions (GOSAT, GOME-2, OCO2/3) would capture the seasonality of SIF in the tropics as observed by a near-daily observer like TROPOMI - but do we know that yet? And do their downscaled products reproduce the SIF signal, the VI signal, or something in between?

Analysis by land cover type

Personally, I am not a fan of grouping land classes to investigate drivers of variables – in this case SIF and GPP. For instance, GPP in EBF in Africa or SE Asia can be driven by a different set of drivers than those in the Amazon. Even within the Amazon basin itself, there is a distinctive gradient in precipitation, temperature, VPD, etc. that is not static in space or time. Drivers of photosynthesis are determined locally by local environmental present and historical factors, disturbance history, species composition, human management, physiological processes, and many other local factors other than just land cover functional type.

Thus, drivers should be investigated at the pixel level. Why not determine the drivers and their strengths and show it on a map? I am highly skeptical of any results that claim things like 'GPP for this land cover type is driven by x' or 'SIF is driven by x for this vegetation type'.

Also, the majority threshold used is somewhat subjective and arbitrary. Even at 75% majority land cover type, a sizable portion of the signal (GPP, SIF, or spectra) is driven by a land cover type other than the one you are interested in. Thus, there is an inevitable bias in the results that can't be remedied. For instance, the seasonality in moist EBF of the Amazon is extremely subtle. Thus, even a small area of another vegetation cover type, such as crop or grassland, may dramatically alter the seasonality for a gridcell. Also, setting a 100% land cover threshold is unreasonable as one will end up with very few pixels for analysis, especially at 0.05-degree resolution.

I have done these analyses myself while writing my 2021 paper published in JGR. I began the analysis by grouping by land cover type, but I obtained very different answers according to the majority % cover threshold that I used. I actually scrapped the entire paper and analysis in favor of showing the SIF-GPP and SIF-VI relationships at the gridcell level as maps, as it was not fair to extrapolate a relationship among all land cover classes globally as being characteristic for that land cover type when in reality the relationships and spatio-temporal relationships were much more complicated. And there are a lot of maps in that paper!

Would we expect the SIF-GPP relationship to be static?

The SIF-GPP relationships shown in Figure 5 - wouldn't we expect these relationships to vary over time and according to vegetation stress and other factors? Perhaps there is a seasonality to the relationships? What about a time series of their slopes, R2, or p values?

Minor comments

Land cover data – Is it really the case that the pixels selected had 'no change' over 2007 – 2014? Classification errors can cause estimated land cover fractions to change slightly from year to year. Also, land cover is changing. What was the threshold for no change? If a gridcell changed by 1%, from let's say 95% to 96%, or vice versa, was it excluded? Or did you mean there was no change in the majority land cover classification?

Line ~40: There was a good LUE model review paper recently, see Yanyan Pei et al (2022).

Line 69-70 – This linearity can be said for spaceborne SIF sometimes, but certainly not tower or leaf-level SIF measurements. Youngryel Ryu showed that at the short term, SIF is more related to APAR, and the Marrs study and Helm study show that SIF and GPP can be decoupled at both short- and long-term scales and at both the leaf and canopy scale.

Line 83 – The argument for not using TROPOMI is confusing. Apart from OCO2/3, it has the highest spatial resolution and certainly the highest resampling with near daily global coverage. GOME-2 is far inferior in these respects, so I recommend the authors better justify their use of GOME-2. There is certainly nothing wrong with the authors using their downscaled GOME-2 product, I am happy to see it.

Downscaled product - Can you include your equations here? Also a brief description of your data sources for each variable would be helpful. From what I remember, the method follows SIF = f(VI) * f(T) * f(W) using MCD43C4 (VIs), MYD11C2 (LST), MOD16A2 (ET).