

Biogeosciences Discuss., author comment AC2
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Reply on RC1

Mark Pickering et al.

Author comment on "Sun-Induced Fluorescence as a Proxy of Primary Productivity across Vegetation Types and Climates" by Mark Pickering et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2021-354-AC2>, 2022

- *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).*

Thank you for pointing this out. We will include the following in the introduction:

- (introduction para3) First observations of global and seasonal terrestrial chlorophyll fluorescence from space, J. Joiner et al. <https://bg.copernicus.org/articles/8/637/2011/2011>
- (introduction para3) Disentangling chlorophyll fluorescence from atmospheric scattering effects in O2 A-band spectra of reflected sun-light C. Frankenberg et. al <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010GL045896> 2011
- Note, we have also added this reference for an alternative downscaling approach via a neural net ~ L85 <https://bg.copernicus.org/articles/19/1777/2022/>

- *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.*

This comment refers to line 56 : '[SIF] is considered to be the mechanism developed by plants to respond near-instantaneously to rapid perturbations in environmental conditions of light, temperature and water availability'

Yes, the reference we cite talks about reduced SIF yield when undergoing 'drought stress' (Fig. 2 in

<https://www.sciencedirect.com/science/article/pii/B9780124095489106323?via%3Dihub> shows simulated responses based on a parameterization with leaves undergoing or recovering from drought stress). But we agree drought stress isn't a 'rapid perturbation'.

We will change the line to '... to respond near-instantaneously to rapid perturbations in the environmental conditions of light and temperature, with the SIF yield also dependent on biophysical conditions such as the concentration of the CO₂-fixing enzyme Rubisco and drought stress'

- Line 79: Please add citations for those data-products

Thank you, we will add at this position:

Retrieval and global assessment of terrestrial chlorophyll fluorescence from GOSAT space measurements LuisGuanter et. al.

Filling-in of near-infrared solar lines by terrestrial fluorescence and other geophysical effects: simulations and space-based observations from SCIAMACHY and GOSAT J. Joiner¹
Global monitoring of terrestrial chlorophyll fluorescence from moderate-spectral-resolution near-infrared satellite measurements: methodology, simulations, and application to GOME-2}, Joiner et al.

Overview of Solar-Induced chlorophyll Fluorescence (SIF) from the Orbiting Carbon Observatory-2: Retrieval, cross-mission comparison, and global monitoring for GPP

Global Retrievals of Solar-Induced Chlorophyll Fluorescence With TROPOMI: First Results and Intersensor Comparison to OCO-2 Philipp Köhler

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL079031>

- *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.*

Please see the response to comment 2. regarding what is meant by bias here. Hopefully this clarifies the issue.

- *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.*

The native resolution of the FLUXCOM RS +METEO run is 0.0833 dd, so bringing the 0.05 dd resolution of the downscaled SIF to 0.0833 would also lead to potential interpolation issues. To avoid interpolation issues, we would do better to aggregate all to a common resolution of 0.25dd, but since the entire point of the downscaling operation is to be closer to the actual elements on the landscape, aggregating and smoothing the fine granularity that has been obtained by downscaling seems to be a missed opportunity. Furthermore, by construction FLUXCOM is bound to be already spatially smoother than SIF. We have thus decided to bring everything to 0.05 and tolerate the potential small interpolation effects that might occur in FLUXCOM.

- *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?*

We agree that it is the linearity assumption that should mostly be questioned here: we don't believe there is any evidence that SIF and GPP do scale linearly all the way to zero, and indeed we don't think there is any reason to believe it should scale like this (especially when we are considering different scales e.g. leaf level effect, or reducing further to quantum level effects). Forcing through zero a relationship that does not evolve linearly to zero will cause us to mis-estimate the parameters in the area where the relationship is linear, or at least more closely linear. Also, as stated by the reviewer, it may not necessarily be correct that when GPP is zero SIF must also be zero (in which case even at macro canopy scale, the intercept may not be zero).

Additionally, as a broader statistical comment, there are also a theoretical motivations for why one should never (more or less) force a linear regressions through zero, particularly on measured real world data.

- *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.*

Thank you for the suggestion. We will produce them and analyse them, and then incorporate the results in the text.

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

Thank you, we will implement this in the update.

- *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*

This is relation to the statement here:

'Strong correlation is noticeable in the SIFDS response to meteorological fluctuations, as can be seen clearly in equatorial rainforests in figure 9: high VPD correlates with high cloud cover, limiting the solar radiation arriving at the leaves and, naturally, reducing temperatures and affecting rainfall (and therefore soil moisture)'

What we are trying to say here is that - looking at Figure 9 - the SIF responds in a similar, consistent way to many of the variables (e.g. in arid croplands, there are similar magnitude shifts in T2M, SSR and VPD, - and a similar reverse magnitude shift in SM - or in equatorial rainforests VPD SSR and T2M respond with similar magnitude). This is likely due to the effect of the meteo variables being co-dependent and can't be treated in isolation. Therefore we can't just say in isolation 'as one variable - e.g. VPD - increases, SIF/GPP increases' because each variable is related to the others. For example an decrease in VPD might generally occur (through some dynamic atmospheric effect, the details of which are unimportant) concurrently with a decrease in temperature and so it is not possible to say which meteorological is really causing the change in SIF (is it the decrease in temperature, or the decrease in VPD driving the SIF)? In reality the variables are all inter-related.

Re-reading the statement now, we agree that it is quite messy and overreaching the original point (which is already captured by preceding sentences) and implying a broader statement on the nature of atmospheric dynamics in rainforests, which we didn't intend.

We will substitute the line with:

'Codependence between the atmospheric variables means that it is difficult to directly explain fluctuations in SIF via individual meteorological variables in isolation, for example, the correlation between warmer temperatures and high VPD, results in a similar SIFDS response...'

- *Line 469: Again, this statement requires caveats.*

Please see the response to comment #4 and see the response to Reviewer 2's comment regarding the fact that we are perhaps over-stating the results with the use of the words 'measure of environmental stress' and 'measure of resilience'.

In this particular case, we will make the change:

L469 'In this context the study suggests that it is possible to use high-resolution SIF as a near-real time measure of the resilience of ecosystems to climate fluctuations'

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'In this context the study suggests that it may be possible to use high-resolution SIF as a

near-real time measure of the response of vegetation productivity to climate fluctuations'

- *Line 481: "Purity" maybe state "quality"?*

We agree and will change it in the revised version.

- *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?*

Yes, we agree. And the fact that the correlation is suppressed both spatially and temporally suggests that it is an issue of noise, or, similarly, high uncertainty in the FLUXCOM model in the tropics. We make suggestions as to the reasons behind this lack of range (e.g. saturation of fapar or constraints in the FLUXCOM model). As we mention in the discussion, the greater dynamic range in SIF opens the possibility of it's inclusion as a variable in the FLUXCOM machine learning model.

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

Please see similar responses in comment #4 and on the theme of 'environmental stress' . For this line in particular, we make the previously noted change:

L580 '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'

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'Proving this technique at a global scale demonstrates that high-resolution SIF responds to meteorological fluctuations in a similar way to FLUXCOM GPP. As such it has potential as a near real-time indicator of vegetation status that, unlike FLUXCOM GPP, is independent of meteorological variables.'