This review was prepared as part of graduate program course work at Wageningen University, and has been produced under supervision of Prof Wouter Peters. The review has been posted because of its good quality, and likely usefulness to the authors and editor. This review was not solicited by the journal.

This manuscript presents the applicability of remotely sensed SIF datasets to explain variability and seasonal dynamics of photosynthesis. Oversampling and downscaling manipulations on the TROPOMI SIF dataset were used to acquire daily estimates of SIF. Two major advantages of SIF over MODIS datasets are its low sensitivity to clouds or aerosols and its ability to detect a decrease in photosynthesis even when plants stay green. A double peak in the seasonal variability of SIF was found and linked with out-of-phase vegetation photosynthesis: woody grasslands and evergreen forests. This double peak was not found in MODIS datasets, even though there was strong correlation between MODIS and SIF on the annual timescale. This correlation was significantly weaker at daily and monthly timescales. Variability in spatio-temporal patterns and its relation with variability in TROPOMI SIF data was determined by using EOFs. These EOFs confirmed the double peak and showed that spatio-temporal vegetation patterns explain 84% of the variability in SIF data.

In my opinion, the study is interesting and introduces a relevant novelty in the narrow scientific community bridging remote sensing science and photosynthesis research. However, I think three flaws are present in the current manuscript, of which I recommend some revisions before publication.

To start with the first issue, on page 9 (lines 7-27) a fourteen-day moving window is used in combination with a spatial downscaling method to obtain daily estimates of SIF at a high resolution. In combination with the consequent pre-weighting of the SIF signal by the underlying vegetation fraction (MODIS NIRv), large-scale changes in spatio-temporal patterns are conserved. On lines 20-21, the authors assume that the observed SIF from TROPOMI likely originates from more vegetated regions within that scene. However, the R^2 value of the linear correlation between SIF and NIRv (0.52, Figure 2 on page 7) implies that a significant part of the variance in SIF cannot be explained by the underlying vegetation fraction. Besides, by using the averaged value of the 14-day moving window, a pseudo-daily average SIF value is created, rather than the actual daily value. This is fine, provided that a certain accuracy assessment is conducted. Especially because the authors mention, on page 9 lines 31-32, significant differences are found with the similar method of Köhler et al. (2018) in which a quality control and accuracy assessment are indeed present. In addition, the downscaling (from 24.5 km^2 to 0.25 km^2) is likely to introduce inaccuracies, which requires quantification.

Therefore, it is recommended that the authors provide more evidence that observed differences in SIF are indeed linked to more vegetated regions, rather than barren areas, water bodies or other external factors. Liu et al. (2019) should be consulted, who state that scattering and re-absorption of sunlight into the leaves means that the SIF measured at canopy level is only part of the total ecosystem SIF emission.

Secondly, from page 11 onwards, the authors use a method to infer GPP from SIF, based on light-use efficiency and the probability of SIF photons escaping the canopy. Interestingly, Paul-Limognes et al. (2018) found that SIF was more affected by environmental conditions than GPP. Midday-depressions in SIF were linked to peak VPD values following peak photosynthetic photon flux density (PPFD). Besides, Walther et al. (2016) state that in evergreen needle-leaf forests, the length of the photosynthetically active period indicated by SIF is up to six weeks longer and commences a month earlier than the green biomass changing period proxied by EVI. Even though the authors used NIR_v instead of EVI to downscale SIF, the different timing could significantly alter the double peak structure. Moreover, the authors state there is a lack of GPP measurements in evergreen forests, while much of California is dominated by this vegetation type (page 13, line 17-19). In combination with the a-synchronous SIF/MODIS dynamics, this will propagate into a major bias in the scaling factor of 18.5 ± 4.9 which is inferred on page 13, line 14. Therefore, I think that the equation on page 13, line 20 ($GPP := 18.5 * SIF$) should include a revised quantification of the error margins. In doing so, the authors should determine an alternative error margin whilst taking into account the fractional contribution of evergreen forests to GPP. The latter can best be inferred from a biosphere model or studies which used eddy-covariance measures in similar evergreen forests.

Lastly, the authors successfully identify a double-peak in the seasonality of GPP. However, the number of (recent) references concerning underlying reasons for this double peak or other case studies in which a double peak is found, is unsatisfactory. References to Xu and Baldocchi (2003), Xu et al. (2004), Xu and Baldocci (2004) explain changes in carbon fluxes between ecosystems and vegetation types well, yet the link with SIF dynamics is lacking (Page 17, lines 15-22). Perhaps the following is a cause of the state-of-the-art novelty of this subject, but there are zero references made to any other recent papers discovering the double peak in GPP/SIF. Given the importance of this conclusion to the subject of the manuscript, I highly suggest investigating and mentioning recent existing literature explaining the double peak phenomenon. If the latter turns out to be infeasible because it is such a novelty, it is suggested to emphasise the scientific novelty in this paper. For instance, Li et al. (2014) imply that MODIS EVI is unsuitable for detecting a double peak in vegetated areas which usually manifest double peaks. This would strengthen the relevance as to why SIF needs to be used.

Minor arguments and small revisions

Minor issue 1: In Table 1, all vegetation types have two or more study sites except for the WSA (Woody Savannas). I would like to give the authors awareness that one study site might not be representative for the entire ecosystems, especially when all other vegetation types have multiple sites.

In Figure 1, Page 3: The description mentioned that black stars show the location of six Ameriflux sites, However I can only discern three and they seem to be closely packed at this resolution.

In Figure 2 on page 7, the axes lack titles. This is relevant to include for the x-axes of the bottom row of graphs, as the range of the axes are different.

In Figure 3 on page 8 the swath resolution is 4.0 km x 7.0 km, whereas in the text on Page 9, line 4 it is stated that this resolution is 3.5 km x 7.0 km. This should match.

On page 9, line 19-20: perhaps it is necessary to introduce that the NIR_v was used in the pre-weighting of SIF, rather than introducing it later on Page 11, line 5.

On page 14 in the figure description, a reference to Panel G' is made, whereas this panel is not present in the accompanying figure (6).

On page 16, line 8-9 it is stated that a 'reasonable consistency' is found. This should be quantified.

In the conclusion on page 20, parts of line 6-7 and line 22-23 have very similar information.

References

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