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# *Interactive comment on* "A double peak in the seasonality of California's photosynthesis as observed from space" *by* Alexander J. Turner et al.

**Anonymous Referee #2**

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The authors of this manuscript present work in which they use recent SIF retrievals from the Tropomi sensor over California and techniques of oversampling and downscaling to obtain a fine spatial resolution of 500m. They compare those to flux tower-derived GPP estimates and find generally good agreement and a linear relationship, which they use to scale SIF to obtain a spatial GPP estimate from SIF. Averaged over the whole state of California they present a double-peak in the seasonal cycle of SIF that is present in both 2018 and 2019 and can be explained by different phasing of vegetation activity in different vegetation types (as shown by time series per biome and by decomposition via EOF analysis). The trajectory of the greenness indices is very dissimilar and the authors explain this with a possible clear sky bias and the fact that SIF is more closely linked to photosynthesis in contrast to vegetation indices.



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This work is highly interesting and its the main contribution to the scientific community is twofold: a) they introduce novel methods of oversampling and downscaling to the SIF community which offers the exciting opportunity to analyse SIF data at unprecedented spatial resolution. They also raise awareness of possible prominent retrieval biases related to bright surfaces. b) The different dynamics between canopy greenness and photosynthetic activity and the resulting benefits of SIF in general, and of Tropomi SIF in particular, to track GPP dynamics.

However, I have two major concerns regarding the second point and main message of the paper which in my opinion are necessary to address before a publication in Biogeosciences:

First, it is not fully clear from the work description if the comparison between greenness and SIF is meaningful: Have any filters been applied to either tower measurements, SIF or the MODIS data to remove low quality data? Also, the authors mention a clear-sky bias of the reflectance measurements as a possible explanation for the different dynamics. This implies to me that the data of VIs and SIF have not been matched in space and time before aggregating to the spacial averages shown in Fig.6. If this was indeed the case, the time series are representative of different places and therefore not fully comparable. I would like to ask the authors to clarify and if need be, to improve on this point to corroborate the main message of the paper.

Second, I see the explanation for the different dynamics in SIF and greenness as incomplete. The authors convincingly argue that the different phasing of activity between evergreen forests and grasses, chaparral, and oak savanna causes the double peak in SIF. However, a similar decomposition by land cover type (Fig.6a) is missing for the greenness indices and I strongly suggest to include this in the analysis (at least for one of the indices) in order to get an idea of where/ in which ecosystems SIF and greenness are particularly dissimilar (Fig.6c). Otherwise, a sentence like in the abstract that SIF 'can detect the downregulation of photosynthesis even when plants appear green' is not justified from the material presented in the paper. Finally, this analysis of where





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and when VIs and SIF disagree, could be completed by a driver analysis to understand which processes does SIF see that greenness does not and to undermine the argumentation in p.17 ll.15-28. There are features as those in May in both years, which coincide with similar dips in light and rain events, it is not clear which of these is more important for which ecosystem. There are other prominent features such as the smaller peak in September 2018 in SIF which does not seem to have an obvious relationship with either precipitation or light.

Overall, the paper is very interesting, well structured, very clearly written with detailed explanations and very appealing figures. It can be streamlined though. The scaling from SIF to GPP based on the tower measurements is not necessary for this manuscript and a distracting side information, especially given the weaknesses of the scaling that the authors discuss. I suggest to remove this and list further minor comments below.

Minor comments:

- The higher correlation between greenness and SIF at longer time scales is mentioned both in the abstract and conclusions but in the main text only in a sub-clause, and is not a main finding of your work. I see this as distracting side information as well, which does not necessarily need to be mentioned in both the abstract and the conclusions.

-Abstract: 'The different seasonality in the vegetation indices may be due to a clear-sky bias in the vegetation indices, whereas SIF has a low sensitivity to clouds and can detect the downregulation of photosynthesis even when plants appear green.' This sentence illustrates my major comment from above that the question of what drives the SIF response in the different ecosystems is not sufficiently covered by the analysis.

-The fact that there is a double peak in SIF but not in the VIs is mentioned twice in the conclusions.

- Apart from the fact, that the scaling from SIF to GPP is not needed in this manuscript, it is rather uncommon to use the unit of mu mol/m2/s from the tower measurements also for seasonal values as in the maps in Fig.6. gC/m2/day is rather common.

- Fig.6G' does not exist, pay attention in caption and Fig6b.

-p.9 l.34: Can you really resolve daily features with an average over 14 days despite daily sampling?

-p.11 l.29 -p.12 l.2: To my (admittedly non-native English) ears the word 'owing' in this sentence sounds misplaced.

- Fig.6B: why is the cropland contribution stressed in this panel?

-p.19 l.26: 'it seems unlikely that the grasslands and forests will exhibit opposing responses to a forcing.' It probably depends and an extended analysis as suggested above can give indications of whether this is true for California or not. There are counter examples e.g. in Flach et al. 2018 https://doi.org/10.5194/bg-15-6067-2018 or Walther et al. 2019 https://doi.org/10.1029/2018GL080535

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