

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Trina Merrick et al.

Author comment on "Unveiling spatial and temporal heterogeneity of a tropical forest canopy using high-resolution NIRv, FCVI, and NIRvrad from UAS observations" by Trina Merrick et al., *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2021-95-AC1>, 2021

Dear Reviewers,

Thank you for giving us the opportunity to submit a revised draft of the manuscript "Unveiling spatial and temporal heterogeneity of a tropical forest canopy using high-resolution NIRv, FCVI, and NIRvrad from UAS observations" for publication in *Biogeosciences*. We appreciate the time and effort required to provide feedback on our manuscript and are grateful for the insightful comments that have led to valuable improvements to our paper. We have incorporated most of the suggestions made and highlighted those within the manuscript. Please see below, in for a point-by-point response to the reviewers' comments and concerns with line numbers noted.

BG Reviewer Comments:

RC1: 'Comment on bg-2021-95', Anonymous Referee #1, 04 May 2021

Summary of the research and my overall impression

Merrick and coauthors present a novel dataset of remotely sensed vegetation indices (VIs) (NDVI, EVI, NIRv, NIRvrad, FCVI) from an UAS in a tropical forest canopy in Panama. They explore both spatial and temporal variability between indices and highlight potential uses for these indices at those varying scales. Specifically, the authors explore temporal correlations between GPP and VIs over the course of a day, diurnal changes in the spatial variation between VIs, and dominant spatial scales for variability in VI signals.

The paper is generally well written and structured and provides exciting insights on how VIs relate to each other. Both the dataset and the comparison are novel and within the scope of BG. Additionally, such direct comparisons between VIs are highly valuable because they provide insight in a field saturated with different VIs as to which VIs are most applicable for certain questions and specific strengths and limitations of each. The data collection approach is largely appropriate for the study, however, the temporal resolution of measurements is a major limitation. Additionally, the authors make claims about their findings in relation to SIF measurements that are not sufficiently substantiated. These major concerns are outlined with more specifics below.

Overall, this an interesting study that will be of interest to the scientific community but needs some revisions to clarify what their findings are vs. what their findings imply.

Therefore, I recommend this paper be accepted with major revisions.

Major Concerns

- The methods section is quite dense and difficult to follow. This makes it challenging for the reader to connect measurement approach to the presented results. I recommend the authors present some sort of conceptual figure showing their measurement approach and processing. I think this will be highly beneficial, particularly for a study that explores spatial variability.

Thank you for this suggestion. We have included a methods and materials summary diagram in Section 2 as a new Figure 1 (Lines 512-521).

- Only one day of GPP data is available. This has led to two specific issues:
 - I am concerned about the validity of a single day's worth of GPP data. I feel as though the statistics used to partition GPP from NEE may be insufficient with only one day available. It's worth some discussion about the limitations of this approach at a minimum. I believe Matteo uses more data to estimate GPP but we only present one day. Ask him to explain and fill in this response?
 - Thank you for pointing out the ambiguity in our description of the GPP from eddy covariance. The GPP estimates were derived from eddy covariance system data continuing several months, from which we extracted the one day of data. Unfortunately, due to a power issue, these data were not available for the first day corresponding with the hyperspectral and lidar data collection.
- In section 3.1, the authors explore the diurnal trend in VI, PAR, and GPP data. They use this trend to draw conclusions over the utility of NIRvrad as a proxy for GPP. However, I do not believe one day of data is sufficient to draw such strong conclusions. Additionally, there is insufficient discussion over how potential physical (illumination, viewing direction, etc.) or environmental effects (drought, seasonality, etc.) may impact these conclusions and the limitations posed by one day of data. Finally, Figure 1 appears to show a higher correlation between GPP and PAR than between GPP and NIRvrad – therefore significantly undercutting the authors main claim in this section – that NIRvrad is an appropriate proxy for GPP over short temporal scales. To me, this section would be better off as a discussion of how NIRvrad in fact does **not** sufficiently capture diurnal variability in GPP – and moreso reflects changes in PAR. I also recommend the authors provide a bit of additional commentary on why the other VIs show low correlations with GPP data.
 - Thank you for the suggestion for added clarity and purpose. We address the limitations of using one day of data throughout the manuscript, specifically in lines 488; 490-492, 534-538, 704-706, 735-737. Throughout the manuscript we have made changes to carefully state that there is greater potential for NIRvrad as a proxy for GPP compared to the reflectance-based vegetation indicators (indices). The reflectance-based indicators, NDVI, EVI, NIRv, and FCVI, have been shown to trend seasonally with GPP in most biomes, but by virtue of calculating reflectances, these omit short timescale changes in incoming, scattered, and reflected radiation. NIRvrad, in contrast to reflectance-based indicators, includes the incoming, scattered, and reflected radiation in the NIR region. For this reason, recent studies (e.g. Wu et al 2020) and our study are pointing to the potential of NIRvrad to trend with GPP on short timescales through a joint relationship between NIRvrad, PAR and GPP. We have added more text in the introduction, results, discussion and conclusion, to address of this for clarity, lines 192-195, 506-509, 532-540, 866-869, and 954-955.

- The authors repeatedly draw the conclusion that presented VI data is suitable for separating out the physiological from the structural component of the SIF signal when SIF measurements are available. However, the authors are not presenting SIF data and therefore not substantiating this claim with sufficient results or appropriate citations. Specific comments are included in specific examples. I feel that much of the SIF discussion in fact takes away from the authors main conclusions and novelty of their other results as it focuses the discussion on what they aren't doing (normalizing SIF with VI data). In particular, the majority of the introduction focuses on SIF. I recommend the authors cut down on this discussion significantly and make it more clear what conclusions they are drawing from their results vs. potential directions for future work.
- The authors appreciate these suggestions regarding the overemphasis on SIF in the discussion and introduction. Based on this thoughtful review, we have modified the manuscript extensively to focus the introduction on the quantities measured in the study and minimize the text and references to SIF and how the quantities may relate to SIF. Specifically, we removed almost all of Lines 52-101 from the original submission. We maintained mentioning SIF in the Introduction only as the studies presented compared NIRv, FCVI, or NIRvrad specifically to GPP and SIF (Lines 193-201), and in the Results and Discussion (Lines 729-731) and Conclusion (Lines 865-869) to make comparisons between measurement techniques for reflectance-based indices and SIF as well as emphasizing how this study might be relevant to SIF, which is an emerging, important potential measurement of GPP.

Specifics:

- Lines 16-18: The statement 'presented here for the first time' is a bit misleading since you are not presenting these VI's for the first time, you're presenting them at this specific field site for the first time. Additionally, this opening does not make it clear the scientific question or problem you are trying to address or appropriate background information.
- Thank you for assisting with clearer wording for this part of the abstract. We have removed the phrase "presented here for the first time" and modified the text (lines 16-23, 593-596) to clarify the purpose of the study. We see that the previous phrasing suggested the vegetation indicators were presented for the first time, when we only intended to point out these indicators specifically from UAV data are novel.
- Line 38: Unoccupied might be a more appropriate term, as presumably the UAS was piloted (just not with someone on board).
 - We have updated to use the term 'unoccupied', as it is more appropriate (Lines 24 and 59).
- Line 57: 'SIF is mechanistically linked to photosynthesis of plants, and thereby, has also been shown to be more sensitive to changes in forest canopy function and structure than RIs' – this deserves a citation. I also don't think you can say it's more sensitive to changes in forest canopy structure(although function yes). See the following for comparisons between SIF and VI's (among others):
 - Cheng, R., Magney, T. S., Dutta, D., Bowling, D. R., Logan, B. A., Burns, S. P., Blanken, P. D., Grossmann, K., Lopez, S., Richardson, A. D., Stutz, J., & Frankenberg, C. (2020). Decomposing reflectance spectra to track gross primary production in a subalpine evergreen forest. *Biogeosciences*, 17(18), 4523–4544. <https://doi.org/10.5194/bg-17-4523-2020>
 - Magney, T. S., Bowling, D. R., Logan, B. A., Grossmann, K., Stutz, J., Blanken, P. D., Burns, S. P., Cheng, R., Garcia, M. A., Köşler, P., Lopez, S., Parazoo, N. C.,

Raczka, B., Schimel, D., & Frankenberg, C. (2019). Mechanistic evidence for tracking the seasonality of photosynthesis with solar-induced fluorescence. *Proceedings of the National Academy of Sciences of the United States of America*, 116(24), 11640–11645. <https://doi.org/10.1073/pnas.1900278116>

- Pierrat, Z., Nehemy, M. F., Roy, A., Magney, T., Parazoo, C., Laroque, C., Pappas, C., Sonnentag, O., Bowling, D. R., Seibt, U., Ramirez, A., Helgason, W., Barr, A., & Stutz, J. (2021). Tower-based remote sensing reveals mechanisms behind a two-phased spring transition in a mixed-species boreal forest. *Journal of Geophysical Research: Biogeosciences*. <https://doi.org/10.1029/2020JG006191>.
- Thank you for this comment. Based on this and the earlier suggestions, this portion of the manuscript was removed and portions of the manuscript referring to SIF significantly more focused on how the vegetation indicators measured related specifically to SIF. These references, however are valuable for our future work and are greatly appreciated.
- Line 87-89: It's worth mentioning which ecosystem types because this is not true across all ecosystems/some types show much better performance than others. The citations you have all have ecosystem type information.
 - We have updated this text to include the ecosystem or coverage of data, i.e., global, from the literature. This portion now appears in lines 82-84, but portions of the paragraph after these lines has also been updated to include more specifics (Line 85, Lines 88-92, Lines 94-98),
- Lines 99-101: Again it's worth mentioning ecosystem type here (ie: specifically tropical in your case) – this doesn't necessarily apply for all ecosystems/we don't have enough studies testing this across varied vegetation cover.
 - Thank you for pointing out this omission. We have now included text to clarify the data used in previous studies, which helps us highlight the tropical forest on which we focused (lines 94-98).
- Lines 111-113: This deserves a citation (or several).
 - Thank you for pointing out this ambiguous statement. We have removed references to using the emerging indices to potentially separate the SIF signal into physiological and physical components, as we did not test this. As a part of this process, this particular phrase was removed.
- Line 124: The introduction deserves some final statement about the broader aims of this work. What ultimate goal this information provides.
 - Thank you for this suggestion. We added a sentence at the beginning of the last paragraph of the introduction (Lines 99-104) to state the broader aims.
- Line 146: there's a period . typo after 12 ms.
 - Thank you, this error has been corrected (Line 129).
- Line 160: As mentioned above there should be additional discussion on the limitations of only one day of data.
 - Thank you for reminder here. We have addressed this in lines 488; 490-492, 534-538, 704-706, 735-737.

- Line 173: I believe the original citation for NDVI is:
 - Tucker, C. J. (1979). Red and photographic infrared linear combinations for monitoring vegetation. *Remote Sensing of Environment*, 8(2), 127–150. [https://doi.org/10.1016/0034-4257\(79\)90013-0](https://doi.org/10.1016/0034-4257(79)90013-0).
 - Thank you for pointing out this oversight, we have inserted this citation (Line 158).
- Figure 1: There appears to be some sort of accidental grid to the side of panel d?

Thank you for catching our oversight. The figure has been corrected (Now Fig. 2, Line 218).

- Lines 236-238: 'Our results demonstrate that UAS-based data are suitable for normalizing SIF at high spatial resolution in addition to recording structural heterogeneity of a tropical forest' – your results don't really demonstrate this because you don't have SIF data. Maybe if you say they have 'the potential' however I still think this distracts here from the other findings.
 - Thank you, we agree and we have removed this reference to normalizing SIF and focused this portion of the manuscript on NIRv, FCVI, and NIRvrad instead (Lines 229-234).
- Line 239: 'Because NIRv and NIRvrad use NDVI, these results also indicate that including NIR reflectance or NIR radiance is the largest contributing factor to this variability' – This is built into the definitions of NIRv and NIRvrad so I would rephrase this to reflect that.
 - Thank you. Lines 233-234 have been updated to clarify this point.
- Lines 250-251: rephrase for clarity to 'The low variability and high means at midday of NIRv, FCVI, and NIRvrad indicate that...' T
 - These lines, now Lines 247-250 have been revised to make this point more clearly. Thank you for suggesting a change in wording here.
- Line 266: 'strong peak' is a bit of an overstatement, it seems much more rounded to me.
 - Thank you, we have rephrased to "distinct" to avoid overstating the shape of the peak (Line 262).
- Line 277: remove 'note how'.
 - We have removed this part of the sentence (Line 272).
- Lines 286-297: This discussion of SIF is much better because it acknowledges the potential, but also notes that SIF measurements are not available. This however also deserves some citations.
 - Thank you. We have included the appropriate citations for this statement in the revised version (Lines 290-298).
- Line 313: Remove 'for the first time' – it's confusing as you're not presenting new indices, you're presenting new data at this particular location.

- Thank you, we removed this from that line (now Line 315), and created a new sentence (Lines 315-316) to clarify that we think we are the first to use such high spatial resolution data of NIRv, FCVI, and NIRvrad (from UAS). Based on this helpful suggestion, we think this more correctly asserts the claim.
- Line 317: I do not believe you can draw this conclusion with one day of data (see my major concern above).
 - We appreciate this suggestion and we re-worded this sentence (now Lines 317-318) to discuss the potential, as well as throughout the manuscript.
- Lines 334-337: SIF discussion here is distracting from your main points.
 - We see this now and agree. We have removed references to SIF and SIF disaggregation from the conclusions.
- Lines 345-346: You do not show that these measurements can be used to separate the components of a SIF signal and you're also not really showing how to use it as an estimate of fPAR, APAR, or GPP. Also worth noting this is for a tropical ecosystem.
 - We have also removed these and updated this portion of the manuscript to reflect this helpful advice. Instead, we discuss the importance of future work using these vegetation indicators in tropical ecosystems and beyond to explore vegetation structure and function (Lines 337-344).