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Interactive comment on "Combining hyperspectral remote sensing and eddy covariance data streams for estimation of vegetation functional traits" *by* Javier Pacheco-Labrador et al.

Anonymous Referee #1

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Review for Pacheco-Labrador, biogeosciences

The paper by Pacheco-Laborador et al. jointly uses airborne hyperspectral reflectance data and eddy covariance data to retrieve ecosystem traits in a Mediterranean tree-grass ecosystem. They use 17 hyperspectral images over three different flux towers (control, N addition, N+P addition) in an inversion framework which couples radiative transfer and soil-vegetation atmosphere transfer using a modified version of the SCOPE model to incorporate leaf senescence. The results suggest that such a framework can estimate vegetation traits and energy fluxes in this ecosystem. The authors also 'scale' their results using synthetic emulated hyperspectral satellite imagery to

place in the context of future hyperspectral missions.

The work described here is a significant effort, integrating many different datasets collected across a range of temporal and spatial scales over the course of 6 years. While this effort is very much appreciated, the many different data sources and complexity of the approach make it challenging to review. It requires a significant amount of background knowledge on the many papers previously published by the authors to completely understand the approach. Despite my best attempt at this, I still found this manuscript very difficult to evaluate. There are too many assumptions made for a complete evaluation of the paper's rigor, leaving the reader to have to place a lot of trust in the authors. If the assumptions are indeed valid (but again, too many to look into to fully address each one) then the paper comes across as a sound methodological approach. Despite these limitations, I think there is value in this work, but I would recommend the authors consider re-evaluating how to best distill this complex story into something more tangible and coherent.

To me (and I could be missing the point), the paper reads very much like a methodological paper, perhaps better fit for a journal like EGU's Geoscientific Model Development. The paper does not go far enough into describing the "interactions between the biological, chemical, and physical processes in terrestrial or extraterrestrial life with the geosphere, hydrosphere, and atmosphere" – as stated as a goal of Biogeosciences. There is very little information regarding what the authors have learned about this ecosystem; the main result is that a seemingly complex approach can produce key functional parameters of vegetation that are robust to several sources of uncertainty. The discussion of sources of uncertainty, in particular, is extremely robust and very much appreciated.

Based on the strengths of this paper, I would suggest a path forward might be to remove the analysis in Figs. 5 and 7. To me, these figures raise more questions than answers. The attempt by the authors to say something more ecological about how vegetation traits co-vary takes away from the paper. Focusing on the key results, Fig. 3, 4, and 6 (Figs. 1 and 2 are also nice) seems like it would help to distill the information content. A reduction in the amount of parameters the authors are trying to predict might also help (moving the rest to the supplementary material). Focusing on a few key vegetation parameters – as opposed to trying to model everything, all at once – followed by a concrete discussion on where and why model-data mismatch or over/underprediction happens might also be a path forward. Currently - while there is a lot of good content in the discussion - it should relate explicitly back to the key results and answer bigger questions about how such analytical techniques could be used to map vegetation traits going forward. I realize this a fairly vague suggestion, but a substantial reframing of the story will also help this paper reach a broader audience.

Minor comments are as follows:

Abstract (and elsewhere): The authors use the word "prove" to describe their findings. This language is too strong, consider "suggest."

The first sentence of the introduction, I would perhaps not mention just climate change as an application for this work, as climate change is never again discussed and by using it as the only potential application, it implies that this might feature into the work more prominently.

The introduction is well written, the authors touch on pretty much every aspect of the paper. If one had time to read all of these papers from a wide range of disciplines, it would certainly make the methods and results easier to interpret. In order to reach a broader audience, I'd suggest a little more 'hand-holding' though, particularly with regard to what exactly some of the plant functional traits are and why they are important.

One main point made clear in the introduction is that an attempt to jointly retrieve functional traits using hyperspectral imagery combined with EC data is lacking. But it's not clear why we need this? How are the other methods failing that require this new approach?

Line 111: The authors note that only one of the examples from the previous

paragraph validates retrievals against actual measurements from gas-exchange measurements. . .but this paper doesn't do that. They make assumptions about other traits or use data from existing literature in combinations that is difficult to follow.

Lines 124-130: The attempt to relate this work to future satellite missions is appreciated, but the amount of detail necessary to introduce readers to how the emulation works is lacking.

Line 143: Describe CT. . .I'm guessing Control Treatment

Line 157: 'mayor' to 'major'. . .there are quite a few grammatical mistakes throughout, I won't comment on them, but please address these. Given the large quantity of co-authors, one would think these could be addressed.

Table 1: This table is appreciated, but for the many other variables used during this entire study it would help to add them as additional columns.

Line 213: Why aren't data from these additional campaigns included?

Line 210-250: There are many assumptions made regarding the biophysical variables used. For example, deriving Vcmax from Nmass,green and a relationship from an existing paper. While there isn't much of an alternative, it should be noted that many of the biophysical parameters are very much inferred.

Line 269: To assume that carotenoid concentration will covary with Chl concentration (derived from a SPAD meter) is one example of gross oversimplification.

Line 299: 'close to solar noon'. Are the actual flight times used to compare to the EC data? Solar noon is much less relevant here as are the incident irradiance conditions. Diffuse/direct fraction, time of year, solar zenith angle. . .it's unclear how these are considered.

Figure 2: This is a useful figure, the axes need labels.

Figure 3: The x axes are displayed as a time series, at equally spaced intervals that

make this difficult to interpret. Consider removing the vertical lines and the x ticks, and simply note the date of acquisition horizontally in each shaded or non-shaded bin.

Figure 4: Many of these fits violate the assumptions of linear regression, in which case I don't think it's useful to include a line of best fit, or the statistics. Also the figure legend has subplots labeled wrong.

Figure 5 and Fig 7: I feel that these take away from the main message the authors are trying to communicate. How are these figures adding to the main results? Am I missing something?

Another confusing part of the analysis is that it appears as if the authors are predicting ecosystem traits, which are a combination of both grass and tree

Generally, there is a lot of good information in the discussion. However, much of it reads as 'intro' material and it does not relate directly back to the results. While a lot of the points regarding uncertainty are important, I do not feel as though the discussion drives home the main results, or how such an analysis could be used in the future. The authors have a deep understanding about many of the uncertainties associated with their approach, and that is much appreciated. It is of my (potentially naïve) opinion, that their discussion is not useful for informing future research that is conducted outside of their own particular research group. I would advise the authors to pay close attention to how this work is perceived by individuals outside of their niche team. After all, this will not only help the authors consider the broader importance of their work, but it will help the rest of the research community.

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