We thank the reviewer for their effort and constructive comments received. The key issues identified are similar to the first reviewer's comments and we are addressing them in the final submission of the article. Any additional figures to include, tables or references will be done in the main text too or appendices.

However, we answer to the comments and suggestions in this document.

Italic font type: Author's replies.

In this paper the authors use a Random Forest model to derive the Plant Fractional Cover by upscaling UAV multispectral data to Sentinel-2 MSI data. They present a method that allows to overcome the limitations of low-resolution images from satellite data by using UAV. The topic fits the purpose of Biogeosciences and the special issue. However, I believe the manuscript needs to go through several revisions as I have comments and questions to the authors regarding the methods, the results, and the overall presentation of the study.

TITLE: Here you use the Plant Community Cover term but in the paper the Plant Community Fraction is used. I suggest correcting that.

This comment suggests a different title and we agree with it. If the Editor accepts it too, we will changing it as "Plant Fraction Cover".

INTRODUCTION: the authors provided a nice introduction to the topic of the paper. They provide information about the advantages of using remote sensing to monitor wetlands, pros and cons of using UAV and Satellite-based imagery, and the potential in the upscaling of UAV images to satellite resolutions. However, the introduction does not clearly state what is the novelty of the study. From my understanding of the last paragraphs, the implementation of Machine Learning (ML) models and especially Random Forest (RF) models to infer Plant Fractional Cover (PFC) is not new. At the same time it is not new to couple these models with DEMs as ancillary data. The last sentences suggest that this is what the study aims to. I think the introduction need to point out what are the new aspects the authors are looking at. What does this paper add to the current state of the field? Was sentinel-2 never used? Is the scale of the study new?

Thank you for your comment.

This study provides the novelty of testing an upscaling methodology from fine-scale images taken with drones to a broader scale at Sentinel-2 image spatial resolution over Baltic coastal meadows to model the plant fraction cover of five plant communities. Several studies have *successfully monitor plant communities using high-resolution images with Unmanned Aerial Vehicles (UAVs) (i.e. Villoslada et al. 2020). However, to overcome the limitations of using UAVs, we studied the possibility of monitoring the plant communities with Sentinel-2 using UAV as reference. Up to this point, no one has been implementing this upscaling process in Baltic coastal meadows of Estonia.*

I also fell like the authors could reorganize a bit the structure of the introduction. At the beginning you mention remote sensing, then move to wetlands, and then back to remote sensing. I would first introduce the wetlands and then the remote sensing aspect. It makes the reading more fluid.

Thank you; we will clarify this part of the Introduction, rearranging its structure.

LINE 39: There is a Ward et al. (2016) and Ward et al. (2016a) cited but only one referenced in the bibliography. Is it a typo? Please correct or add the citation to the list.

Thank you, this has been reviewed and corrected.

LINE 71: Acronym VI was not introduced before.

Thank you, this has been reviewed and corrected.

LINE 75: What's the tidal range here? Do these areas go underwater regularly at each tidal cycle? During high tide/spring tide? Or only rarely during storms?

Thank you for this question. The tidal range varies mainly due to storms. The answer to this question is going to be added in the main text and we answer it in the following lines:

The tidal variation in coastal wetlands is approximately 2 centimeters characterized by irregular inundations (floods) dependent on fluctuations of meteorological conditions across the North Atlantic and Fennoscandia (Kont et al., 2003 and Ward et al., 2016).

In addition, a table with a description of plant communities (as suggested in this document) will explain the different inundation regimes affecting them. The areas can be affected by floodings, affecting the plant community distribution.

LINE 76: Here you use Figure 1 to reference to the figure. Many times throughout the paper you use the short form Fig. to reference to figures. I suggest picking one style and being consistent.

Thank you, this format error has been corrected.

LINE 82: I have to say that I am not familiar with the plant species here considered. I think it would be important to provide more information on these species/communities. For instance, what is their phenology? This would be a very important information. If they have a growing season in a specific time window, it means there is a limited period of the year of useful remote sensing images.

We have added a table providing the information requested as found in references.

FIGURE 1: This is just a personal preference a very subjective suggestion: I would increase the font size, especially for the latitude and longitude coordinates. I would also identify the study sites not with numbers but with their names and acronyms directly in the map instead of giving a legend in the caption. It would be much easier to locate them.

Thank you for this suggestion. We have increased font sizes in all the figures. This improves the readability.

LINE 96: Would it be possible to provide the confusion matrix for the classification. It is the best way for the reader to quickly assess the quality of the classification. You could simply put it in the Appendix.

We have added a new figure in the appendix , which includes the confusion matrices per study area corresponding to the number of pixels in the test fraction classified per each plant community. These derive from the study of Prentice et al., 2021.

Results from the Random Forest pixel classification. MAT (Matsalu), KUD (Kudani), TAS (Tahu South), RAL (Rälby), TAN (Tahu North) and Rumpo (RMP). Kappa values are MAT: 0.98, KUD: 0.92, TAS: 0.93, RAL: 0.89, TAN: 0.99 and RMP: 0.99. Each class of Predicted and Actual Plant Communities are LS (Lower Shore), OP (Open Pioneer), US (Upper Shore), TG (Tall Grassland) and RS (Reed Swamp).

LINE 101: I understand that the classification is based on a different study. However, since it is key to the analysis, the authors should provide a few more details on the classifications (see comment above).

Thank you. We have added the confusion matrices (shown in the previous comment) and also the dGPS used to measure the ground truth height necessary to validate the Digital Elevation Models. Hyperparameter tuning of Random Forest will be also provided. Table A1 shows the presence of plant communities surveyed in each study area.

LINE 107: The analysis used only one Sentinel-2 scene. Do the authors think that the study would benefit from using multiple scenes? Since the authors say that the method has implications on monitoring (i.e. using multiple images), I think that testing the model on a different scene would prove that the method is more robust. Having said that, I am not suggesting that the acceptance of the paper should depend on this additional analysis as I believe it is a serious task to undertake.

This is a very interesting suggestion. Indeed, we consider contributing to the study of Baltic Coastal meadows with a time series of Sentinel-2 in the visible – near infrared spectrum. The present study is a first approach to estimate the PFC using Sentinel-2 images having the centimeter-resolution UAV images as a reference. Further work will be focused on monitoring Baltic Coastal meadows in a broader period.

Was the Sentinel scene taken during low tide? I think the presence of water can affect the reflectance. The authors should specify (maybe in the discussion) what kind of images are good for this method. In the discussion there is only the mention to take UAV images close to the satellite passage. The authors can expand here on what are other important aspects that goes into the choice of the scenes.

Thank you for this suggestion. The presence of water affects the spectral response of plant communities but not during the dates chosen. However, the drone flights were carried out during high phenological activity and when the weather conditions were optimal for those flight plans. Including the Red-Edge band enhances the plant community detection with images as this band is more sensitive to chlorophyll reflectance. This is why we chose Sentinel-2, too.

We consider this comment for the discussion and further work related to the previous comment: Studying the affection of presence of water to plant community detection from a time-series approach.

TABLE 1: I think Table 1 is missing some information. I see only the study area and the drone flight dates, but no tile number or satellite overpass date is reported. Either reformulate the caption or add the information in the table.

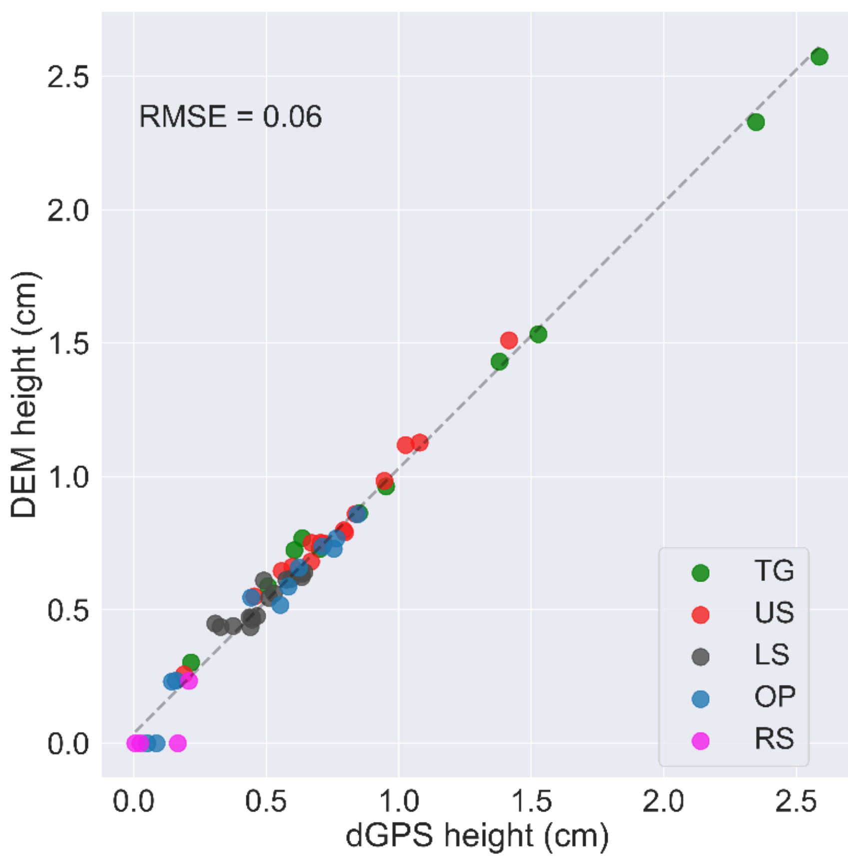
Thank you, the table caption is corrected. This table includes the dates of drone flights. The main text mentions the tile number and date of the Sentinel-2 image acquisition because it was the closest to the drone flights.

TABLE 2: units of wavelength and spatial resolution are missing.

Thank you, these have been added in units of nanometers (nm)

LINE 122: could the authors provide the vertical error of the DEM? Since the microtopography is important, it would be useful to know what's the vertical error.

This has been added as a new figure in the main text. Please see below the figure:



The dGPS height is the height measured with Sokkia GSR2700 ISX differential global positioning system (dGPS). The total RMSE is 0.06 cm.

FIGURE 2: Like for Figure 1, I suggest to directly use site acronyms in the map. I also suggest increasing the size of the scale bar. It's very difficult to read it.

Thank you, the scale bars have been modified for a better readability. We have replaced the numbers with the acronyms of the study areas.

LINE 137: 'A correlation and a linear function were used' is not very precise. From this I understand that there are two levels of evaluation. What kind of correlation? Is the correlation found with a linear function? Please rephrase for more clarity.

Thank you, we will rephrase this statement in the main test, as follows:

The comparability and consistency of the spectral data from PS and MSI bands was analyzed by fitting the values in a linear model, calculating the coefficient of determination (R^2) and Root mean squared error (RMSE). The p-value showing the significance of the relation between PS and MSI.

LINE 138: Does the averaging of the elevation smooth the microtopography? Do you think this step has an effect on the performance of the model?

Thank you for this question. Different aggregation methods produce different results on the distribution of values within the spatial unit of a pixel considered here, affecting the spatial characteristics too.
We will include an explanation in the main text. As follows:
We considered that aggregating with the average value in a spatial unit of the pixel produces more predictable behavior (Bian and Butter, 1999).
The purpose of this study was not the comparison of different aggregation methods but we agree that using different aggregation might affect the performance of the predictions.

LINE 140: I am a bit confused on the separation of DF1 and DF2. DF0 is a sub-sampled dataframe from DI, and it already contains the elevation variable since the DEM information was previously added. Thus, I understand that DF2=DF0, and it is not really a new dataframe. Would it be easier to consider just two dataframe? One with and one without elevation?

After undersampling the initial dataframe (DI), we obtained DF0. We decided to follow a sequential order, so the following step was to split this dataframe into two new and different ones (DF1 and DF2). DF1 is used to predict without the variable of Digital Elevation Model and DF2 is used to predict with that variable.

LINE 155: I think the reference to Figure 1 is wrong. Please check.

Thank you, this has been corrected.

LINE 169: I assume that in each MSI pixel we want to have the sum of all PFCs equals to 1. If you have a separate model for each plant community, how do you make sure of that? Do you force it somehow? Please clarify this as I think is an important step.

Thank you for this comment. We can clarify this in the main text.

The initial dataframe (DI) contains all the individual PFC in different columns. In the figure caption of figure 3 of the preprint version , "yi" (where " i " can be LS, OP, US, TG or RS) is the PFC of each plant community and their sum is 1 (100% cover). Then, each "y_i*" is used for one single model. For example, in the Random Forest model for LS, we train with the PFC of LS and then test it.*

LINE 173-174: I am not sure the reference to Figure 4 is correct: it looks more correct to reference these sentences to Figure 5.

Thank you, we have corrected this.

LINE 174: rewrite as 'predefined hyperparameters'.

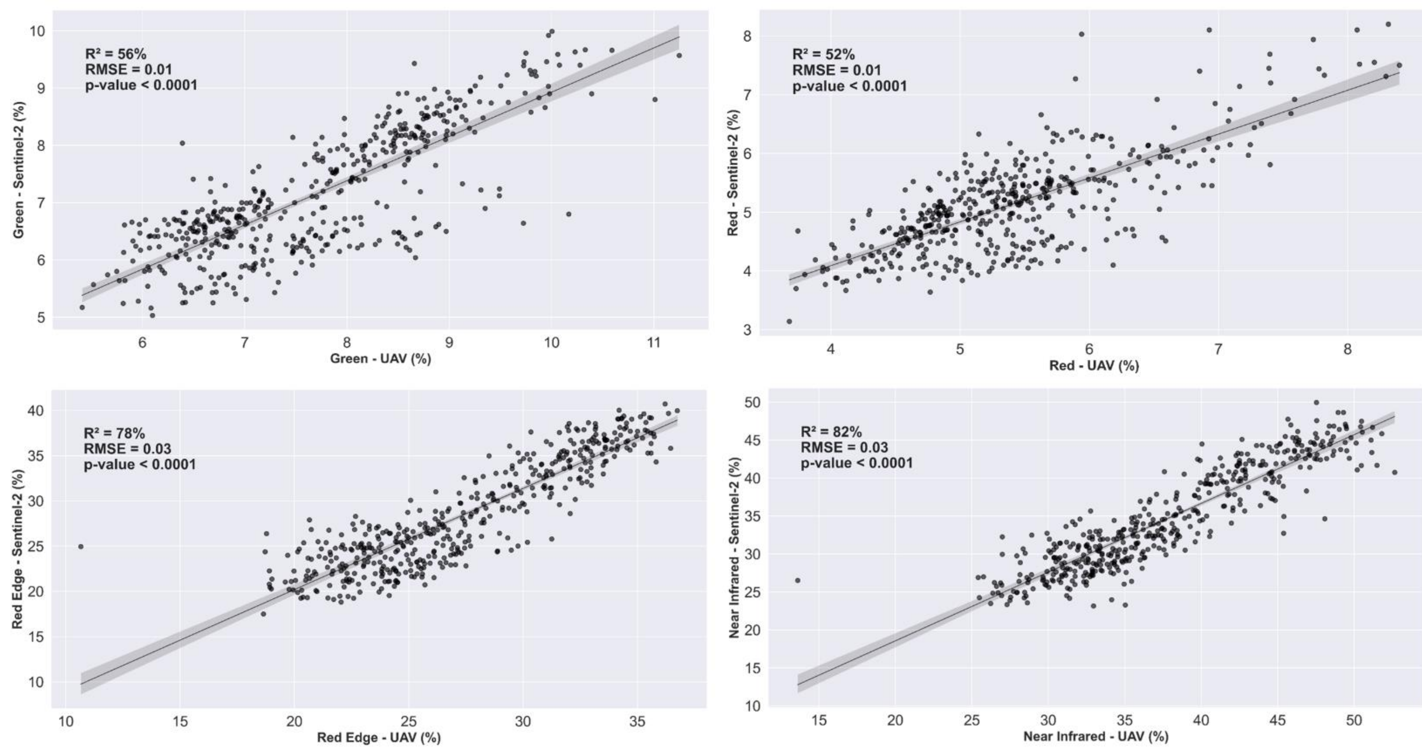
Thank you, we have corrected this.

RESULTS: Here you clearly show that elevation is a key feature to predict vegetation zonation. That makes 100% sense. I think it would be important to show the DEM to the reader due to the importance of this parameter. That would help to understand whether a species prefers a high or low area, which is directly linked to the ability to withstand a low or high hydroperiod.

This will be included in the table suggested before to describe the plant communities under study. This table will summarize this comment as well. In addition, an average height range will be provided.

LINE 185: it would be good to add a figure were you show the correlations between the MSI and PS reflectance in the MSI GRID. Maybe this figure can be shown in the Appendix/Supplementary Material.

We have added this figure in the appendix section, for each spectral band. Please, see below:



This figure clarifies the R^2, RMSE and p-value obtained from the linear fitting between bands. X and y axes are in reflectance units (%) as well as RMSE. Correlations in all the cases are significant,

We apologise because the previous numbers in the preprint version were wrong. As seen in this figure, R^2 and RMSE are different as in the preprint version. Therefore, we will update it and review the Discussion.

LINE 194: I invite the authors to consider moving Figures A1 to A4 to the main text, since they show the goodness of the models. Maybe Figure 2 can be moved to the Appendix to avoid overloading the main text with figures. It just shows the grid, so it is not as useful as the other 4 figures.

Thank you, we have moved the Figure 2 to the appendix, as the comment suggests, it is not as informative as the Figures A1 to A4.

TABLE 5: It would be better to specify the p-value instead of simply indicate <0.05.

The p-values are very low, under 0.0001, then, we will specify this number instead of 0.05.

LINE 216: I would be more specific. Are your results comparable to those studies you mention? I think it would be better to expand here the discussion and show a comparison with other similar studies. It would be very informative to know what you did better. Did these studies consider DEM or only VIs?

Thank you for this comment. The studies mentioned in this line also have found positive correlation between bands for an upscaling methods, although they did not model plant community distribution. Padró et al. 2018 used an exhaustive comparison between sensors, two of them were MicaSesnse and Sentinel-2 MSI, finding good correlations. Díaz-Delgado et al. 2019 used the same camera than in this study, Parrot Sequoia, on board of a fixed-wing drone and compared its spectral bands with Sentinel-2 MSI. Both studies are done in the Iberian Peninsula, whereas our study is done in the Baltic region, agreeing with a good correspondence between a multispectral camera on board of a UAV and the MSI of Sentinel-2.

LINES 210-224: I think this part is not really useful. Here you are just repeating the methods and giving more results. You can move lines 222-224 in the results sections.

Thank you for this comment. We found good correspondences between spectral bands of Parrot Sequoia and MultiSpectral Instrument (MSI in Sentinel-2), similarly to other studies mentioned. We consider that this is an important result to discuss in spite of having different dates between the UAV flights and the Sentinel-2 overpass. But we might considered the rest of the lines you mention to be included in the Results section.

LINE 235: ‘figures 6 and A1’ should be ‘Figures’.

Thank you for this comment. We have corrected this.

LINE 236: Typo metre. Please correct. Also when you use the number use the unit. So in this case it should be ‘1 m’. Please make sure to follow that throughout the paper.

Thank you for this comment. We have corrected this.

LINE 237: Are you sure that the reference to Figure A2 is correct? I think the correct reference is Figure A3.

Thank you for this comment. We have corrected this.

LINE 238: Looking very quickly at Figure A4, elevation seems to be the most important by far with the only exception for the OP class. If I read correctly for the other classes, elevation in terms of importance it is around 0.5, and 3-6 times more important than the VI with highest importance. One could argue that acceptable results could be achieved with only elevation. Have you tried to do that? Can you comment on this?

Thank you for these questions. We used the Digital Elevation Model as the only independent variable in Random Forest Regressor model to predict the Plant Fractional Cover but its results had higher RMSE than the rest of the models with a very low variance explained. We also trained using different hyperparameters but without good results. This is because the Vegetation Indices calculated from the spectral bands are also indicators of the presence of vegetation, their phenological activity and their density because bare soil specially modifies the reflectance in the Red Edge and Near Infrared bands of the spectrum. We include this comment in the discussion.

LINE 239: This comment concerns the entire manuscript. I had hard time to remember all acronyms. I know that they make the writing faster, however I think it would be better to reduce the number. Maybe the authors can simply pick the most used ones.

Thank you, as another suggestion, we are including a table of abbreviations in the Introduction section.

LINE 242: I think the authors could expand here. Why these communities are so dependent on elevation?

The elevation (microtopography) determines the salinity, flooding periods, nutrient fluxes and topsoil moisture. This is explained in detail in Ward et al., 2016. Moreover, we will include this clarification in the main text.

LINE 245 to 248: The sentence starting with 'Overall, …' is badly written. I suggest reformulating and breaking it into short sentences.

Thank you, this is corrected.

LINE 251: Do the authors think that other satellite data would have been applicable to the study (e.g. Landsat)?

Not for this study. We used Sentinel-2 for some reasons: the Red-Edge band (band 6 of MSI sensor), its high spatial resolution (10 meters and, as mentioned in the methodology section, downscaling the Red Edge band to 10 meters using the Superresolution Algorithm in SNAP software), the high temporal resolution (5 days reached by the Sentinel-2 constellation, thus, more chances to get an image closer to the flight dates with low cloud coverage) and its public and instant availability. On the other hand, Landsat does not have Red-Edge band in its sensors.. In addition, the spatial resolution is higher.

As mentioned in a previous comment, a multi-sensor approach, the fusion Sentinel-2 and Sentinel-1 might improve the PFC prediction models.

LINE 257: Can the authors suggest other ancillary data for future research besides DEM? Maybe inundation time?

Yes, and this will be added to the main text. The use of a multi-source sensing approach has been done for these wetlands, where the use of Synthetic Aperture Radar (SAR) together with optical images from UAV reveal inundation patterns by sporadic seasonal storm surges. Thus, the SAR sensor is an optimal candidate but for a fusion with Sentinel-2 images. On the other hand, we can suggest the use of aspect, grazing management history and distance from the coast as ancillary data because, in spite of the low tidal ranges, the study areas do not have similar effects due to open water.

APPENDIX: The axis ticks are small and hard to read. Especially Figures A3 and A4. In Figures A1 and A2 you are not showing the predicted error. You are just comparing predicted PFC with measured PFC. When you are comparing modelled and observed values you do not need to compute the R^2 since you are not really looking for a model between the two. The RMSE values is a good index to evaluate the goodness of your predictions. Maybe you could add a second index like Model Efficiency or Percentage Bias (it doesn't necessarily have to be these ones). Especially with the second one you could quantify the general tendency of your model to underestimate, and overestimate observed values.

Thank you for this comment. We keep the metrics and will include the Mean Bias Error in these figures as well as in the methodology.

What is the unit/variable in y axis in Figure A3 and A4? Is it the explained variance for each variable? Please clarify.

Thank you for this question. We will clarify this in the text, adding "Variable Importance", clarifying in the figure caption that this variable importance ranges from 0 to 1, indicating the contribution of each single feature (variable) to each of the tree's total impurity reduction. In Random Forest, the importance is calculated as the average of importance over all trees.