

Interactive comment on “Combining multi-spectral proximal sensors and digital cameras for monitoring grazed tropical pastures” by R. N. Handcock et al.

Anonymous Referee #3

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The manuscript shows an interesting study on the use of in situ continuous multi-spectral sensors to monitor pasture biomass in livestock production systems as an alternative to traditional time-consuming methods based on destructive sampling and visual assessments. This proximal sensing approach would also provide alternative and/or complementary information to remote sensing data whose main limitation is its temporal resolution, especially in tropical areas affected by frequent cloud cover. In this context, the research questions addressed are relevant, especially for their potential effectiveness on decision making, and clearly fall within the scope of Biogeosciences. However, I have some comments/concerns about the approach taken in the manuscript. Authors have emphasized (both in the paper and in the manuscript discus-

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sion) that this is a pilot study, this means that it should be considered as a preliminary analysis before committing to a full-blown study or experiment. I agree with the authors that these pilot studies are an essential tool in any research study as a preliminary step in understanding processes, detecting weaknesses and even proposing new hypotheses. However, the question I want to rise here is, should this pilot studies be published even if we recognize a number of limitations that can be seriously affecting the results?. In my opinion this pilot study has significant limitations (some of them recognized by the authors) regarding:

1. Quantity and quality of the measurements: Spatial representativeness and significance of the statistical analysis
2. Statistical issues not entirely addressed as is the case of collinearity analysis and independent validation.
3. Indirect estimations of ground truth where potential errors have not been evaluated.

I found very interesting (and useful for potential readers) the discussion on the technical limitations of the system and the recommendations and “lessons learnt” that can be derived. However, I am quite skeptical about both the methods and results described in the paper regarding the relationship between sensor data and field estimates. Having said this, I would recommend refocusing the manuscript in order to put more emphasis in those issues related with sensor deployment, technical problems, data filtering, spatial and temporal trends found, etc. and reduce, or even remove, the model development unless more robust relationships can be established. Specific comments addressing particular scientific/technical/formal issues follow:

Abstract

The last part of the abstract, where the information on the model results is provided, is not easy to understand if you haven't read the whole paper. I would recommend to rewrite this part to simplify the information and provide only the main results without

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explicit references to dates and variables that have not been previously explained and, therefore, are difficult to contextualize.

Introduction

In this section I miss references to specific studies using proximal-remote sensing to monitor grassland ecosystems and more specifically those related with on-farm decision-making analyzing the state of the art regarding methods and instruments so the reader can easily get the main scientific contributions of the paper.

In page 18009 lines 15-16. I recommend refining this phrase as, in some cases, the operational availability of satellite derived products for decision making is not "so expensive". That basically depends on the application including the combination of spatial and temporal requirements.

Methods

In page 18012 lines 15-16. It would be very helpful to add here information on the min and max FOV obtained (according to vegetation height changes).

Same page lines 19-24. Authors should explain here why they took the decision of having one fenced and one unfenced node (implications in methods and results). This is not clear neither in the methods or the results/discussion sections

In page 1803 section 2.2. Please check the use of the acronyms. I suppose here the correct acronym should be always VWC and not VMC?

Same page line 25. Please, indicate the correlation found

In page 18014 lines 13-14. Same FOV? Do you mean that the two instruments where somehow aligned? Same size and area observed?

Same page line 18. Please, explain why automatic balance was chosen while most authors and networks (as is the case for Phenocam <http://phenocam.sr.unh.edu/webcam/>) recommend manual/fixed white balance in

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order to minimize in-camera processing of day to day changes in illumination.

In page 18015 line 8. Please, explain why the image taken nearest to 12:00 was selected. In general all these decision should be better justified.

Same page line 13. Manually here means visually?

Same page lines 16-17. Is this crosscheck analysis somehow included in the results?

Same page line 20. Was the FOV verified/characterized by the authors before sensor deployment. If not, it would be convenient to explicit that FOV is 25 degrees "as indicated by the manufacturer" considering that published works have demonstrated that some sensor FOVs greatly differ from manufacturers specifications. See the work of MacArthur, A.; MacLellan, C.J.; Malthus, T., "The Fields of View and Directional Response Functions of Two Field Spectroradiometers," in *Geoscience and Remote Sensing, IEEE Transactions on*, vol.50, no.10, pp.3892-3907, Oct. 2012 In general, all information about instrument calibration/characterization before its deployment in the field is quite useful and should be explained.

In page 18016 lines 5-8. I recommend refining this phrase as the SWIR bands sensitive to vegetation water content are not the ones selected in this study.

Same page lines 18-19. It is not clear to what complexity the authors refer to.

In page 1817 lines 9-10. Incomplete sentence?

Same page lines 14-17. Please review and rephrase.

Same page lines 23-28 and next page lines 1-14. I would suggest to extend this part with additional explanations on the decisions taken. For example, in table 2 it is not clear to me why there are two different criteria based on NDVI: $NDVI < 0.1$ and $NDVI < 0$. Also, what authors consider a "brief" drop to zero or why the specific temporal period of the data source for filtering during daytime (12 to 13) was selected and why is it different from the time period of data used in the analysis (10 to 14).

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In page 18018 lines 18-19. I agree that destructive sampling is not recommendable within the area under sensor's observation but, what about sampling in a nearby area? In the case of the fenced area could be even within the fence but out of the FOV. This should be considered an option unless authors assume that their measurements are so specific that can not be supposed representative of a larger area (which indeed would be not a good assumption).

Same page line 23. Please, describe how these "standard photographs" were taken

Same page line 24. Authors state in this paragraph that total biomass was not divided in the field between green and dry components because of the technical difficulties, however, this is a key issue in this type of analysis (as it was further observed in the results). Trying to estimate total biomass using either proximal or remote sensing is a tricky question due to the contribution of the dry fraction to the signal. However, in this study, I assume that the estimation of the two fractions is a key subject from the operational perspective, so deeper elaboration on this issue is expected.

Same page lines 27-20 and page 18019 lines 1-12. Did the authors validate somehow this visual assessment? Visual estimates are commonly conditioned by the subjectivity of the interpreter. Can the estimations be considered temporally consistent (same interpreter)? Did you try to calibrate potential uncertainty in the estimations by comparing with direct measurements (destructive sampling)? This can be done in a nearby area (not the one observed by the sensors) and can be used to calibrate the indirect visual estimates.

In page 18019 line 10. Check repetition?

Same page line 18. Which area? 1x1 m? please specify

In page 18020 line 3. Use the acronym (VWC)

Same page line 4. Yearly Julian date?

Same page lines 16-17. Please, specify the number of matching time points

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Same page line 20. In fact there were not biophysical measurements at such but indirect estimations by visual analysis. Authors should clarify this statement

Same page lines 23-24. In my opinion this is one weak point in the statistical analysis. Collinearity analysis should be done as this could affect not only to the indices but also to other input data which has been combined in the regression as is the case of weather data, soil moisture and spectral indices. In fact, authors recognize that "soil moisture sensors. . . showed a strong correlation with the average of precipitation recorded at the CTA and TA stations" (page 18013). Also, additional information should be included on the statistical method applied, especially on statistical indicators for model evaluation that are later analyzed on the results section (tables 3 to 5).

Results

In page 18021 lines 4-6. This is also a key point in the statistical analysis. Authors state that the standard split-sample validation was not performed due to the insufficient data. However alternative techniques as bootstrapping could be applied to analyze generalizability and reproducibility of the models. See Richter, K; Atzberger, C; Hank, TB; Mauser, W(2012): Derivation of biophysical variables from Earth observation data: validation and statistical measures. J APPL REMOTE SENS. 2012; 6

In page 18022 lines 4-8. But this is not related with the distance between the nodes, is a consequence of the differences in grazing activity (fenced vs not fenced), isn't it? This should be clarified in the text.

Same page line 16. Lost periods should be expressed in relative terms as in section 3.1

Same page line 18. It would be very useful to include in the images the approximate area observed by the Skye sensor FOV.

In page 18023 lines 10-11. This residual standard error (RSE) is equivalent to the most commonly used RMSE? I would recommend using a relative measurement such as the

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NRMSE or RRSME (see Ritcher et al 2012) which is more easily comparable.

Same page lines 25-26. But, is this really useful for farmers? In page 18009 authors mention that farmers need to assess, at the end of the wet season, how much green feed remains in the paddocks. Therefore I assume that the most useful information, from an operational perspective, would be to provide an accurate estimation of the green and dry fractions. These seem not to be possible if the analysis is restricted to the wet season, where green fraction dominates.

In page 18025 line 18. Please, add references to backup this statement

Same page lines 21-24. Please, rephrase

Same page line 25. Please, add discussion here on what authors expect a non-pilot study could add apart from having in situ weather data.

In page 18026 lines 24-26. Please avoid repetition "available"

In page 18027 lines 3-4. Why this type of filtering was not implemented in this pilot study?

Same page lines 13-14. How authors propose model validation? Spatial/temporal strategy? It would be interesting to include further discussion on that key question.

In page 18028 line 13. It would be interesting to know what was the problem related with sensor failure.

In page 18029 line 6. This is an interesting issue that could be also explored and demonstrated with the dataset provided by this pilot study.

Same page line 27. Please explain what authors consider "sufficiently sampled". The definition of the spatial and temporal sampling strategies is a key point in this type of approaches so it is important to discuss here.

Conclusions

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In page 18031 line25. Please rephrase to avoid repetition "limiting data acquisition"

Figures

In figures 2 and 3. Remove NDVI range in the title of y axis (not necessary).

In figure 3. Remove horizontal axis for NDVI = 0. The x axis should start the first day with spectral data available and finish also with the last day of data available (not before and after).

In figure 4. The image on the left looks blurry. A better quality image would be recommendable

Interactive comment on Biogeosciences Discuss., 12, 18007, 2015.

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