**Biogeosciences
Discussions**

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Interactive comment on "Evaluation of satellite based indices for primary production estimates in a sparse savanna in the Sudan" *by* M. Sjöström et al.

M. Sjöström et al.

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First of all, we would like to thank the reviewers for their very helpful comments and suggestions for improving the paper. Your help and feedback is much appreciated.

Both of the reviewers mentioned the issue with the flux tower footprint and it seems appropriate to begin on this matter. After that, we will go through each of the reviewers comments separately and how these will be addressed in the revised manuscript.

The flux footprint: We totally agree that the footprint of the flux tower should be taken into account when performing these kinds of comparisons. We therefore performed such an analysis using the footprint model FSAM by Schmid (1994, Boundary-Layer Meteorology). Unsurprisingly, the footprint was found to be less than the 1.5 x 1.5

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km area previously used and we reduced the window over which reflectances were extracted only to cover pixels which fell inside the footprint source areas. This means that the results, or rather values, retrieved from the analysis have been reformed. The following will be changed in respect to this matter:

i) We will add a section in Data and methods called Eddy covariance footprint (around page 2991, line 3 in first submission) in which we will describe the method used to calculate the footprint.

ii) We will add a short description in Data and methods under Satellite data on how the result of the footprint analysis was overlaid with the MODIS-data, how pixels for analysis were chosen (p. 2992, l. 15-20).

iii) In the section Results R^2-values, RMSE-values, tables, figures etc., affected by the altered window size, will bee changed accordingly.

iv) A section will be added to the Discussion (p. 2996, l. 9)

Reviewer 1:

I wonder whether it would not be appropriate to indicate the variability among the 3x3 pixels with standard deviations in the appropriate figures.

Good idea. We will add this to the figure showing the seasonal progression of measured and modeled GPP (Figure 5a in first submission).

Title: why not use gross primary production this is what the manuscript is about.

Agree. Will be changed.

p. 2988, l. 23: "estimates of carbon" - too vague; pools/fluxes of carbon; carbon - CO_2, CH_4, VOC?

Ok. Will change to fluxes of CO_2.

p. 2998, l. 19: this is rather a summary than a conclusion.

Ok. The Conclusion will be rewritten.

p. 2999, l. 16: who are the authors of this reference?

Author/Authors were anonymous. We were of the assumption that anonymous authors should not be specified in the reference list. Anyways, will change this.

Table 1: what does the lowermost line in the table refer to?

Nothing. It was not in the manuscript but somehow got in there anyway but we will see if we can remedy this somehow.

Figs. 2, 3, 5, 6: why not express GPP as an 8-day daily average with units of gC/(m2d) instead of 8-day sums, this would make comparisons with other studies much easier.

It seemed more logical make all results as consistent as possible, keeping to the MODIS 8-day periods and to show the mean error over all of these.

Reviewer 2:

Why did the authors use a soil water content factor to estimate Reco?

We will add some more details regarding this in the Data and methods section (p. 2990, l. 14). Using both temperature and water availability in determining CO2 emissions, especially for areas with dry conditions as in the Sahel, is important. In this article, the soil water dependency on Reco was assumed to be multiplicative. Using soil moisture together with regression model of Lloyd and Taylor (1994, Functional ecology) to estimate Reco for a savanna environment has previously been used by for instance Hanan et al (1998, Global Change Biology). By implementing the soil water content factor, effects of the amount of available water on respiration are taken in account with Reco declining linearly as the soil dries.

Why did the authors choose these indices/why not other indices like MODIS NDVI or NDWI to compare their performances?

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This is a very good point. We will extend our manuscript with an analysis of NDVI as well. A short description of NDVI in Data and methods under Satellite data (p 2991, l. 3) will be added. We will also add the results from the NDVI analysis in the section Results together with figures. Regarding NDWI, Fensholt & Sandholt (2003, Remote sensing of Environment) tested two configurations of SIWSI based on surface reflectance in the MODIS NIR and MODIS band 5 (1230 to 1250 nm) and MODIS band 6 (1628 to 1652nm). MODIS NDWI use reflectance 860 nm and 1240 nm which correspond to one of the configurations tested. The configuration similar to NDWI was shown to perform slightly worse in this case. We therefore did not use this configuration in our analysis. However, this should be mentioned, so we will add a text about this in Data and methods under Satellite data (p 2991, l. 3).

Why are chlorophyll-based and water based indices correlated in this specific case (why are there strong relationship between EVI and SIWSI?)

We think we touch this subject in the discussion. The high correlation between chlorophyll based indices and water based indices is mainly caused by similarities in seasonality. Although EVI depict an increase and a decrease in plant growth whereas SIWSI would depict an increase and decrease in water availability, they are proportional to each other in this case. However, it is important to mention that this proportionality might only be true for a set of specific species and conditions.

p.2992, line 1: do you mean Reco?

Yes. Will be changed

p. 2994, Results, line 9. Although vegetation index values are relatively low... explain. If vegetation indices are high, there is saturation so you might not be able to see a trend. But if the values are low?

This is true. The sentence is badly constructed and will be changed.

p. 2994, Results, line 17. Although the analysis show... Did you mean... As the

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analysis show?

Yes. Will be changed.

p. 2994, Results, line 19-21. I am not sure what you mean here. Explain better.

Ok, will do.

p. 2996, line 1. Is this significant?

Yes, regression was statistically significant. But since there are cyclical trends in all sets of data used, we decided not to show significance.

p. 2996, line 5: It is also apparent... Maybe you could explain better this statement later in the Discussion chapter.

We will add an explanation to this statement in the Discussion (p. 2997, l. 6). What we basically mean here is that growing season measurements are rather scattered. The low vegetation cover in the beginning of the growing season is the main cause of this. If we basically were to separate measurements, correlation between SIWSI and soil moisture would be more near zero for growing season values.

Table 1: explain the missing data thing (5th variable, what do the lines mean??)

Nothing. It was not in the manuscript but somehow got in there anyway but we will see if we can remedy this somehow.

Fig. 2 (and other figures): for the lazy reader who is not reading the text, explain where the data come from (MODIS EVI, Eddy Covariance GPP, etc).

Good point. Will be taken care of.

Fig. 2c. The light green dotted horizontal line is not clear. If the 2a and 2b seem to be the 0, it is difficult to understand what the line in 2c means.

Ok, will add clarity to this and edit figure 2c.

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