

Interactive comment on “Analysing surface energy balance closure and partitioning over a semi-arid savanna FLUXNET site in Skukuza, Kruger National Park, South Africa” by N. P. Majozi et al.

Anonymous Referee #2

Received and published: 29 March 2016

General comments The authors evaluated a 15-year EC data record of a savanna FLUXNET site. The authors focus in their analysis on the surface energy balance closure and energy partitioning. Among others they derive monthly mean surface energy fluxes and energy balance ratios (EBR) over the last 15-years. The topic fits very well into the scope of HESS, and the dataset would be interesting for a broad readership of HESS. My main criticism is that the data analysis remains in large parts superficial and does not go into depth. That is also the reason why at the end the authors do not come to really novel conclusions. There is no question that this is a great and unique dataset. This dataset must be published, but this dataset also deserves an in-depth analysis. Many questions remain open: What was the reason for the huge systematic

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errors in the flux measurements of the years 2004-2008 and 2013? Do the data show any long-term trend with regard to the energy and water fluxes? Are the EC flux data sufficiently accurate to be used to validate satellite-based evapotranspiration methods? What's about the uncertainty range of the flux data due to the lack of the closure of the energy balance? Which years were the extreme years with regard to temperature and rainfall? Etc. Before publication the authors must spend significantly more efforts in the analysis of this great dataset!

Specific comments Line 21-24: This paragraph is not really needed in the Abstract.

Line 27: Please cancel the word concept.

Line 39: Because there are besides canopy and ground heat storage other minor flux terms, I suggest to rewrite the sentence as follows: "... (G) heat fluxes and other minor flux terms such as heat stored by the canopy and ground."

Line 65: Please rewrite as follows: "... is an accepted performance criterion of EC flux data ...". Remark: Please use everywhere the introduced abbreviations.

Line 86: The abbreviation EB was not introduced before.

Table 1: Please state here the number of replicates of the soil heat flux measurements. And it remains unclear, whether the authors considered the soil heat storage change in the upper 5-cm layer in their calculation of the soil heat flux at the surface. Please explain!

Table 1: Please state the exact installation depths of the CS615 probes.

Line 117: Please explain here or in the Discussion why you decided to switch from a closed-path to an open-path gas analyzer. Is the change in the instrumentation somehow related to the performance of the EC system?

Line 120: As in line 117: Please explain why you did not continue to measure net radiation with the CNR2 sensor.

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Line 157: The summation sign is not needed here. Write the equation simply as $(H+LE)/(Rn-G)$.

Line 174: The authors should give at the beginning of the Results section an overview of the weather conditions over the last 15 years. Which years were particularly dry or wet. Which years were particularly warm or cold. Did you observe any long-term trend in the weather data.

Line 176: From Figure 1 I would expect that the year 2013 was the year with the largest number of missing values and not the year 2001. Please explain.

Line 214: What do you mean here with combined? Please explain. Did you not exclude here the years with low EBRs? Why does the mean EBR here does not agree with the figure (0.93) you gave in chapter 3.1.1?

Line 236-242: Here it is not sufficient to explain the low EBR over the night time by referring to other studies. Please check you statement/conclusion against your own data. Are low EBRs related to low friction velocities?

Line 249: I think it would be better to compile all the numbers in a table, and please do not aggregate the data to multi-year daily means. You lose so much information. The authors should think about, for example, to give mean, minimum, and maximum monthly fluxes for every year.

Line 255-260: Why did you limit this analysis to the year 2012? Please explain.

Line 270-272: Here the meaning of the months in brackets remains unclear. Also here it would be better to compile the data in a table. Please give in this context also the Bowen ratios.

Figure 6: I suggest to plot the data as stacked columns and to include in the figure the residual! In this context it would be also important to give the possible range of fluxes due this residual (see e.g. Falge et al., 2005; Ingwersen et al., 2015) and to discuss whether the residual hampers the use of the data to validate satellite-based

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evapotranspiration methods.

Line 338: This sentence reads strange. The sensible heat flux is not a part of net radiation. Please rewrite.

Line 331: Please revise the conclusions. They remain too general and on the level of text book knowledge. There must be something novel that we can learn from this 15-year long-term EC data record.

References: Falge, E., Reth, S., Brüggemann, N., Butterbach-Bahl, K., Goldberg, V., Oltchev, A., Schaaf, S., Spindler, G., Stiller, B., Queck, R., Köstner, B., and Bernhofer, C. (2005): Comparison of surface energy exchange models with eddy flux data in forest and grassland ecosystems of Germany, *Ecol. Model.*, 188, 174–216.

Ingwersen, J., Imukova, K., Högy, P., Streck, T. (2015): On the use of the post-closure methods uncertainty band to evaluate the performance of land surface models against eddy covariance flux data, *Biogeosciences*, 12 (8), pp. 2311-2326.

[Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-76, 2016.](#)

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