Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-685-RC2, 2017

Hydrology and Earth System Sciences
Discussions
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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* Nobuhle P. Majozi et al.

Anonymous Referee #2

Received and published: 7 February 2017

General comments The authors evaluated a 15-year EC data record of a savanna FLUXNET site in Kruger National Park. This is a great and unique dataset. The authors focus in their analysis on the surface energy balance closure and energy partitioning. The topic fits very well into the scope of HESS, and the dataset will be interesting for a broad readership of HESS. The dataset is carefully evaluated for several aspect. The authors give interesting insight to technical problems that showed up over the 15 years, and they analyzed, among others, the effect of the season as well as the friction velocity on the EBR.

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My major concern is related to the measurement of the ground heat flux. Firstly, important information is missing. As far as I got it, the authors did not determine the heat storage change in the layer above the heat flux plate (HFP). Please state this clearly in the Material and Method (MM) part. Moreover, it remains unclear how the three HFP readings were averaged. Two were installed under tree canopies and one at open space. How did you compute the mean ground heat flux representative for the footprint? Did you compute a weighted mean or did you compute simply the mean over the three plates. In the latter case the ground heat flux would be systematically underestimated, because the areal fraction of tree canopy is only 30%. Secondly, in general neglecting the heat storage term must result to a certain extent in a systematic underestimation of the ground heat flux and hence to a systematic overestimation of the available energy and consequently to an underestimation of the EBR. From own measurements (HFP were installed in 8 cm depth) I know that this storage term can reach at unshaded surfaces 50 to 100 W/m2. Please discuss in detail the magnitude of error that might originate from your methodological approach.

Moreover, I wondered why the authors do not give any information on monthly or annual evapotranspiration rates (in mm). With that information one could get a guess of the climatic water balance of that ecosystem. I think this would be very interesting for the reader and would further strengthen the manuscript.

Specific comments Line 132: Please state which software tool (e.g. TK3 or EddyPro) was used to process the EC raw data. Line 135: How did you detect outliers? Please explain! Line 170: The intention of Figure 1 is to show temperature, VPD and rainfall anomalies between the years. I think this way of displaying the data is not really optimal for this purpose. The authors should think about a better way to present these anomalies. One way could be to compute for every month the difference from the 15-year mean and list these differences in a table (rows: month; column: years). Months, for example, that were warmer than the 15-year average get a red color, months that were colder get a blue color. The larger the difference the more intensive the red and

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blue color. Line 198: This is a little bit data cosmetic. The very good EBR is achieved thanks to the really bad year 2013, which had an EBR of 3.76. If you remove this year as outlier the mean EBR reduces to 0.77. I suggest that the authors start this chapter with explaining the technical problems that showed up over the years with the very low EBR and the extremely high EBR in 2013. And after that the authors should refer only to the years with no data or technical issues. Figure 2: In the OLS approach, the dependent variable (turbulent fluxes) is plotted against the independently derived available energy. See for example Wilson et al. (2002). If you plot it the other way round, as you did, the slope of the regression does not fit to the EBR. If the EBR is below one than the slope must also be below one. In the year 2007, for example, the EBR is 0.44 but the slope is 1.46. That does not fit together. Moreover, if you use the turbulent flux in a regression as independent variable your statistical model assumes that this variable has no error. Please correct everywhere the figures and update the numbers for slopes and intercepts! Line 205: Here it would be important for the reader to know how you modeled the in- and outcoming longwave radiation, so that they can avoid this mistake in future. Please describe this model in more detail.

Technical comments Line 28: Avoid the wording bad and good data. Please use instead e.g. low- and high quality data Line 40: I would not count energy stored in ground as a minor flux term (see above). Please rephrase. Line 83: If you start the sentence with first I expect that there comes a second item. Line 150: Replace “incorrect assumption” with “simplification”. Line 148: Introduce here the symbol “R2”. Line 158: Rewrite “4” in “four”. Line 224: Here it is unclear which storage term was included by Sanchez et al. (2010). Please rewrite! Table 1: Why clayey? In the MM part you write that the texture ranged from sand to loamy sand. Please check! Table 1: Campbell Scientific is not the manufacturer of the HFP. The manufacturer is Huskeflux. Please correct that and mention whether you used self-calibrating plates or not. Table 1: Beside the wind speed the anemometer measures also the sonic temperature. Please add this variable to the list. Fig. 2, 3 etc.: Please mention in the MM part which software you used to create these graphs. Line 498: Replace “ground conduction heat”

with “ground heat flux” Line 239: Typo: “if” not “It” Line 257-258: Please rewrite this sentence. This sentence is unreadable. Line 323: From here on the numbering of the figures is wrong. In this line, for example, your refer to Fig. 8 not to Fig. 9.

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