

Interactive comment on “Energy balance closure and footprint analysis using Eddy Covariance measurements in Eastern Burkina Faso, West Africa” by F. Bagayoko et al.

Anonymous Referee #1

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Review: “Energy balance closure and footprint analysis using Eddy Covariance measurements in Eastern Burkina Faso, West Africa” by F. Bagayoko, N.C. de Giesen, and S. Yonkeu, # hessd-2006-0003

- 1) Does the paper address relevant scientific questions within the scope of HESS? Not at all
- 2) Does the paper present novel concepts, ideas, tools, or data? Not at all
- 3) Are substantial conclusions reached? No

- 4) Are the scientific methods and assumptions valid and clearly outlined? No
- 5) Are the results sufficient to support the interpretations and conclusions? Not at all
- 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Not at all
- 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? No
- 8) Does the title clearly reflect the contents of the paper? Yes
- 9) Does the abstract provide a concise and complete summary? Yes
- 10) Is the overall presentation well structured and clear? Yes
- 11) Is the language fluent and precise? Yes
- 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Not at all
- 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No
- 14) Are the number and quality of references appropriate? No
- 15) Is the amount and quality of supplementary material appropriate? No supplementary material

In my opinion the paper cannot be considered for publication. Major concerns are on methods and materials. Results cannot be evaluated since, the materials and methods are not appropriate for achieving the main paper objective, which is to test the energy balance closure in a West African field.

Comments:

- 1) Material and methods: this is a major concern. Indeed if the objective of the study is

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to test the energy balance closure the methodology is not appropriate because: a. you should measure all the principal components of the energy balance, i.e. latent heat (LE) and sensible heat (H) fluxes, net radiation (Rn) and ground heat flux(Q). Instead the authors measured LE and H with an eddy correlation system and estimated Rn and Q from air temperature mainly. They tested the eddy correlation measurements of LE and H with simplified models for estimating Rn and Q. This is a very approximate approach. And I have several concerns on the methodology used for estimating Rn and Q (see the following comments). However, I can't understand why if you installed very expensive instruments for eddy correlation measurements (gas analyzer and sonic anemometer) you didn't installed cheaper instruments for measuring Q and Rn. b. Moreover I don't see any data of rain or soil moisture, which are key elements in the savanna systems for understanding land surface interactions and energy balance closure problems, and analyzing the results. See, e.g., Baldocchi et al. (2004), Kurc and Small (2004), Williams and Albertson (2004) and Detto et al. (2006). c. Rn is estimated through a very simple model, equations (1)-(11). My major concern on this is that a key element of the net radiation estimation is the surface temperature (Ts), which is in equations (4), (5) and (6). Surprisingly Ts is not defined in equation (4) and it is not explained how it is estimated throughout the paper. The estimate of Ts is a major problem in heterogeneous terrains. For instance, measurements of Ts can be made with infrared transducers. Moreover, the estimate of Rn with the proposed model is very approximate. You need net radiometer measurements for testing this model. In conclusion Rn estimate cannot be considered correct. And it cannot be considered a way for testing the estimates of the other terms of the energy balance. d. The estimate of Q is made through a model, which is a function of the soil temperature (T). But T is not measured. The authors wrote that the initial T "was set as soil surface temperature, which was calculated by extrapolating the air temperature at 2 m and 10 m above the soil surface". First the soil temperature is very different from the surface temperature. Second, it is very difficult to estimate the surface temperature from air temperature measurements at 2 m and 10 m. In my opinion the estimate of Q cannot

be considered accurate. You need field measurements of Q for testing this model. A lot of common instruments are available. In conclusion Q estimate cannot be considered an appropriate way for testing energy balance closure. 2) Footprint analysis: a. first of all the authors need a 2 dimensional footprint model. The field is heterogeneous, hence it is very important to estimate the footprint of the field in both the dimensions. Several 2-D footprint models exist. See, for instance, Detto et al. (2006) for a simple 2-D model. b. The trees of the field were located through a GPS. It is much more detailed to use high-resolution satellite images, such IKONOS or quickbird. See Baldocchi et al. (2004), or Detto et al. (2006). This is a very useful tool also because you can distinguish and quantify the vegetation cover density of each tree. 3) Results and discussions: I can't considered acceptable the results because the simplified estimate of R_n and Q cannot be considered a test of the eddy-correlation estimates of LE and H, which are themselves estimates from simplified models especially in heterogeneous terrains. See previous comments. In the following three comments that are correlated, and that want highlight that the conclusions are not a new finding: 4) introduction section, pag. 2792, rows 4-6: it is not true that usually "the researchers don't pay much attention to the position of the measurement station with respect to the dominant wind direction". Any micrometeorologist that use eddy correlation tower knows it. It is written on the instrument manual. 5) Introduction section, pag. 2792, rows 17-19: the authors stated that "in order to obtain representative surface flux samples over a terrain in such region, the station should be installed such that the fetch area covers the higher number of trees within the dominant wind direction". This is not true. If the terrain is heterogeneous you have to put the tower where the terrain is heterogeneous. It depends if you want "see" more trees or grass. It depends from the footprint. Recently a lot of work has been made on footprint in heterogeneous terrain: Finnigan (2004), Detto et al. (2006), Kim et al (2006). 6) Pag. 2802, rows 6-9 and pag. 2803, rows 4-5. Again, see comment 4), any micrometeorologist that use eddy correlation tower knows that "before installing the station one could first find out about the dominant wind direction". It is not a new finding.

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Minor comments:

11) introduction section, pag. 2791 and 2792: the year of the Culf et al papers is 1997 (as written in the introduction) or 2004 (as written in the reference section)? 12) introduction section: the last paragraph seems to be repeated twice.

References not cited in the paper

Baldocchi D. D., Xu L., and N. Kiang (2004), How plant functional-type, weather, seasonal drought, and soil physical properties alter water and energy fluxes of an oak-grass savanna and an annual grassland, *Agric. Forest. Meteorol.*, 123, 13-39. Detto M, Montaldo N, Albertson JD, et al., Soil moisture and vegetation controls on evapotranspiration in a heterogeneous Mediterranean ecosystem on Sardinia, Italy, *WATER RESOURCES RESEARCH* 42 (8): Art. No. W08419 AUG 11 2006 Finnigan, J (2004), The footprint concept in complex terrain, *Agric. Forest. Meteorol.*, 127 (3-4) Kim J, Guo Q, Baldocchi DD, et al., Upscaling fluxes from tower to landscape: Overlaying flux footprints on high-resolution (IKONOS) images of vegetation cover, *AGRICULTURAL AND FOREST METEOROLOGY* 136 (3-4): 132-146 FEB 1 2006 Kurc, S. A., and E. E. Small (2004), Dynamics of evapotranspiration in semiarid grassland and shrubland ecosystems during the summer monsoon season, central New Mexico, *Water Resour. Res.*, 40(9). Williams CA, Albertson JD (2004), Soil moisture controls on canopy-scale water and carbon fluxes in an African savanna, *Water Resour. Res.*, 40 (9), Art. No. W09302.

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