

Interactive comment on “On the factors influencing surface-layer energy balance closure and their seasonal variability over semi-arid loess plateau of Northwest China” by X. Xiao et al.

Anonymous Referee #2

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General

The inability of micrometeorological surface flux measurements to close the energy balance of earth surfaces is a common observation. Being against the fundamentals of energy conservation, this topic has been subject to extensive theoretical and experimental research during the last few decades. An overview of the existing research was given some years ago by Foken (2008), who summarized the current knowledge on the topic and concluded that the lack of energy balance closure is likely to be related to large-scale exchange processes.

This paper presents analysis of surface energy balance closure (EBC) based on eddy-

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covariance (EC) measurements made in semi-arid loess plateau of Northwest China. The authors use several indicators and methodologies to study the EBC at their measurement site. They apply Ogive-analysis to study the low-frequency (large-eddy) contribution on turbulent energy exchange (Moncrieff et al., 2005; Foken et al. 2006) and flux source-area analysis (Rebmann et al., 2005). In addition, they compare three different methods to estimate soil heat flux (G) and its influence on EBC. The topic of the study is potentially interesting and appropriate for HESS.

Unfortunately the analysis and the results presented in this paper are almost completely site-specific and cannot be generalized to this type of ecosystem. The paper reports information which is of course for great importance to people working with the experimental data from this specific site. However, since neither the methodology or the findings and their interpretation are new, I doubt that the paper in its present format provides new insights on the EBC problem. Especially, sections 4.1 (EBC effects of flux contribution from the target source zone) and 4.2 (EBC effect of low-frequency part of turbulence spectra) serve mainly as a basis for quality analysis and pre-screening of flux data before interpreting ecosystem energy- or gas-exchange characteristic at this site. I do not see significant development of already existing methodology in this paper or broader significance of the site-specific results. Therefore I cannot recommend the paper to be published in HESS.

Other comments

The quality of English is rather poor, which makes parts of the text very difficult to understand. The organization of the paper is quite odd and should be greatly improved. The Results and Discussion sections are overlapping and both contain extensive parts which would rather belong to Methods.

The characteristics of the SACOL site are not sufficiently described. Without knowing details on the spatial heterogeneity of the site, the reader is without tools to understand the results presented in section 4.1 (Figure 2 and Table 2). Moreover, it is quite ob-

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vious that if there is significant heterogeneity within the footprint of EC –fluxes (as in this site?), the source areas of net radiometer and EC –setup will differ and possibly cause low EBC-ratios. The description of measurement construction, instrumentation, data-acquisition and post-processing is not complete, e.g. details on net radiation, soil temperature and soil moisture measurements etc. are missing. The authors should significantly improve this part to convince the reader on the quality of the measurements.

The EBC -analysis is done separately for the four seasons but this choice is not motivated. Are there distinct seasonality in climatic conditions or it this choice arbitrary?

What is the basis of neglecting the biomass energy storage term in EBC analysis? What is the neglected “additional energy source/sing Q (p. 563 l.18-19)?

How the values in Table 1 were calculated; sentence on p. 563 l.25- is very unclear?

Majority of values given in Results and Discussion have too many significant digits.

p. 566 l. 19-25. Sentence very unclear.

p.567 l. 10-11. “In a similar way we analyzed the seasonal flux contributions in a range of z/L and wind directions in agreement, on the whole, with the above.” What is meant with this sentence?

p. 570 l. 1-12. Motivate why you define Ri_w as in eq. 10 and use it as an indicator for the strength of the turbulent mixing over more conventional measures such as friction velocity? Moreover, did you classify the EBC to Ri_w classes or vice versa as the text implies?

p. 572 l.7-8. So? It is obvious that correct measure of G will be needed to close the energy balance.

p. 572 l.14-17. This information is not relevant for broader audience. Also, the spring data quality is not optimal but only slightly better than in other seasons.

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