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Biogeosciences Discussions

Interactive comment on “Role of net radiation on energy balance closure in heterogeneous grasslands” *by* C. Shao et al.

Anonymous Referee #1

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Summary:

I have reviewed two earlier drafts of this paper for a different journal. A comparison yielded that with the exception of adding one author (G. Tenney), some refined wording in the abstract, and a slightly revised title the main body of the manuscript has essentially remained the same. It appears the authors did not attempt to address the final set of recommendations by the earlier reviewers and overcome major weaknesses of their instrumentation / analysis and overreaching conclusions. The addition of one author without any changes in the main body also raises the question of the author's contribution. My recommendation therefore remains the same: a rejection of the current manuscript for publication. My original comments are referenced below.

– Comments on revised draft This is the revised, resubmitted version of a paper that

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Interactive Discussion

Discussion Paper

was initially rejected for publication. After carefully reviewing the revised version of the manuscript as well as the responses to the two reviewers comments, I recommend final rejection of this paper for the following reasons:

1) The authors didn't address the reviewers' concerns and comments satisfactorily. Despite the many changes the authors did to the manuscript, which led to improvements in some sections, the authors still claim to have used high-quality net radiometers suitable to produce accurate estimates of the residual in the surface energy balance (Page 16, ln 306-308).

2) The authors also forewent the opportunity to make the spatial variability of the net radiation measurements a focus of the manuscript. This potential was the basis for my initial assessment of the results as to 'have international value'. However, the authors did not improve this part with regard to data analysis, footprint calculations, or spatial geostatistical methods. In fact, the concluding statement found on Page 22, lns 423-424 even emphasizes that sensor differences are more important than spatial variability as evaluated from this setup. This is not surprising, again, given the low-grade sensor used in this study.

3) The paper lacks a clear focus. Although some sections may have a scientific merit of their own, the combination of discussions including sensor accuracies, clipping treatment and spatial variability is done with little skill so that the paper appears to have no focus.

4) Uncertainties in Eddy-covariance data processing: Despite the fact that the authors included a brief description of the EC processing, many questions remain open: Did the authors use periods for regressing the available energy against the sum of turbulent fluxes when gaps were filled with look-up table values? Falge et al. (2001) discussed gap-filling primarily with a focus on carbon dioxide fluxes, not sensible and latent heat fluxes. Furthermore, the incorrect use of citations in the EC section may suggest that the authors are not familiar with the EC data processing.

5) The lack of new, convincing and well-conceived conclusions: What have we learned from this study what hasn't been known already? As I said, reporting sensor accuracies and absolute numbers of the residual in the surface energy balance without a high-grade reference standard are futile. Yes, we knew that selecting good sensor makes a difference in our ability to estimate the energy balance components.

– Comments on original draft

Summary:

This paper investigates the impact of net radiation measurements on the closure of the surface energy balance (EB) in several grassland ecosystems in Mongolia. The study goes beyond simple instrument comparisons and also investigates the effect of surface heterogeneity on the EB closure as well as the effect of management regimes (grazing) of different intensity simulated by clipping. Although many studies have investigated the effects of uncertainties and errors arising from different types, models, and corrections of/for net radiometers over the past decades, the novelty of this study is the evaluation of spatial heterogeneity of the surface conditions and its contribution to explain a fraction of the observed residual in the EB. Although I have large concerns about the accuracy of the measurements taken using both the CNR-1 and Q7.1 radiometers (see comments below), I believe that the precision of the Q7.1 observations used to investigate spatial heterogeneity is sufficiently high and the results have international value. I further found that the result and discussion sections were somewhat rushed and imprecise, not stating all assumptions and foremost not tapping the entire potential of the rich data set. I encourage the authors to go beyond simply stating the differences without explanations, but to explore possible reasons for the observed residuals. The authors made an attempt to do so, but it can be much improved. The study design was sound but left a few important questions open (see comments below). In general, the language is acceptable even though some paragraphs need revisions to clarify meaning. Its length is appropriate, figures informative and clear, yet figure and table captions will need to be revised carefully. In summary, I believe that the results

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have international value and the topic is appropriate for the journal. I recommend conditional acceptance with major revisions given the authors can address the comments listed below.

General comments: 1) Choice of sensors: Since the net radiation is the largest component in the surface energy budget, great caution must be exercised when selecting the instrumentation measuring the amount and sign of energy available for the turbulent fluxes of sensible and latent heat as well as the molecular conductive subsurface transport of energy. It has been shown by many studies that both sensors selected in this study do not fulfill the high requirements to radiation sensors to give meaningful absolute, ie., accurate readings of net radiation, despite their frequent use in similar EB or ecological studies. Thus, the magnitude of the residual cannot be evaluated in absolute terms without assigning an appropriate uncertainty (sensor error) to the results. Only the technological development of high-grade radiation sensors typically referred to as secondary WMO standards in the 1980/90s made the scientific community become aware of the non-closure of the EB, which has remained a recurring, unsolved issue. However, I believe that the comparison of two sensors used in this study can be used to evaluate its relative deviation. This relative, comparative nature of the results has to be clearly stated.

2) Processing of eddy covariance (EC) data: For completeness, the authors need to include a brief summary of the EC data processing rather than just referring to their previous publication. A comprehensive evaluation of the various correcting and transforming steps commonly applied to EC data was presented in Mauder, M. and Foken, T., 2006. Impact of post-field data processing on eddy covariance flux estimates and energy balance closure. Meteorol. Z., 15(6): 597-609. A similar brief discussion of its impact on the EB closure in the current study would greatly benefit its importance for the community.

3) Discussion of potential causes for the observed residuals: The discussion of potential causes for the observed residuals was disappointing in a sense that the authors

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offered rather ‘generic’ explanations found in previous studies rather than exploring their data more deeply using creative ideas. Calculation and comparison of Ogives (cumulative cospectra) may help to explain observed differences among the different days. Another possibility may the exploration of differences in wind speeds across the paired net radiometers/ cup anemometer stations resulting in different magnitudes of wind speed correction factors, just to name a few. I was surprised the authors didn’t discussed the most likely potential causes known to affect the non-closure of the EB (eg., Foken, T., 2008. The energy balance closure problem: An overview. Ecologic. Appl., 18(6): 1351-1367) despite the fact this paper was mentioned in the introduction.

4) Study design: It was not clear to me why the authors placed the array of additional mobile EB systems using the Q7.1 sensors downwind of the paired EC/ CNR-1 system. Since the flux footprint extends upwind of the sensor location, any heterogeneity measured by the mobile Q7.1 would be meaningless in a discussion of EB closure since the footprints don’t even overlap. The authors need to clearly state why this spatial configuration was chosen. In addition, I couldn’t find any information on prevailing wind directions, or observed wind speeds. An estimation of the flux footprint for the EC data would be extremely valuable to evaluate the representativeness of the measured turbulent heat fluxes.

Detailed comments:

a) Page 4, ln 59 and throughout the manuscript: The author should adopt a more precise and unambiguous wording to describe their results: qualitative (greater and smaller) should be preferred over judgmental (better and worse) expressions; it would be of advantage if the authors used the words non-closure/ imbalance/ residual of the EB instead of energy balance closure (EBC) in combination with qualitative adjectives.

b) Page 5, ln 82: Despite all efforts to attribute the non-closure of the EB solely to differences in instrumentation, comprehensive studies have shown that the residual doesn’t vanish even when highest grade-sensor are applied with greatest care. At this point,

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Interactive Comment

a conceptual explanation seems rather adequate based on what energy transporting eddies are captured by various sensors, which depends on site and atmospheric conditions. Please see discussion in Foken (2008) for details. Hence, we are far from 'a final conclusions' of the problem, so please consider removing this sentence.

c) Page 7, ln 134: Do you mean 'arbitrarily' instead of 'randomly'?

d) Page 7, 2nd paragraph: Please include a short description of your EC data processing for completeness.

e) Page 8, ln 147, and throughout the manuscript: It is incorrect to speak of wind speed 'calibrated' net radiation measurements, as this refers to a comparison with a standard. The authors want to say that it is wind speed 'corrected'.

f) Page 10, ln 191: It was not clear to me how many days were used for the comparison. Are all results based on comparisons of the individual days 12,16 and 17, or were data observed over a period of 12, 16 and 17 days? In either case, why were so few data selected for this study?

g) Page 10, In 200: Very interesting point: later in the text the authors mentioned that the residual was smaller (at a minimum) when soil moisture was increased. So, was the non-closure of the EB in general smaller directly after rain events? This may point to a possible explanation of the residual that needs to be discussed.

Page 11, ln 208: do you mean 'were significantly different' by 'detectable'?

Page 11, ln 218: The acronym OLS is only introduced in figure captions, not in the text. Please add.

Page 12, 2nd paragraph: As I mentioned before, these results on spatial variability should be a focus of the study since they are novel and exciting!

Page 12, ln 244-245: Interesting finding. How do the authors explain the observed differences: differences in albedo (more ground shines through in heavily clipped areas), or different species?

Page 14, 2nd paragraph: The difference in importance for wind speed corrections during day- and nighttime shouldn't come as a surprise recalling the physical reason of the correction: the thermal conductivity (resistance to radiative and heat transfer) of the boundary layer surrounding the sensor is a function of wind speed (depending on the Reynolds number). At night, the heating of the sensor is negligible as shortwave radiation input is negligible, and wind speeds are typically much smaller. A physical explanation for the observations needs to be provided.

Page 15, ln 296ff: Again, both sensors are no high-grade instruments are not used anymore in high-quality EB studies. Results here have rather relative, no absolute character.

Page 16, ln 328: I disagree. The observed spatial variability can explain a large fraction of the residual. I can't see where the authors get the estimation of $\sim$ 100 Wm-2 from – all figures (1,2, 4,6) show a maximum of 80 W m-2. If spatial variability can explain as much as 13 W m-2 on average, and soil heat flux an additional 40 W m-2, then a large fraction of the residual can be explained (given the signs agree).

All figure and table captions: Each caption must stands on its own and the reader needs to be able to understand the plot without reading the entire manuscript. What do a,b,c mean? How many days are shown? What experiment are the results from? Are ensemble averages or individual diurnal courses shown? How large was the variability? What are the errors bars in Fig. 5? . . .

Interactive comment on Biogeosciences Discuss., 8, 2001, 2011.