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.

About the adding authors, it is my duty to clarify some facts. I did this field study when I was a PhD candidate in years 2006 and 2007. Miss G. Tenney with her teacher (also mine) designed a spatial variation of G and Rn contributing to the energy balance closure (EBC) study in 2006. We worked together to finish this research and then published a paper in soil heat flux aspect. Considering many other reasons other than spatial variability of Rn (our results showed a 20 W m-2 among the ~110 W m-2 lost energy) could affect the EBC and we wanted to quantify how much of the effects on the EBC from these reasons, I designed additional experiments including differences from instruments between CNR1 and Q7 (as a main part because we thought the wide use of these two radiometers in the community at the former manuscript, not now), mowing intensity, source area and dome condition. We did not emphasize the spatial variability of Rn in the first draft. But after submitted and followed the reviewers' opinion, we changed the focus on spatial variability via weakened the part of differences from instruments between CNR1 and Q7, also including your advice saying this part was the international data and valuable to publish. Considering this point, we added Miss G. Tenney and the other two co-authors who contributed to our earlier field work.

– Comments on revised draft

This is the revised, resubmitted version of a paper that 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).

By using the sentence (Page 16, ln 306-308) "Clearly, the error related with the different Q7.1 could be neglected with good maintenance", we did not attempt to claim "have used high-quality net radiometers suitable to produce accurate estimates of the residual in the surface energy balance". We wanted to illustrate a comparable result between the Q7.1s. As a commercial goods, the system errors of good maintained Q7.1s might cause together either higher or lower than the 'truth value' in all the treatments, it should not be one Q7.1 higher while other one lower than the 'truth value'. This point could not deny our analysis in the manuscript. We revised this sentence following the other reviewer with "Clearly, the error related with the different Q7.1 could be reduced with good maintenance."

This study is therefore only a relative intercomparison to identify differences between net radiometer, source area, vegetation and dome condition without necessarily aiming at estimating the "true value".

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.

You know, we cannot sample the vegetation in order to get consistent Rn results. So we designed the clipping experiments to quantify the footprint affections. We will try to learn if spatial geostatistical methods are suitable for our future researches.

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.

We illustrated this in the introduction part of the manuscript. The unclosure of energy balance is due to underestimation of the turbulent flux and/or overestimation of the available energy. The R_n is the largest flux term in the energy balance of a terrestrial ecosystem. Fluctuation of a small percent of R_n would significantly affect the EBC. (1) What is the magnitude of the spatial variability in R_n at the three EC measurements grassland sites? (2) What are the potential causes of R_n variation within and between eddy covariance measurement grassland sites? (3) What are the contributions of R_n to EBC at these sites due to its spatial variation caused by heterogeneous vegetation? To answer the above questions, we considered our conditions and designed four field experiments in a typical grassland type of Inner Mongolia and applied a mobile energy system, which consists of nine net radiometers and other meteorological sensors with three eddy flux towers to record spatially independent R_n and associated surface properties (i.e., vegetation and soil). We specifically hypothesize that the heterogeneity of vegetation structure plays an important role in energy balance closure due to its direct alterations of the outgoing (or reflectance) of short- and long-wave ration that determine the magnitude of net radiation.

Only after we considered these uncertainties from Rn, we could further compare the EBCs between eddy covariance sites and clarify the effects of unclosure on the EC measurements. Although the loss energy derived also from the turbulence energy, the contributions from the available energy could not be neglected. We did noticed some former studies just compared EBCs among site while they did not consider the large differences from the Rn measurements and even constructed relationships between the CO2 fluxes or Bowen ratios with their EBCs.

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.

1. We'd like to introduce what we dealed with the EC data processing and EBC calculation method. The July to September raw 10hz TS data from the eddy covariance measurements were processed off-line using the EC_Processor software package (http://research.eeescience.utoledo.edu/ lees/ECP/ECP.html) (Noormets et al, 2007; 2010; 2008), which were corrected by the double rotation method. The turbulent fluxes were adjusted for fluctuations in air density using the Webb-Pearman-Leuning expression (Webb, 1980). A series of data quality controls were used in the EC_Processor and before the gapfilling, for example, data quality was judged by atmospheric stability, obvious outliers were removed, such as anomalous or spurious data that were caused by sensor malfunction, sensor maintenance, rainfall events, IRGA calibration, power failure, etc. The friction velocity u^* (Goulden et al, 1996; Moncrieff et al, 1996) <0.15 m s^{-1} were used in this study (Zhang et al, 2007). After these quality tests the remaining data were classified as 'good data' to submit to gap-filling procedure. Considering the effects of gapfilling data on EBC, rainy days data were deliberately avoided, as you say, because the turbulent flux instruments would not work well at those times. Then we got the same set of data from 12, 16 and 17 days at sites I, II and III, respectively, were compiled into 30 min averages to illustrate the residual fluxes of EBC and in linear regression method. The friction velocity u^* <0.15 m s^{-1} were used in this study. If deleted all of the u^* <0.15 m s^{-1} data, the residual method would be no data in the morning, evening and even the whole night. Your question we have detected and found there are not much changes by the OLS coefficient of determination (r2) <0.03%, the OLS slope

<1.5%, and the intercept changed little, respectively, compared with/without the gapfilling data in the selected days data, both in Q7.1 and CNR1. On the other hand, we can use these gapfilled data to illustrate the Rn measurements acted on the EBCs although the absolute data of the turbulence data are important, either. We really cannot get all the absolute data under all the conditions due to the limitation of the eddy covariance method. Again, only the no raining day's data were used in our analysis. The following is the equations and the r2 with/without the gapfilling data. And we also attached our lookup table data for gapfilling the turbulence fluxes, and the data of regressing the available energy against the sum of turbulent fluxes and the gooddata criterion of site I in a separated excel file. We like further discuss on these data.

For Q7.1

Site	With gapfilling data	Without gapfilling data	R2 with	R2 without
I	T=0.90A-2.37	T=0.90A-0.52	0.956	0.954
II	T=0.85A-9.54	T=0.85A-9.37	0.946	0.944
III	T=0.77A-0.55	T=0.78A-0.51	0.922	0.921

For CNR1

Site	With gapfilling data	Without gapfilling data	R2 with	R2 without
I	T=0.84A+15.10	T=0.84A+15.78	0.956	0.953
II	T=0.84A+0.11	T=0.84A+0.81	0.949	0.949
III	T=0.70A+15.7	T=0.72A+15.89	0.918	0.919

T=H+LE, A=Rn-G

We attached the raw data and calculation process in a separate excel file.

2. Falge et al. in year 2001 published two papers (Falge et al, 2001a; Falge et al, 2001b), one(2) is C gapfilling method and the other(1) is energy fluxes gapfilling method. See below and attached excel file pls find our lookup table following this method:

(1) Falge E, Baldocchi D, Olson R *et al.* (2001a) Gap filling strategies for long term energy flux data sets. *Agricultural and Forest Meteorology*, **107**, 71-77.

(2) Falge E, Baldocchi D, Olson R *et al.* (2001b) Gap filling strategies for defensible annual sums of net ecosystem exchange. *Agricultural and Forest Meteorology,* **107**, 43-69.

3. About EC data processing, there are no commonly methods are accepted by the community. Including TK2, Alteddy, EdiRE, EddySoft (Mauder et al, 2008), EC-processor, and many others derived from their own organizations. Some methods used the TS data and the other used the 30-min online flux. We used the EC_Processor software package (http://research.eeescience.utoledo.edu/lees/ECP/ECP.html) (Noormets et al, 2007; 2010; 2008), which was derived from our lab.

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 highgrade reference standard are futile. Yes, we knew that selecting good sensor makes a difference in our ability to estimate the energy balance components.

The R_n is the largest flux term in the energy balance of a terrestrial ecosystem. Fluctuation of a small percent of R_n would significantly affect the EBC, which link with the accurate or not of the carbon and water fluxes measurements from eddy-covariance technique. Inadequate spatial sampling of R_n, especially when patchy vegetation and complex terrains exist, has also been examined as another possible reason for EBC problems (Malhi, 2004; Schmid, 1997). An EC site is conventionally selected to meet the theoretical needs of a large, homogeneous, and flat landscape. While turbulent energy components (i.e., sensible and latent heat fluxes) have a footprint of an entire ecosystem (normally, 50-100 sensor heights from all directions, Chen et al, 2004), net radiation and soil heat fluxes are sampled with a much smaller footprint, with about 100 m^2 for R_n and 10^{-4} m^2 for G. These mismatched measurement footprints would not be a problem when the spatial variation of vegetation, soil, and topography is minimal (i.e., vegetation source area contributes equally; Schmid, 1994; Schmid, 1997). However, such ideal conditions rarely exist. To improve the EBC, one solution is to increase the sampling numbers of R_n and G (i.e., increasing the measurement footprint) within the larger footprint of the turbulent footprints (Schmid, 1997; Shao et al, 2008).

But up to now, to our knowledge, nobody had quantified the spatial variability of Rn, and this variability's contribution to the energy balance closure. If we do not know these uncertainties, including from available energy, turbulent energy, etc, how to well explain the loss energy? How to well explain the relationship between underestimated CO2 with unclosure of the energy balance? We must go on with our research even we cannot get the high-grade instrument to measure the absolute truth value. As above, a commercial goods, the system error of good maintained Q7.1s might cause either higher or lower than the 'truth value' together, it should not be one Q7.1 higher while other lower than the 'truth value'. The Q7.1s were checked before field set up despite the new factory calibration. Differences in ±0.1 W m^{-2} for the Q7.1s at night were found, mean few differences between the Q7.1s. Again, we designed this study, only wanted to illustrate the reason of energy unclosure from the two commonly used net radiometers- Q7.1 and CNR1. One study could not work out all the problems. Furthermore, many studies (Oncley et al, 2007; Turnipseed et al, 2002; Wilson et al, 2002) used these two types of net radiometer to study the energy balance closure in the literatures. Wilson et al (2002) referred "Recent inter-comparisons of independently calibrated sensors at several sites showed only small differences in sensor type (unpublished data using Didcot DRN-305 and REBS Q7 at Brasschaat, Germany, and REBS Q7 and Kipp and Zonen NR LITE at Walker Branch, TN)". Turnipseed et al (2002) also referred "It is also unlikely that improper measurement of net radiation during the day is responsible for the energy imbalance, since both the CNR-1 and the REBS Q7.1 were in relatively good agreement over the extent of the study."

We then designed this experiment to give the first try in the real field to answer the questions.

– Comments on original draft

Summary:

The below answers were replied before. We have tried to revise the manuscript at last time following them.

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

Thank you very much for providing detailed, professional and constructive suggestions for improving the manuscript. We have revised the manuscript by focusing on the potential sources of measured Rn to EBC, including spatial variability, vegetation heterogeneity, domes, sensor type, and timing of future focuses. With your suggestions, the manuscript leans more toward the science.

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.

We have substantially revised our manuscript by placing a major effort in the comparative nature of the results from the two types of radiometers instead of the absolute net radiation value because the shortcomings from the instruments. The same as our original thought, the purpose of this manuscript was not to prove the superiority of one net radiometer over another, but to quantify the impact on EBC due of R_n measurements on EBC, resulted from two frequently used net radiometers within/among eddy flux measurement sites. We emphasized this point in the topic sentences by including other sources of error such as vegetation and dome conditions.

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.

We added the following paragraph to illustrate eddy-covariance data processing strategy as follows,

"**2.7 Eddy-covariance data processing and gap filling**

The EC data were processed with the "EC Processor" software (Noormets et al., 2007), which were corrected by the double rotation method using the Webb-Pearman-Leuning expression (Paw U et al., 2000; Mauder and Foken, 2006). We also removed anomalous or spurious data that were caused by sensor malfunction, sensor maintenance, rainfall events, IRGA calibration, power failure, etc. Data from stable nocturnal periods were also excluded, specifically when the friction velocity u^* (Goulden et al., 1996; Moncrieff et al., 1996) was <0.15 m s^{-1} (Zhang et al., 2007). Consequently, 29, 21 and 28% of the July-September data obtained from our EC systems from sites I, II and III, respectively, were discarded in *experiment 1*. These introduced data gaps that were filled following the methods of Falge et al. (2001), using sensible and latent heat fluxes. Linear interpolation was used to fill the gaps that were less than 2 hours by calculating an average of the values immediately before and after the data gaps. Larger data gaps were filled using empirical relationships (look-up tables). For each site, one look-up table, which sorted by photosynthetic photon flux density (PPFD) and vapor pressure deficit (VPD), was created from July 1 to September 30 and, after gap filling the data for corresponding days, were extracted and analyzed with the net radiation and soil heat flux data."

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 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.

Thank you very much for giving us these constructive suggestions. We made major revisions in this part following your advices. We moved the 'generic' explanations to methods part. Added wind speed correction probability to EBC. And revised referenced Foken, T., 2008.

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.

We added the site information on prevailing wind directions and the mean wind speed of the three sites with "The prevailing wind directions were northwest for sites I and II and southwest for site III with an average wind velocity of 3.5, 3.4 and 3.1 m s^{-1}

from June 1 to September 30 for sites I, II and III, respectively.”

A series of eddy towers, were constructed for long term flux observations. In order to get high quality representative data, we designed the EB system near the three eddy-covariance towers to verify the EBC problem. To not disturb the EC system, the EB system was deployed on the downwind direction within the footprint. Our measurements should not affect the spatial variability of available energy because each site was relatively homogeneous, although local vegetation structures within a few meters vary. Perhaps there was a misleading text in previous manuscript as we did not describe enough about the deployment plan of EC and EB systems. Additional text was added to clarify the EB installations and measurements in the methods section.

Our Footprint analysis was undertaken using the method proposed by Stannard (1997, Eqn (18), p382), indicating that approximately 99% of the measured scalar fluxes originated from all the three site towers in each direction.

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.

We adopted this suggestion and changed throughout the manuscript.

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, 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.

We deleted this sentence because it is too early to say 'a final conclusions'.

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

Change has been made accordingly in line 129.

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

We added a paragraph to introduce this process because it is important for the following results in lines 205-220.

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'.

Changes have been made accordingly in line 142.

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?

The EB system was rotated during the growing season of 2006; site I was sampled from July 9-29, site II from July 31-August 20 and site III from August 21-September 15. After abandoned the raining days (more in site I), 12, 16 and 17 days were retained for our analysis. We revised the text in the methods for *experiment 1*.

g) Page 10, ln 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.

Thanks for your recommendation. We introduced this phenomenon in the discussion part. Further study is also needed in this point.

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

We deleted this part in the revised text.

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

We defined the OLS method in “*Data analysis*” part with “the data was examined using an ordinary least square (OLS) linear regression by relating dependent turbulence energy ($H+LE$) and independent available energy (R_n-G).” in lines 226-228.

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!

Thank you very much for providing detailed, professional and constructive suggestions for improving the manuscript. We emphasized and expanded this point in the text.

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?

Yes, we are developing another manuscript focus on mowing effects on energy partitioning and relationships between community composition, structure. Results showed this is from the difference in species caused vertical structure and community litter shading as your decision “more ground shines through in heavily clipped areas” with high reflection.

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.

We deleted the relevant content because we thought it was not very necessary to discuss the wind speed correction as the reviewer I suggested.

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.

We revised this part with weaken the absolute differences instead of relative values.

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 ~ 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).

Good eyes. The ~100 W m^{-2} is the maximum imbalance 80 W m^{-2} from the Fig. 1 add the maximum spatial variability in Rn of 19 W m^{-2}. Why use this number also because we thought about the instrumental accuracy either. We revised this sentence in a more exact tone in lines 319-320.

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? .

Changes have been thoroughly made accordingly throughout the Figs and tables.

As you know, our data are half hour data in Fig.5, if error bars were added, figs showed unclear. But we are sure the errors did not affect the results we reported.

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