We would like to thank the reviewers for their comments.

- Our point-by-point responses are in *blue*
- Our proposed changes to the manuscript in *red*. Line numbers (L) are the new ones unless otherwise indicated.

Reviewer 2

Review of: Title: Surface energy balance fluxes in a suburban of Beijing: energy partitioning variability
Authors: Dou, J., S. Grimmond, S. Miao, et al

Summary:
========
This manuscript describes observations of radiative and turbulent fluxes over 16 months in a suburb of Beijing. The paper describes the results and fills in any missing information with models and/or ancillary information. As one would expect, as plants/vegetation starts transpiring and there is liquid water for evaporation in the summer, the latent heat flux increases at the expense of the sensible heat flux. There is some nice additional information related to how irrigation might be affecting these results...however, this type of information/result is not anything new or unexpected. I am not an urban specialist (and some of the comparisons with results from other urban sites seems a bit difficult to appreciate).

In general, the manuscript is well-written, though there are a few grammatical errors and confusing sentences which I highlight below.

There is a lot of information in the manuscript and the presentation is done well, but I don't think the overall results about how Qe increases at the expense of Qh are highly novel. I also think the manuscript would be much more impactful if the storage terms are actually measured rather than roughly estimated with a model and then hand-waving about the results look "reasonable". Finally, some of the references related to the SEB closure are a bit old/outdated and a more recent summary of SEB work should be provided.

We would like to thank the referee for the helpful comments and suggestions, as well as some interesting new references.

We have compared and summarized the previous reports of sub/urban sites and the observation results of two farmland with a similar climate to Miyun and believe that the seasonal variation characteristics of turbulent flux in Miyun are the result of the combined effect of local climate background and large-scale farmland irrigation. These new analyses have been added to Lines 430-442 in Section 3.3 of the revised manuscript.

Regarding the issue of heat storage terms and SEB work, please refer to our specific response below.

General Comments:
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1. There is something I don't understand about the footprint analysis...the primary wind direction is from NE/E (ie, Fig 2c-2f). however, the flux footprint (shown in Fig.1c) is from the E/SE....shouldn't the footprint follow the wind direction? It also does not make sense to me that the shape of the

footprint in Fig.1c is as "round" as it is...I would expect a very small contribution from the NW direction (since the wind rarely comes from that direction). What am I not understanding about this?

The eddy covariance flux footprint provides a basis to identify which area (and weighting) should be used to estimate the land surface fractions impacting the measurements. Generally, there are two methods for determining the flux footprint, the one way determine the footprint and resulting land cover fractions dynamically (e.g. for each 30-min period), whereas the other assumed a constant radius/contour (e.g. based on a climatological analysis or rule of thumb).

This study used the latter method. At Miyun, footprint distance (Fig. rev 1) is calculated by using the method of Kljun et al. (2004) in Eddypro software (v6.1.0 beta, LI-COR). After obtaining the output results of footprint distance (m) each 30-min during the observation period, the mean values of footprint distance every 5º are calculated according to wind direction. This calculated footprint distance of 360º around flux tower is recorded as a diurnal average. The daytime is defined as incoming short-wave radiation $K_{\downarrow} > 5$ W m^{-2}. Taking this as the standard, the footprint distance data involved in the calculation are divided into two data sets: daytime and nighttime. Then, according to the calculation process mentioned above, daytime and night averaged footprint distances are obtained (as Fig. 1c shown), respectively.

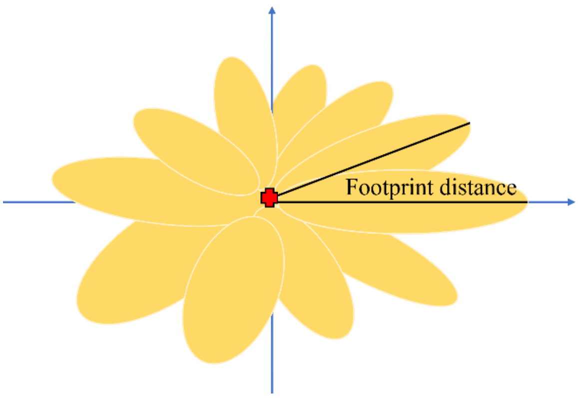

Figure rev 1. Diagram of flux source areas (90%, shaded ovals) and footprint distances (black lines) for 30-min period in various wind directions by using the flux source area model of Kljun et al. (2004) for study site.

2. In order to improve/push the science, there should be some measurements of the storage terms (rather than just state that these are "too difficult", so we are going to use a model). In my experience, the storage terms are critical for getting a successful surface energy budget closure (e.g, Leuning et al 2012; Swenson et al 2018). I realize that it's too late to change this, but I feel the paper would be a more significant contribution if those terms were measured.

We agree with your suggestion, however, we did not have the ability to conduct this measurement at that time. To carry out heat storage terms observation in urban, it is the basic experimental requirement to install a lot of thermometers in different land cover types (building roofs and walls, roads, grassland, cropland, wood, etc.) to measure surface temperature. Our research funds and the number of instruments cannot meet the experimental requirement. In addition, it is difficult to find suitable buildings and cropland for instrument installation. We regret not doing this.

3. Related to the previous comment: how is the thermal heat storage by buildings taken into account? It looks like values from Nanjing are used (Table B1). Were any measurements of building temperatures taken to confirm this? An IR temperature sensor? Also, though the soil heat flux is considered, the heat stored in the soil layer above the soil flux sensor does not seem to be considered. This can be a significant contribution to the surface energy budget.

One of the methods to estimate the heat storage flux is using the OHM model in this study. The urban canopy layer is viewed as a “box” by enclosing all surface elements (ground, buildings, and vegetation), and the heat storage is estimated by the accumulation of the heat storage of all surface elements in the box according to the proportion of their area as a weight.

The parameters in the OHM model used for the calculation of the heat storage by buildings at Miyun are the values from Nanjing (Wang et al., 2008). Measurement of building temperature is not carried out in this study.

To calculate the heat storage above the soil heat flux plate, it needs to use the observation data of soil temperature and volumetric water content and need to know the soil bulk density. Unfortunately, we don't have these data, so cannot calculate it.

Specific Comments
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* The authors suggest that the title should be: "Surface energy balance fluxes in a suburban area of Beijing: energy partitioning variability. I would suggest something shorter/simpler: "Surface energy fluxes in a Beijing suburb: energy partitioning variability.

Thank you for your suggestion. Since the energy flux terms analyzed here are all components of the energy balance equation, we feel that the current title is more appropriate and did not modify it.

* l.31, "information about surface energy balance exchanges..." I would re-word as, "information about surface energy exchanges..."

Done. The word “balance” in the sentence (Line 32) has been deleted.

* l.35, "In recent years, these have been...". Specify what you mean by "these"..."these measurements"? If so, cite a few cities and references as examples in recent years? The examples you list on l.28-29 are from 2005 which I don't consider "recent"...

The sentences "In recent years, these have been..." is modified to “Since the 1990s, EC methods have gradually been undertaken in cities around the world. These measurements provide information about surface energy exchanges, helped development of parameterizations (Grimmond and Oke, 1999b, 2002; Järvi et al., 2019), and evaluation and application of land surface models (Grimmond et al., 2010; Järvi et al., 2011; Järvi et al., 2014; Karsisto et al., 2015; Ward et al., 2016; Liu et al., 2017; Kim et al., 2019) and remote sensing products (Kim and Kwon, 2019).” in the revised version (Line 31-36).

"These" has been specified as "These measurements" (Line 31-32).

* l.43, I don't understand the sentence that starts with "These have located..."?

Sentence deleted (Line 43-45).

* l.52, "..provide many new insights." is vague. can you mention/highlight a few of the most important new insights from all of these papers/studies?

A summary of the new findings and new insights of two EC long-term observation studies has been added to line 53-58.

Text added (Line 53-58):

Such as at a suburban site in the UK, energy partitioning favors turbulent sensible heat during summer but latent heat in winter and is strongly dependent on land cover fractions (Ward et al., 2013). However, the seasonal variability of energy fluxes normalized by net radiation is relatively small in a residential neighborhood of Singapore, as the measurement site in the equatorial with a very small variability in the background climate (Roth et al., 2017).

* l.94, is "lateral" wind the same as cross-wind? If so, isn't lateral wind a component of the horizontal wind?

Yes, lateral wind is the same as crosswind. The words "horizontal, and lateral" has been changed to "along-wind, and crosswind" in revised manuscript (Line 101).

* l.100, why do you use the double-coordinate rotation? Why not the planar fit? Does this choice affect/change the results?

We chose double coordinate rotation instead of planar fit rotation for two reasons.

Firstly, planar fit correction is not suitable for Miyun. The effect of planar fitting on fluxes estimate was ever considered over an urban landscape in Helsinki by Vesala et al (2008). They found that the vertical rotation angle (the angle that indicates the slope of the surrounding surface) is a function of the wind direction, and the angle values were not in agreement with the local topography in the sector where massive buildings are located (the average height of the buildings is 20 m), which probably modify flow field and create artifacts in the rotation angle and lead to turbulent flux estimates deviation. Given that Miyun and Helsinki have similar land use characteristics (many residential buildings (~18 m) are located at the west, northwest, and north of the Miyun tower), planar fit rotation was not adopted in this study to avoid possible data estimates errors. Moreover, when applying the planar fit rotation, it needs a long period (days to weeks) to estimate the differences between the anemometer alignment and the mean stream field for a given measurement site (Foken, 2008). While at Miyun, the rapid growth of crops will lead to the continuous change of underlying surface height and flow field, which make it difficult to meet the time conditions for planar fit method application. It is suggested to use the double coordinate method when canopy height and roughness change quickly, such as during the growing season in a crop field (LI-COR, 2019).

Secondly, the double coordinate method has good applicability and is widely used in the EC data processing of urban and suburban sites. To some extent, the data from different observation sites are more comparable with the same data processing method.

In summary, for the reasons mentioned above, the double coordinate method is selected to use in this study.

Vesala et al (2008) found that the planar fitted fluxes were generally 10% lower than those obtained by double coordinate rotation. However, there is no specific standard or index to evaluate the fluxes of the two methods at present. Given most of the urban or suburban fluxes adopted the double coordinate rotation, it makes Miyun results comparable with those reports of research sits.

Foken, T.: Micrometeorology, Berlin: Springer Nature, 165-169. doi: 10.1007/978-3-642-25440-6, 2017.

LI-COR, Inc: EddyPro software instruction manual, 2-26pp, Version 7.0, 2019.

Vesala, T., Järvi, L., Launiainen, S., Sogachev, A., Rannik, Ü., Mammarella, I., Siivola, E., Keronen, P., Rinne, J., Riikonen, A., and Nikinmaa, E.: Surface-atmosphere interactions over complex urban terrain in Helsinki, Finland, Tellus. Ser. B. Chem. Phys. Meteor., 60, 188–199, doi:10.1111/j.1600-0889.2007.00312.x, 2008.

* l.101-103, From Table 1 I can't tell how much data are excluded for each reason...for example, how much data are excluded due to the LICOR poor-quality flag? How much is excluded due to precip? You should provide the reader with these details in Table 1...

The amount of the deleted Q_H and Q_E data due to poor quality (N=1552 (6.6%) of Q_H, 1987 (8.5%) of Q_E), during rain and two hours after rain (N=615 (1.4%)) has been supplemented on line 109-110 of revised version. At the same time, Table 1 has been deleted from the revised manuscript because its' data availability is displayed too cumbersome.

* l.104. I thought the EC sensor (ie, sonic) was installed pointing into the prevailing wind direction (see l.92), but now you are saying that it was installed based on the magnetic north? As long as the sonic orientation is fixed for the entire project there shouldn't be any need to correct WD for changes in the magnetic declination...the boom/sonic direction should be measured relative to true north (if using a compass to do this, then the declination angle needs to be accounted for)....or, perhaps I don't understand the point of this sentence?

EC sensor was installed pointing to the prevailing wind direction (90º), but this 90º is determined with a compass. The north direction determined by the compass is geomagnetic north, which has a declination angle with the geographical north. The north direction of land use/cover maps is geographical north.

When the characteristics of turbulent flux in a certain direction are analyzed, the impact of land use in that direction on the turbulent values will be considered. At this time, there exists an angle deviation between the direction of the turbulence and that of the land use/cover map, so the magnetic declination needs to be corrected.

* l.108, how much radiation data was removed due to the radiation data being outside of "physically reasonable thresholds"?

That radiation data beyond the physically reasonable threshold refers to those non-zero incoming and outgoing shortwave radiation data at night. These shortwave radiation data account for 5.2% of the available data. Given these data values are very small, basically no more than 1×10^{-5}, they did not been removed, but are forced to zero.

* l.110, "instrument failures". which instrument? the sonic? The IRGA? sometimes these failures are only for a day?

The data gap from January 14 to February 18, 2013, is due to CR3000 data-logger failure. Other short-term data gaps happened during the power cut caused by instrument inspection and maintenance. The data gap for one day was only a few hours of a power failure and did not last for the whole day.

To make the manuscript more rigorous, and also refer to the suggestion of another reviewer, the description for data gaps of a short time period has been deleted in the revised manuscript (Line 123).

* l.119, I did a google search for "ZQZ-TF, Aerospace Newsky Technology" and didn't find any information about this sensor. maybe it's easy to find it in Chinese, but not English? is it a sonic? prop-vane? since it might be a new sensor/model/company to the english-speaking world a few more details are needed...maybe provide a web link to more info?

ZQZ-TF is the model of wind sensor. Wind speed is captured using an anemometer with three lightweight, conical cups. Wind direction is captured via a wind vane.

It is easy to find the Chinese description of this instrument, such as through this website, https://max.book118.com/html/2019/0709/7033066031002040.shtm, but the English description is not found.

The website of Aerospace Newsky Technology CO., LTD is https://www.js1959.com, but I don't know why this website is currently inaccessible.

Since effective English information about the instrument and manufacturer cannot be provided at present, the web link is not added to the revised manuscript.

* l.122, what is the WUSH-BH? A data logger? Why do you need it to average to 1-min? Also, why not use the same averaging period as with the EC system (ie, 30-min)?

WUSH-BH is the model of the data logger.

The processing flow of observation data from meteorological stations is uniformly stipulated by China Meteorological Administration and cannot be changed at will.

When starting the data analysis of this paper, we can only download the hourly averaged data of Miyun meteorological station and cannot get the raw data to calculate 30 min mean values.

* l.135-136, Is there are reason "Normal" is capitalized? (seems to be capitalized throughout the entire paragraph/paper).

All "Normal" changed to "normal" in the revised manuscript.

* l.142-143, May seems very low/strange. Could this be a sensor problem? Does soil moisture corroborate the precip measurements? It also seems unlikely because the latent heat fluxes are fairly large in May 2013 (ie, Fig. 6f). It seems unlikely that Qe would increase if there was no rain that month...or perhaps you have an explanation for this? [ok, I see this discussed in Sect 3.5].

The precipitation in May is indeed very small. We have checked the precipitation repeatedly and queried the rainfall data of the automatic weather stations around the Miyun site, which are also small in May. It can be seen from Figure 9 (reordered as Figure 14 in the revised version) that soil moisture under natural conditions has been decreasing in May due to small rainfall. As we discussed in Section 3.5, larger Q_E in May could be attributed to supplement water supplied by cropland irrigation.

* l.147, what is "existence hours"?

"existence hours" changed to "the start and end times" (Line 161).

* l.160, if the buildings are 50.4 m and your measurement level is at 36m, then you are in the roughness sublayer and not the surface layer.

The building with a height of 50.4 m is located 180-185º south of the tower. For this direction, the observation height of 36 m is indeed in the rough sublayer rather than the constant flux layer. The turbulent heat flux in this direction represents the micro-scale energy exchange between earth and atmosphere rather than the local scale. However, the available flux data in this direction only account for 2.3% of the total data, which has little impact on the conclusions of this paper. Moreover, when energy fluxes and ratios are compared between farmland and buildings-dominated directions, the observed data in this direction did not participate in the calculation.

* l.177, For "During the observation period" you should point out that these are from the quality-controlled statistics.

The sentence (Line 194-195) is changed to "During the observation period, after quality control (N=17142, 30 min periods), the stability is predominately unstable (40.9%) and stable (42.4%), with neutral conditions for 16.7% of the time.

* l.195, The two papers cited (Wilson, et al 2002; Foken et al 2008) are rather old. There has been a lot of work in this area since then, you should provide a more recent reference and summary of recent work done in this area...

These two references are cited to explain that the underestimation of Q_H and Q_E is one of the reasons that $\Delta Q_{S,res}$ is the upper limit of ΔQ_S. It is not the focus of this paper to discuss the energy imbalance of EC observation. Given these two references are quite classic, a cite to Wilson et al. (2002) and Foken et al. (2008) is still retained.

* l.205, what does, "Daytime and daily mean fluxes of net all-wave radiation, sensible heat flux and latent heat flux are estimated based on monthly mean diurnal patterns." mean? How do you get daily mean fluxes from the mean monthly diurnal pattern? I don't understand this statement.

This sentence wants to express that because the missing data are not gap-filled, the monthly (seasonal) daytime and daily mean fluxes cannot be obtained by directly averaging the available 30 min data of the month (season). It needs to calculate the mean value of every 30 min within 24 hours of the month (season) firstly (i.e. the mean value of 00:00, 00:30, 01:00, 01:30, …… 23:00, 23:30 in a day), and then average the daytime ($K_\downarrow$ > 5 W m^{-2}) or daily (24 h) period. The monthly (seasonal) daytime or daily mean ratios (e.g. albedo, Bowen ratio, etc.) are estimated based on the monthly (seasonal) daytime or daily mean fluxes.

This sentence has been rewritten as follows (Line 223-229 in the revised manuscript):

Missing data are not gap-filled. The mean values of radiation and turbulent fluxes for each half-hour during a day in the month (season) are first calculated to get their mean diurnal patterns. Then the daytime ($K_{\downarrow}$ > 5 W m^{-2}) or daily (24 h) mean values are averaged from corresponding periods within the mean diurnal patterns. The daytime (daily) mean ratios are the ratios of daytime (daily) mean values of corresponding radiation and energy fluxes.

* l.209, "At the MY site, all radiation fluxes vary seasonally". This statement is true for any location on planet Earth. I don't think you need to inform readers of this...

The sentence 'all radiation fluxes vary seasonally" has been deleted in the revised manuscript (Line 232).

* l.220, rewrite, "...causing albedo becomes increases..."

Done. Changed to "becomes an increase to 0.6 and then a decrease with days since snowfall" (Line 245-246).

* l.238, rewrite, "...small impacts presence of snow...".

This sentence (Line 271-273) has been deleted in revised manuscript.

* l.263, "Hence, the MY values appear to be reasonable.". Can you explain why there is such a large variation in Qf among these different locations? What you wrote doesn't lead me to conclude that the MY Qf values are "reasonable"...

A few sentences of the Q_F comparison between Miyun and other suburban sites (Montreal, Swindon) in the world has been added to the revised manuscript (L309-325). The newly added content explains the rationality of Miyun Q_F value and the differences with those values of other suburban sites by comparing population density, temperature, heating/cooling demand, etc.

Text added (L309-325):

The median Q_F vary between 5 and 39 W m^{-2} diurnally across all of the months (Fig. 6). Heat released from buildings ($Q_{F,B}$) dominated Q_F, with its diurnal median value ranging from 4.6 to 36.4 W m^{-2} and accounting for 85-95% of Q_F (Fig. S2a). Contribution of vehicle emission ($Q_{F,V}$) is small (1-9% of Q_F). The maximum $Q_{F,V}$ value during the morning rush hour (8:00-9:00) is only 1.5 W m^{-2}, since the house is usually close to the working place or school and residents do not rely on cars for daily travel or commuting at MY (Fig. S2b). The diurnal variation of human metabolism ($Q_{F,M}$) is the constant within a year because of the fixed population density (5657 people km^{-2} in 2012 and 5702 people km^{-2} in 2013). Like other studies, $Q_{F,M}$ is small, with values ranging between 0.4-1 W m^{-2} and contributions being 5-8%.

The fluxes are larger in summer and winter associated with cooling and heating needs, respectively. Our $Q_{F,B}$ values are larger than estimated in other suburban sites, such as Montreal during winter (Bergeron and Strachan, 2012) and Swindon (Ward et al., 2013), even the air temperature in Montreal is lower in winter. The reason is that Miyun has a greater mean building height (13.1 m) and building cover (21%). Moreover, buildings include hospital and office buildings, which usually have greater energy consumption and emissions than residential buildings. However, our $Q_{F,V}$ values are lower than those reported in Montreal and Swindon, due to the small number of motor vehicles and various travel modes at Miyun.

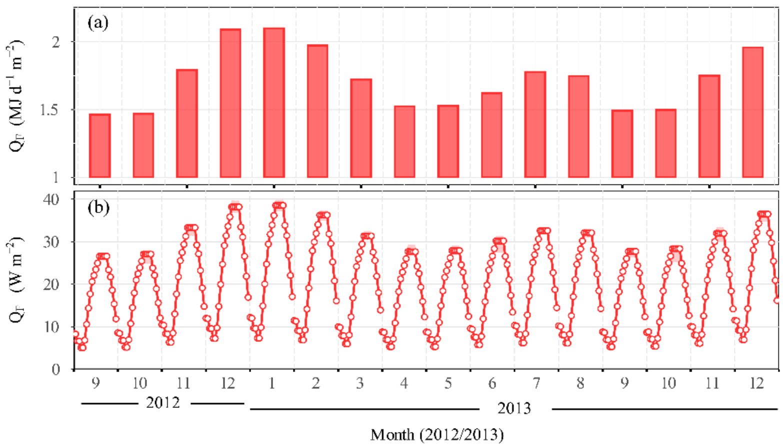

Figure 6: Monthly anthropogenic heat flux (Q_F) at Miyun (September 2012 to December 2013) calculated as described in Section 2.2: (a) mean total for 24 h, (b) median diurnal patterns with inter-quartile range (IQR) (shading).

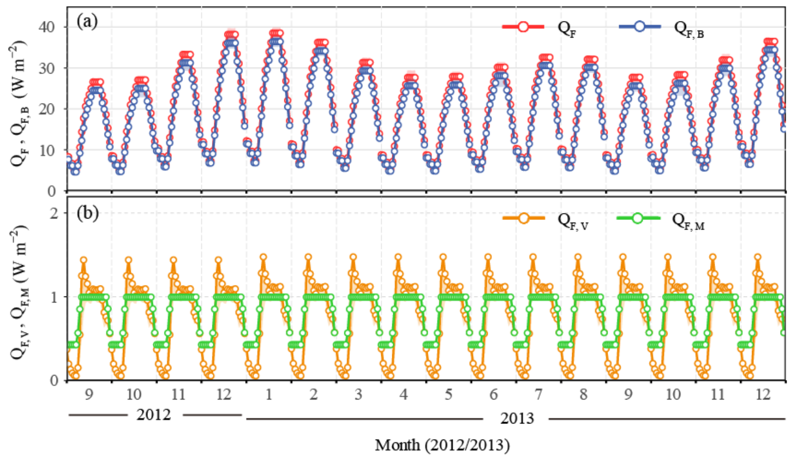

Figure S2: Monthly median diurnal pattern (points) with inter-quartile ranges (shading) of (a) anthropogenic heat flux (Q_F) and heat released from buildings ($Q_{F,B}$), (b) heat released from traffic ($Q_{F,V}$) and human metabolism ($Q_{F,M}$).

* l.276, "...not including all components of heat storage flux, such as biomass heat storage". Why don't you include the biomass storage?

To estimate biomass heat storage, it needs observation data of canopy temperature and leaf area index, and the values of specific leaf weight (mass of dry leaves per square meter of the leaf) and the water content on a wet master basis. However, we don't have these data and cannot calculate it.

* l.323, not only evaporation, but also transpiration (assuming there are crops/plants).

The word "transpiration" has been added to the revised manuscript (Line 439).

* l.342-345, not sure you need to re-state this since it's already discussed in the previous paragraph...

Restatement has been deleted from the revised manuscript.

Sentences (Line 461-464 in the revised version) has been modified to "Monthly totals of Q_E are between 0.32 and 7.16 MJ d^{-1} m^{-2} (Fig. 9e), with corresponding maximum and minimum monthly totals occur in two seasons (summer - July and winter - December 2013, respectively)."

* l.425-427, Fig. 9 is very helpful. That irrigation provides larger Qe is logical and having the soil moisture measurements to show this is very helpful/useful.

Figure 9 (reordered as figure 14 in the revised version) is slightly adjusted to show the soil moisture in 2012 and 2013 more clearly.

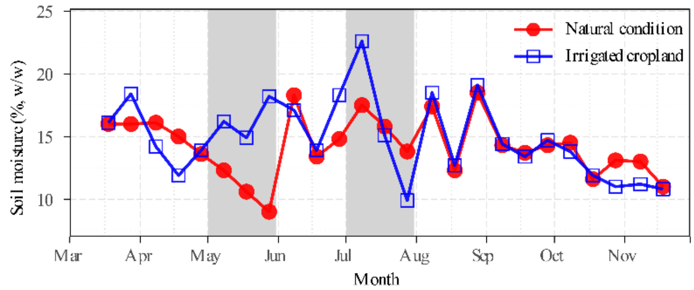

Figure 14: Gravimetric soil moisture (%) at a depth of 0.1 m measured on the 8th, 18th and 28th of each month at the MY site from 18 March to 18 November, 2013 under natural condition and irrigated cropland. The shading is May and July 2013, respectively.

* l.430-432, Fig. 11, these results could also be influenced by different magnitudes in the storage terms (which are not accounted for) for the land surface types.

We agree with the reviewer's comments. The heat storage terms (ΔQ_s) differ in land use/cover types. So, the ΔQ_s of different land use/cover types have varying degrees of influence on their respective Q_H and Q_E, and energy flux ratios ($Q_H/Q_\downarrow$ and $Q_E/Q_\downarrow$). However, given ΔQ_s is calculated as a residual term of the energy balance equation and its uncertainty is relatively large, it is difficult to accurately quantify the impact of ΔQ_s on the $Q_H/Q_\downarrow$ and $Q_E/Q_\downarrow$ of land use/cover dominated by impervious surfaces and farmland, respectively. Thus, we do not discuss it here.

* l.432-435, Fig. 12, this is an interesting result and the interpretation seems plausible. However, I'm not sure I fully understand what is shown in Fig. 12...for example, what does a frequency of < 60 (blue box) mean?

Figure 12 (reordered as Figure 17 in the revised version) in the manuscript shows the frequency distribution of Bowen ratios (β) in different ranges with the time variation since the last rainfall. The different color blocks in the legend represent frequency range (F), i.e. "F=0， 0 < F <5%， 5≤F< 10%， …… 55≤F < 60 %, 60 ≤ F < 65%".

Taking the frequency of Bowen ratio at farmland-dominated direction in the 14th hour since the last rain as an example (Table rev1), the frequency in the range of $0 < \beta \leq 0.5$ is 56%, so it is represented by a dodger blue block of $55 \leq F < 60\%$ (abbreviated as "< 60") in the Figure 12b (Figure 17b in revised version).

Table rev1. Frequency of Bowen ratio at the farmland-dominated direction in the 14th hour since the last rain.

Bowen ratio range	Frequency (%)
≤ -1	2
(-1, -0.5]	0

(-0.5, 0]	15
(0, 0.5]	**56**
(0.5, 1]	21
(1, 1.5]	6
(1.5, 2]	0
(2, 2.5]	0
(2.5, 3]	0
(3, 3.5]	0
(3.5, 4]	0
(4, 4.5]	0
(4.5, 5]	0
(5, 5.5]	0
(5.5, 6]	0
(6, 6.5]	0
(6.5, 7]	0
(7, 7.5]	0
(7.5, 8]	0
> 8	0

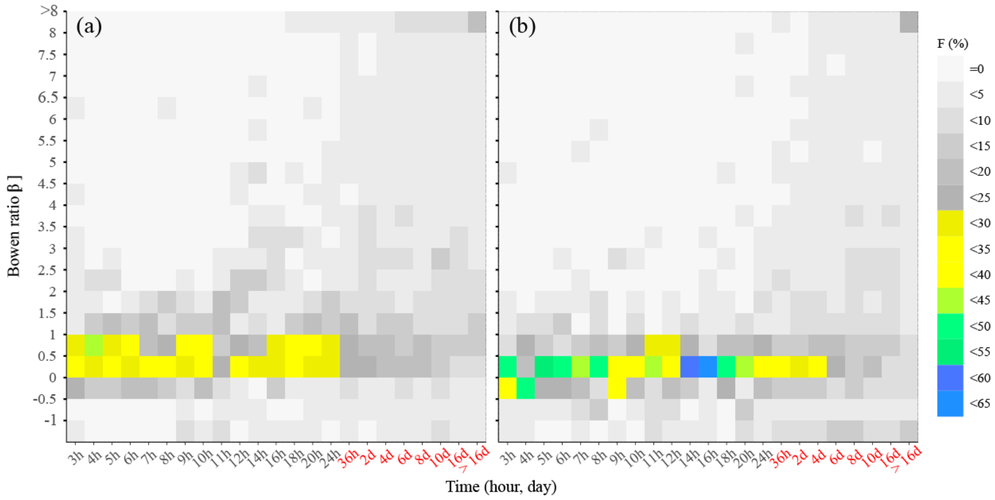

Figure 17: Frequency (F, %) of Bowen ratio β values by time since last rainfall for (a) building-dominant (210-360°) and (b) farmland-dominant (30-150°) directions.

References:

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Leuning, R., van Gorsel, E., Massman, W. J., and Isaac, P. R.: Reflections on the surface energy imbalance problem, Agr. Forest Meteorol., 156, 65-74, doi: 10.1016/j.agrformet.2011.12.002, 2012.

Swenson, S. C., Burns, S., and D. Lawrence: The impact of biomass heat storage on the canopy energy balance and atmospheric stability in the Community Land Model, Journal of Advances in Modeling Earth Systems, 11, 83-98, doi:10.1029/2018MS001476, 2019

Thank you very much for the recommended references. We have read them carefully but did not cite them, as the manuscript did not focus on the EC energy balance.