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Comment on acp-2021-1022

Anonymous Referee #2

Referee comment on "Surface energy balance fluxes in a suburban of Beijing: energy partitioning variability" by Junxia Dou et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1022-RC2>, 2022

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:

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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 Q_e increases at the expense of Q_h 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 Finally, some of the references related to the SEB closure are a bit old/outdated and a more recent summary of SEB work should be provided..

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?
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.
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.

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.

- * l.31, "..information about surface energy balance exchanges..." I would re-word as, "..information about surface energy exchanges..."
- * 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"...
- * l.43, I don't understand the sentence that starts with "These have located..."?
- * 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?
- * l.94, is "lateral" wind the same as cross-wind? If so, isn't lateral wind a component of the horizontal wind?
- * l.100, why do you use the double-coordinate rotation? Why not the planar fit? Does this choice affect/change the results?
- * 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...
- * 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?

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

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

* I.119, I did a google search for "ZQZ-TF, Aerospace Newsy 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?

* I.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)?

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

* I.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 Q_e 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].

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

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

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

- * I.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...

- * I.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.

- * I.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...

- * I.220, rewrite, "...causing albedo becomes increases..."

- * I.238, rewrite, "...small impacts presence of snow..."

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

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

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

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

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

* I.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.

* I.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?

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