

Interactive comment on “Area-averaged evapotranspiration over a heterogeneous land surface: Aggregation of multi-point EC flux measurements with high-resolution land-cover map and footprint analysis” by F. Xu et al.

F. Xu et al.

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Dear Referee # 3:

We appreciate very much for your valuable comments and suggestions on our manuscript. According to your comments and those from Referee #1 and Referee #2, we have carefully revised all sections of the paper (revisions and corrections are marked in red). Detailed response to your worthwhile comments and suggestions are as follows:

General comments:

C1

1. To address the scientific problem in this paper, 30-min flux might be sufficient, given the uncertainty in gap-filling method (rainfall, fog etc). Daily ET (section 4.4) does not help a lot here. Indeed, it might be more necessary to clarify data quality and uncertainty of the EC and LAS measurements.

Response: Thanks for your comments. Yes, by giving the uncertainty in gap-filling, the 30-min flux might be sufficient to address the scientific problem in this paper. However, the major objective of this study, besides refining the aggregation method for area averaged fluxes (based on our unique and comprehensive dataset of the HiWATER), is also finally useful for the water balance study extended to the whole Heihe River basin. So the area-averaged daily ET over the study area is added for reference. We have also clarified the data quality and uncertainties of the EC and LAS measurements in Section 2.1.1 of the revised paper.

2. Besides, P-M estimated ET could be removed.

Response: Yes. According to your comments (and those from other referees), we have removed the descriptions relevant to P-M estimated ET, such as those in Section 2.2.2, Section 3.3, and the paragraph on the comparison with P-M ET in Section 4.4.

Specific comments:

Page 5 Line 24 “following Fig.3” Since it is the first figure appearing in this article, it's better to change the number from 3 to 1.

Response: Accepted.

Page 6 Line 15 “EC data from 16 towers. . .” According to section 4.2, in addition to site 3, sites 5/8/13/16 were also not used. It is better to use a consistent dataset throughout the paper.

Response: We are sorry for the unclear statements about the data used in the paper. EC data from 16 towers were all used for analysis. We have improved our statements in Section 4.2 of the revised manuscript.

C2

Page 6 Line 16 “no irrigation” and how was the weather during the period?

Response: To choose “no irrigation” days is mainly for reducing the effect of local advection. The two days, 29 and 30 June 2012, were typical clear days.

Page 6 Line 22 “coordinate rotation” why not use Planar Fit?

Response: Our study area, the oasis in the middle reaches of Heihe River basin, is relatively very flat. To use the common 2-D rotation method is not only simpler but also enough in this situation. Actually, we have compared the results of 2-D rotation and Planar Fit during previous works. The differences were very small.

Page 7 Line 13 “MOST” there are different solutions. Add either corresponding equations or references here.

Response: A reference had been added: Andreas, E. L.: Estimating Cn2 over snow and sea ice from meteorological data, JOSA A, 5, 481-495, 1988.

And how were roughness height and zero-plane displacement estimated?

Response: The roughness height and zero-plane displacement of the study area were obtained following Martano (2000). This has been added in the revised manuscript. Martano, P.: Estimation of Surface Roughness Length and Displacement Height from Single-Level Sonic Anemometer Data, Journal of Applied Meteorology, 39, 708-715, 2000.

Page 7 Line 16 ‘daytime. . .’ It’s a bit confusing. Local time is better.

Response: We have stated the time difference between Beijing Standard Time and Local time in the revised paper.

For data quality control, what is the threshold value of signal strength?

Response: For BLS series (BLS4500/BLS900), the threshold value of signal strength is 1000.

C3

Section 2.2.2 This section could be abbreviated if the preliminary land cover has already been done by Liu and Bo (2015).

Response: Accepted.

Page 8 Line 11, specify the date of the google earth image.

Response: The Google Earth image used was collected on 3 September 2012. This has been added in the revision.

Page 8 Line 16-20, it might not be necessary to compare with PM-ET. The principle of that is the same as the comparison with LAS in terms of flux aggregation and there might uncertainty in PM-ET.

Response: As mentioned in the beginning of this reply, we have accepted this comment and made revision.

Section 4.3 Page 15 &16. It’s better to look into the details to figure out the factors contributing to the bias between EC and LAS, instead of just mentioning ‘heterogeneous distribution of surface covers’.

Response: Accepted.

Section 4.4 I didn’t see the difference between Table 5 and 6 in terms of addressing the problem despite their different units.

Response: We have removed all the related text on the comparison with PM-ET (including Table 6 and Table 5) in section 4.4 of the revised manuscript.

Thank you again for your valuable comments and suggestions on our manuscript. The revised manuscript is attached as supplement.

Sincerely yours,

Feinan Xu

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C4

PS. After revising our manuscript and finishing the above responses to you, yesterday, we received the comments from Prof. Thomas Foken (as Referee #4). Some important revisions would be needed based on his comments. A new version might be uploaded within two weeks.

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Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-602/hess-2016-602-AC3-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-602, 2016.