

Response to reviewer comments (AGRFORMET-D-14-00470)

We would like to thank once again the editor and reviewers for revising our manuscript, their positive comments and patience.

Response to Reviewer #1

1 General comments

The authors have substantiated their discussion section, but it appears they focused on adding a short review of literature material. The authors have added some site-specific arguments as well, but I would have preferred if they had gone further in discussing the energy balance components for their site (instead of referring to the literature). However, since the paper is now focused on comparing the WB versus EC method (despite only for one site) I think it could be published. In our view a speculation on possible magnitude of the minor fluxes at our study site, is not fully necessary in the present paper and could be misleading for the reader. The aim of the present study was to cross-check the evapotranspiration data obtained with the EC method against ET rates measured with the soil water balance method and to define a suitable post-closure method for the study site in order to calibrate and parameterize land surface model. Therefore, in the discussion part we wanted to keep focus on the results of our experiment and compare it with those described in the literature. One of our main conclusions is that there is no universal approach to post-close the energy balance gap, and that the composition of the energy residual is site-specific and therefore should be estimated for each study site separately. Nevertheless, we also wanted to keep the reader aware that the problem of imbalance is rather more complex than to decide which one from three fluxes responsible for the imbalance. We wanted to emphasize the fact that the energy balance gap could be made up by other unconsidered or biased energy storage terms, and therefore we introduced results of other studies. Because we did not perform any additional experiment on estimation of minor fluxes during the two campaigns, we would like to omit the speculation on these fluxes.

2 Specific comments

2.1 Main questions

1. Partly mitigated in the new version.

Please see above.

2. Clear. But I would prefer to see the semi-variogram instead of believing the authors on their word. I think the authors could have also added a line about this in the revised document.

We added the results of geostatistical analysis into the manuscript. (Line: 226-230, 368-372).

Please find below the semivariograms for different observation periods (Fig.1.).

3. I think the authors could have used this in the Discussion section.

We added this information into the manuscript (Line: 488-493).

2.2 Minor questions

4. 6789/20: Clear.

5. 6786/16: Improved in text.

6. In the Discussion section I couldn't find how much would the small terms would matter for their study site. It is okay to cite other studies, but the authors could have made a quick estimate for their site (based on literature for similar sites) for the OP during the growing season.

Please see above.

- 7. Ok
- 8. Ok
- 9. Ok
- 10. Ok

3 Technical corrections etc

3.1 References

11. The authors made some corrections but there are still some missing et al's for papers with multiple authors. E.g. lines 77, 81, 84 (maybe some more, please check). Also Mauder. M 2011 should become Mauder and Foken 2011.

Corrected.

3.2 New corrections

1. L129: has a gently sloping landscape

Corrected.

2. L173: units should be Roman, not italic.

Corrected.

3. L231 “we used the average footprint weighted based on the frequency”:
sound gibberish to me

We corrected it. It reads now as: “*In the present study, we used the weighted average footprint of these atmospheric stratifications.*” (Line: 231)

4. L137: please use a degrees symbol instead of a 0 superscript

Corrected.

5. L320: of

6. L341: the minus should be on the next line

Corrected.

Response to Reviewer #2

The revised version is almost ready for publication from my perspective. The authors handled my comments well.

I agree very much with the introduced lines L468-L469; this is an important point and too much neglected by the modelling community.

The reference added (Gebler et al., 2015) is not correct. The second author (H.J. Hendricks Franssen) is missing. Please correct.

Corrected.

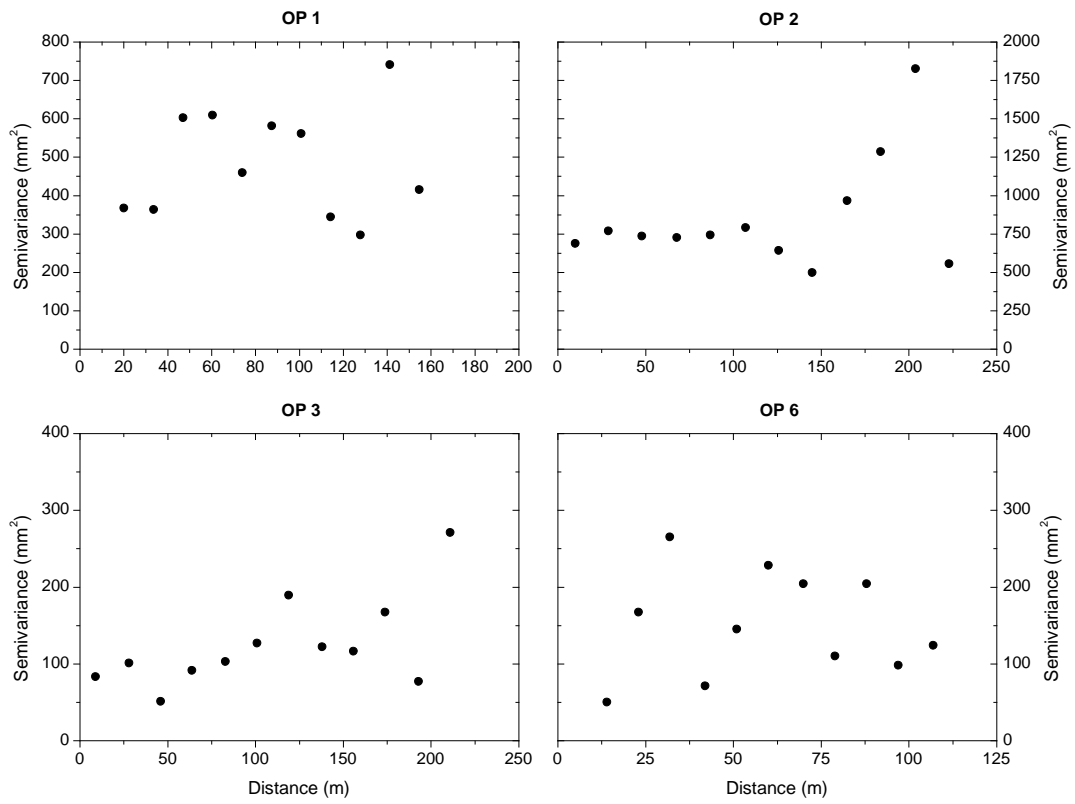


Fig.1. Semivariograms of ΔS