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Biogeosciences Discussions
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Interactive comment on "Remotely sensed land-surface energy fluxes at sub-field scale in heterogeneous agricultural landscape and coniferous plantation" *by* R. Guzinski et al.

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We would like to thank the reviewer for helping us to improve the quality of this manuscript. Please find our replies below the questions

General comments:

1. The adopted methodologies are sound and described in detail, but I do miss a better overview of the different model implementations and model runs being evaluated – a table or flow diagram with a clear outline of the tested configurations would be very helpful.

A flow diagram and the description of the model configurations used will be added.

2. I'm also missing some details on the Landsat data used; specifically number of valid acquisitions, their frequency and distribution over the growing seasons for the studied period (again a table/figure showing this along with e.g. LAI dynamics would have been helpful).

We will provide a figure showing the temporal distribution of Landsat scenes and the magnitude of LAI in those scenes.

3. Landsat-scale fluxes are extremely useful for applications that require sub-field scale resolution but the temporal frequency of valid acquisitions is an issue. This point should at least be mentioned in the paper with reference to recent work that tries to capitalize on the spatial detail of Landsat and high temporal frequency of MODIS for producing daily surface energy fluxes at the Landsat spatial scale (e.g. Cammalleri et al., 2013; Gao et al., 2006).

We are aware of the above studies and will add a mention in the discussion section of the paper.

4. A key concern is the lack of LE validation at the Landsat scale. Only the sensible heat fluxes are currently being compared against the tower observations all though for many applications the latent heat fluxes are of most interest. I would highly recommend including LE in the Landsat-scale flux validation. A full evaluation of the surface energy fluxes (H, LE, G and Rn) would have been even better. This would also provide a better basis for diagnosing the cause of the estimation uncertainties.

We have decided to validate only the H fluxes in the original manuscript to improve the clarity of graphs and the discussion. Since LE is estimated as a residual, the accuracy of H estimates should translate into the accuracy of LE estimates. However, we are modelling all the fluxes and will present validation of all of them against ground measurements in the revised paper.

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5. Validation of vegetation inputs (LAI) would also be useful for this purpose (if possible).

Unfortunately field measurements of LAI were only undertaken in 2013 and only in one of the fields covered by the agricultural flux tower footprint. We will assess whether this data can be used for validation of satellite based LAI estimates. We will also add a discussion of uncertainties introduced by LAI estimates.

6. The paper would benefit from a careful read through to correct for some language issues and several sentences should be shortened and rewritten for improved clarity and flow.

The paper will be edited to improve the language.

Specific comments:

1. Abstract: The abstract should also mention TSEB as it forms the basis for the Landsat scale fluxes.

A mention of TSEB will be added to the abstract.

2. Page 4858 L18: Firstly, only the surface heat fluxes are being evaluated which should be made clear here (or seriously consider including latent heat fluxes in the validation). Secondly, the accuracy very much depends on the dataset used (S75, S100) and the source of the meteorological input (Table 2, 4). It is important to mention this in the abstract.

Latent heat fluxes will be validated in the revised paper (see general comment 4). We will also clarify that the statistics mentioned in the abstract are coming from the optimal case.

3. Page 4859 L1-2: A full evaluation of the surface energy fluxes (LE, H, G and Rn) would have been welcome, and is typically done (e.g. Kustas et al., 2012, Advances in Water Resources, 50, 120-133). As a minimum LE validation results should be

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included.

Please see answer to general comment 4.

4. Page 4859 L11: Should also mention thermal resolution of L8 (100m).

The resolution of L8 will be added to the sentence.

5. Page 4860 L3: "more robust TSEB" – more robust compared to what?

The sentence tries to convey that ALEXI and DTD are more robust than pure TSEB, since they use changes in the observed temperatures instead of the absolute values. This will be clarified.

6. Page 4864 L24: thermal observations resampled from 120 (L5), 60 (L7) and 100 (L8) to 30 m by. . .

The sentence will be updated.

7. Page 4865 L1: The spatial and temporal resolution of the MOD08 gridded products used should be mentioned.

The sentence will be updated to include the spatial and temporal resolution.

8. Page 4865 L10-13: Resulting time-series of Landsat/MODIS scale LAI over the study sites should be displayed and validated (if possible). Large uncertainties are associated with the MODIS LAI in some cases and these will translate into the Landsat scale LAI as Feng Gao's regression tree approach produces MODIS consistent LAI. Given the importance of accurate LAI for surface flux mapping, uncertainties in LAI over the sites should be quantified (if possible), or at least mentioned (with reference to appropriate literature on MODIS LAI uncertainties).

Please see answer to general comment 5.

9. Page 4865 L21: Reference for Corine land cover missing.

The reference will be added.

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10. Page 4865 L24-26: Radiation inputs may also be supplied by geostationary satellites (GOES, Meteosat).

That is true but other required inputs, such as wind speed or air temperature, cannot be reliably estimated from geostationary satellites over land surface. In addition the study area is located at quite high latitude (around 56 degrees north) and therefore estimates derived from geostationary satellite observations might not be accurate due to longer atmospheric path.

11. Page 4866 L20. Duplicate symbols used for the view zenith angle (VZA or sigma) – choose one.

VZA is an acronym, while sigma represents the value of VZA in model equations. We think that the acronym and the symbol play a different role in the paper but will review the text to reduce any confusion caused by this, if present.

12. Page 4868 L15: The ALEXI pixel resolution depends on the resolution of the geostationary satellite and may range from 3 – 10 km.

The text will be updated.

13. Page 4868 L25-27: Not sure this is true; I believe a fairly coarse resolution air temperature prediction is needed for approximating a regional blending height temperature (as the authors mention themselves in the discussion – page 4879).

Blending height air temperature is certainly more spatially uniform than near-ground air temperature and we make use of this in the discussion on page 4879 and in other parts of the paper (e.g. section 2.3). However, in highly heterogeneous landscapes, especially when strong LST contrast is present, the blending height temperature might not be constant due to advection and localized couplings between the surface and the atmosphere (Anderson et al., 2004). This will be clarified in the manuscript.

14. Page 4869 L20-23: Not sure I understand correctly. In any case, validating LE would help determine if this is the case.

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Since LE is estimated as a residual of the other fluxes, the errors present in the other fluxes estimates will contribute to error in the LE estimate. H is calculated directly from model inputs, and so the errors in other flux estimates do not contribute directly to errors in H estimate. Therefore it can be assumed that errors in modelled H will be smaller than the errors in modelled LE. This will be clarified. Validation of LE will also be performed (see general comment 4).

15. Page 4870-4869: Consider creating a flow diagram of some sort depicting the required steps and model configurations used. That would be very helpful.

Please see general comment 1.

16. Page 4807 L20-21: It says here that disaggregated fluxes of both sensible and latent heat are being evaluated against the tower observations. As far as I can tell only sensible heat flux is being validated (Table 2 – 5). LE validation is only done at the MODIS scale using DTD (Table 1).

Please see general comment 4.

17. Page 4871 footprint model: Mention if the footprint model is considering atmospheric stability.

The Hsieh et al. (2000) footprint model does take atmospheric stability into account. This will be clarified in the text.

18. Page 4871 section 4.1: The DTD results are characterized by really high RMSE and low correlations even for the series implementation. It may partly be a footprint issue (as shown for H) but unfortunately the effect of the disaggregation on LE has not been assessed. Errors in the vegetation inputs and H/ET partitioning are other plausible causes. I think the authors mentioned this briefly in the text, but a fix should be implemented (or screened out if it can be justified) to avoid the large number of pixels with zero LE (modeled) (Fig. 2).

Whenever DTD cannot obtain a plausible solution at the end of model run (LE < 0), it is

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assumed that this is due to dry conditions and therefore there is no evapotranspiration. In those cases LE is set to 0 and the net radiation is split between H and G (see Appendix for details). This is a backup behavior of the model and can lead to inaccurate results when the lack of convergence in the model was not due to low ET but due to, for example, wrong inputs. This is happening much more frequently in the forest site than in the agricultural site which could point to inaccurate characterization of MODIS LAI or LST in the forest. However series resistance network leads to reduction in the number of those cases. In the revised manuscript we will show the results both with those cases included and excluded, since this is not the primary model behavior. Disaggregated LE will also be evaluated.

19. Page 4873 L13-15: It would be helpful having a figure showing the frequency and distribution of acquired Landsat scenes over the growing seasons, overplotted on LAI time-series for example.

Please see general comment 2.

20. Page 4873 L27 to 4874 L9: Please rewrite this part for better clarity, some parts (e.g. less points in panel (d)...) I don't fully understand.

The section will be rewritten to improve clarity.

21. Page 4875 L13: "reasonable accurate" I guess it depends on what you define as reasonable: : :the statistics in Table 3 indicate some major model performance issues.

In this sentence "reasonably accurate" refers to EF approach producing most balanced results at GLU, as is elaborated on page 4878 L4-11. Once LE validation is performed this should, hopefully, become clearer.

22. Discussion section 4.4: There are some very good points and observations here but try to write it in a more concise (short sentences) and to the point manner, as far as possible.

This section, like the rest of the paper, will be edited to improve the language.

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23. Page 4878 L25: Validation of net radiation could have been more integrated in the result section (along with LE and potentially G), for a full evaluation of surface energy fluxes (as the manuscript title suggests).

Please see general comment 4.

24. Page 4880 L26-27: The RMSE and bias depends on the dataset used S75 versus S100), and a bias of -14 W m-2 (S100) is not negligible and the correlation (0.65-0.94) does not constitute a perfect (1) agreement. Please be precise in your description of performance metrics. This sentence referred to the optimal case which is S100 dataset with ERA interim meteorological inputs, as shown in Table 4. However, the description of results will be made more factual and quantitative.

25. Appendix A: It is very detailed and most of the equations have already been listed in other publications (Norman et al., 2000), but on the other hand it is also helpful with a repetition and complete list. Please check that all parameters and symbols have been properly defined.

The purpose of the appendix is not to describe new model developments but to clearly show the model implementation used in this study. It is often the case that when model equations come from a number of different papers it is unclear which formulation was actually used, thus making it harder for others to replicate the results. We will add a clarification at the beginning of the appendices explaining this point. We will also double check the presented model formulations.

Page 4882: Try to avoid having duplicate symbols for the same variable (LAI and F, VZA and sigma).

Please see specific comment 11.

How is the vegetation height parameterized (measured, seasonally variable, empirical function of NDVI)?

Vegetation height at the coniferous forest was kept constant, while at the agricultural

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site it varied between a minimum and maximum limits based on an empirical function of LAI. This will be clarified.

P4882 L21: "vegetation width" – do you mean leaf width or row spacing?

Vegetation width refers to the plant crown width. This will be clarified.

26. Figures: very small text size – difficult to read properly.

The figures will be redone will larger text size.

Language issues (just a few selected issues listed. Careful review needed to correct for many, mostly minor, language and flow issues):

Please see general comment 6.

Interactive comment on Biogeosciences Discuss., 11, 4857, 2014.