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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.

Anonymous Referee #3

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The paper "Remotely sensed land-surface energy fluxes at sub-field scale in heterogeneous agricultural landscape and coniferous plantation" evaluates the utility of the Dual Time Difference (DTD) and Two-Source Energy Balance (TSEB) models for predicting land surface fluxes at the scale of MODIS (1 km) and Landsat (30 m), using different disaggregation methodologies and both tower-observed and modeled (ECMWF reanalysis) meteorological inputs. The paper addresses some interesting scientific questions of relevance within the scope of the Journal. The work represents an admirable and comprehensive modeling exercise, which contributes with new insights on the utility of these modeling frameworks for mapping surface fluxes (mostly sensible heat), and I'm generally supportive given attention to the specific issues raised below.

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. 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). 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). It would definitely be very interesting to test MODIS-Landsat data fusion approaches (within the framework of TSEB) in Denmark with its significant cloud cover and heterogeneous landscapes (maybe something to consider for future work).

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

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.

Specific comments: 1. Abstract: The abstract should also mention TSEB as it forms the basis for the Landsat scale fluxes. 2. Page 4858 L18: Firstly, only the surface heat fluxes are being evaluated which should be made clear here (or seriously con-

sider 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. 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 included. 4. Page 4859 L11: Should also mention thermal resolution of L8 (100m) 5. Page 4860 L3: "more robust TSEB" – more robust compared to what? 6. Page 4864 L24: thermal observations resampled from 120 (L5), 60 (L7) and 100 (L8) to 30 m by... 7. Page 4865 L1: The spatial and temporal resolution of the MOD08 gridded products used should be mentioned. 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). 9. Page 4865 L21: Reference for Corine land cover missing. 10. Page 4865 L24-26: Radiation inputs may also be supplied by geostationary satellites (GOES, Meteosat). 11. Page 4866 L20. Duplicate symbols used for the view zenith angle (VZA or sigma) – choose one. 12. Page 4868 L15: The ALEXI pixel resolution depends on the resolution of the geostationary satellite and may range from 3 – 10 km. 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). 14. Page 4869 L20-23: Not sure I understand correctly. In any case, validating LE would help determine if this is the case. 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. 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). 17. Page 4871 footprint model: Mention if the footprint model is considering atmospheric stability. 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). 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. 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. 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. 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. 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). 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. 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. Page 4882: Try to avoid having duplicate symbols for the same variable (LAI and F, VZA and sigma). How is the vegetation height parameterized (measured, seasonally variable, empirical function of

NDVI)? P4882 L21: “vegetation width” – do you mean leaf width or row spacing? 26. Figures: very small text size – difficult to read properly.

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

1. Try to avoid using “ones” in sentences like “empirical ones”, “the higher resolution TSEB ones”. There a several other examples of this and it should be avoided. 2. Page 4860 L3: “(TSEB) modeling scheme based models,..”. Instead try something like “TSEB-based models, such as..” 3. Page 4868 L24: “..might not necessary hold..” change to “..do not necessarily hold. . .” 4. Avoid using “halved” if possible; for example page 4873 L12 “RMSE is halved from. . .” 5. Page 4879 L11-17: “When comparing. . .”. Very long and confusing sentence. Try to rewrite for improved clarity. Several other similar examples in the manuscript.

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