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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 Dr. Kalma for detailed and constructive review of our manuscript. Please find our replies below the questions.

It is noted (p. 4860, lines 3-5) that the ALEXI and DTD models are based on TSEB. Similarly, on p. 4867 (lines 8-9) it is stated that ALEXI and DTD have been developed from the TSEB. The TSEB model has been presented in Appendix A1. Where is the TSEB version referred to in Sect. 3.1 and Appendix A1 different from the versions described in Norman et al. (1995), Norman et al. (2000), Anderson et al. (1995), and Anderson et al. (1997)? Similarly: where does the DTD model described in Appendix

A2 differ from that described by Norman et al. (2000), except for the formulation of the flow resistance network. Do we need these two Appendices? Also: is the disaggregation algorithm used here essentially the same as DisALEXI described by Anderson et al. (2004)?

The TSEB and DTD models used in the study are the same as presented in the listed papers, with the exception of the series resistance network in case of DTD. However we still think that the appendices are warranted. Their purpose 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.

Regarding the disaggregation algorithm, it is different from the algorithm described in Anderson et al. (2004) in two main respects: 1) the original algorithm used instantaneous H estimates during disaggregation, while the current approach uses the constant ratio; and 2) the original algorithm kept the ALEXI-derived air temperature constant and adjusted the high resolution Land Surface Temperature (LST) data, while the current approach keeps LST constant and instead adjust air temperature at lower resolution scale. This makes it more similar to the approach described in Cammalleri et al. (2013). However in the current study we go a bit further by evaluating 3 constant ratios for performing the disaggregation. In addition we do not perform the disaggregation on daily H fields, as is done in Cammalleri et al. (2013), and which would require multiplying the daily ratio by averaged daily net radiation or incoming solar radiation. Instead we use the constant ratio itself as a disaggregation factor. Finally, we apply the disaggregation to DTD derived fluxes, and not ALEXI derived ones, thus we are not able to set the initial blending height air temperature estimation to the ALEXI-derived blending height air temperature.

The general idea of weighing individual pixels according to their contribution to the EC

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measured flux (p. 4870, lines 24-25) makes sense. However, turning to lines 22-28 on p. 4872, it is not clear whether this paragraph relates to missing Landsat pixels in the MODIS pixel and/or to missing Landsat pixels across the footprint of the EC measured flux. This impacts on the two data sets S75 and S100. It is also not made clear on p. 4876 (lines 14-28) and on p. 4877 (lines 1-10).

Set S75 contains dates when Landsat pixels contributing to at least 75% of the EC measured flux are present. The percentage of Landsat pixels missing within the modelled MODIS pixels can be smaller or larger than 75%. Set S100, which is a subset of S75, consists of points where additionally all the Landsat pixels within the modelled MODIS pixels are present. This does not necessarily mean that pixels representing 100% of EC measured flux are present, since sometimes the measurement footprint extends slightly beyond the modelled MODIS pixels. However, in practice all the dates in S100 set contain at least 98% of EC measured flux footprint. This will be clarified in the manuscript.

The paper omits to describe explicitly whether results such as the 30-min flux values used in tables and figures relate to instantaneous observations and simulations or are average daily flux values. (I note that Norman et al. (2000) have shown how the DTD approach may be used to simulate sensible heat flux H throughout the day). Does one disaggregate instantaneous flux values or mean daily fluxes? The issue of non-congruent times of MODIS and Landsat observations needs to be discussed more extensively than is done at present (see lines 1-10, p. 4869). This is also important when discussing the use of the three ratios in the upscaling of instantaneous estimates to daily values.

All the results compare instantaneous fluxes modelled by DTD, TSEB or disaggregation algorithm against 30-minute averaged EC flux measurements containing the time at which the fluxes are modelled (i.e. the satellite overpass time). The disaggregation is also performed using instantaneous flux estimates. The use of mean daily fluxes is avoided altogether in this study by using the constant ratio factors directly during the

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disaggregation procedure (see the first answer), with the assumption that those factors do not change between the times of MODIS and Landsat overpasses. To obtain mean daily fluxes from the constant ratios, the ratios would have to be multiplied by the mean daily net radiation (if EF is used) or mean daily incoming solar radiation (for the other factors). Since the same mean daily net/solar radiation would be used for estimating mean daily fluxes from the instantaneous MODIS and Landsat fluxes, the mean daily net/solar radiation would just serve as a scaling factor. Therefore its use is redundant. An explanation will be added to the manuscript.

On p. 4889 (lines 4-10): The original DTD formulation used the "parallel" resistance network (as expressed by A36) and in the new DTD formulation this "parallel" resistance network has been replaced with a "series" resistance" network (as expressed by A39). This is NOT what has been written in lines 6-10. This problem can be resolved by interchanging series and parallel in line 7 and replacing "latter" with "former" in line 8.

The sentence refers to the original DTD model using many of the TSEB equations presented in appendix A1, but replacing the series resistance network presented in A1 with parallel resistance network. The new (series) DTD formulation is mentioned for the first time on line 1 of page 4890. The text will be reworded to avoid confusion.

Tables and Figures: State more explicitly in headings and captions of all figures and tables whether the data refer to instantaneous values at the time of the various satellite overpasses or to mean daily flux values Rewrite the Table headings for Tables 4 and 5 by stating more clearly that these results have been obtained after replacing tower-based meteorological data inputs with ERA-Interim Reanalysis data inputs. ERA does appear in the heading but needs to be used in an explanatory sentence. The headings of Tables 4 and 5 refer to three approaches used for estimating the constant ration, whereas results for only one (EF) are presented. I assume that the headings for Tables 1-3 where just copied.

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All the results shown in tables and figures show refer instantaneous flux values. This will be clarified. Headings for Tables 4 and 5 will be rewritten to make it clear that ERA data inputs were used and to remove the reference to the three approaches of estimating constant ratio.

4861 13: Explain "which are not at the extremes of their distribution". Can this be rephrased?

This will be rephrased to ". . ., especially for estimates which are not either very high or very low.

4861 25: "with certain heterogeneity". Is it important or not important?

A sentence will be added: This allowed us to evaluate the performance of the disaggregation algorithm in different ecosystems and at different spatial scales of heterogeneity.

4861 3:"in most cases obtaining satisfactory results". A few references to recent studies are needed.

A reference will be added to Guzinski et al., 2013

4861 29: "between the modeled canopy and soil fluxes". Explain the type of (energy) fluxes we are concerned with.

The interaction is between soil and canopy sensible heat fluxes. This will be clarified

4865 13: "Emissivity was linear scaled". You mean it was scaled linearly between NDVI =0.15 and NDVI =0.70?

Yes, it was linearly scaled between fractional vegetation cover of 0 (when NDVI <=0.15) and fractional vegetation cover of 1 (when NDVI >= 0.7).

4865 18: explain "LST of 0 K"

When estimating the upwelling atmospheric radiance and transmittance, the LST was set to 0 K and emissivity to 1 to avoid any emitted or reflected long wave radiation

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signal from the surface. This will be clarified

4865 21: What is meant with "of the Corinne land cover"?

It's a pan-European land cover map. A reference describing the data set will be added.

4866 7: "that it can be treated as blending height temperature". How does spatial resolution impact on vertical mixing.

At regional scale (which is the spatial resolution of ERA data set) the air temperature at blending height is more uniform than the 2 meter air temperature. Therefore it makes more sense to treat the ERA air temperature as the temperature at blending height.

4867 9: What are "thermal-based energy balance models"? Do you mean "energy-balance models in which each of the terms of energy balance can be expressed as a function of temperature"?

They are the energy-balance models which utilize LST and air temperature measurements as the main inputs for deriving the land surface energy fluxes. "Thermal-based" will be changed to "temperature-based" to avoid confusion.

4870 24-25: How does one "weigh each modeled pixel according to its contribution to the overall measured flux"? This needs rephrasing/more explaining.

The footprint model of Detto et al., (2006) estimates the source of the EC measured fluxes as a 2D grid of pixels, representing the relative contribution of each pixel to the total EC measurement footprint, with the sum of all pixels being 1. When evaluating the high resolution fluxes, each modeled pixel is scaled according to how strong is the contribution of its location to the EC measurement. This will be clarified in the manuscript.

4872 10: What is meant with "native" MODIS resolution?

It means that the MODIS data was not resampled after obtaining it form the provider. The word "native" will be removed to avoid confusion.

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4872 23-24: "containing at 75% of the flux footprint weights" needs rephrasing/more explaining.

4872 28: Are we concerned about missing Landsat coverage within the MODIS pixel or about missing Landsat coverage within the tower flux footprint? Or both?

Please see the answer to the second question at the beginning of this document.

26:"scaled by the fraction of the missing footprint" is not clear

This means that if, for example, pixels representing only 80% of EC footprint are present then the aggregated modeled flux is divided by 0.8 before it is compared with EC measured flux. This will be clarified

4873 10-11: "This is true for the dates both in S75 and S100" and "errors for dates in S100" You mean days here, but instantaneous data or daily averages?

Instantaneous data is used in all the comparisons.

4878 25: Explain "Once again points to underestimation of net radiation"

The strong underestimation bias in the modelled turbulent fluxes could indicate underestimation of net radiation. In accordance with suggestions of Reviewer 3, in the revised paper we will also evaluate the accuracy of modelled LE and Rn. Therefore this text will be rewritten.

4879 11: Can this be rephrased?

The statement will be rephrased to: Finally, we discuss the impact on estimated fluxes of using model meteorological data instead of measured data as input.

Figures The figure captions will be rewritten and S75 and S100 referred to whenever applicable.

General editorial comments and corrections

Thank you for a very thorough review of the manuscript. We will reread the manuscript

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and implement all the listed corrections.

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