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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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(1) General comments This well-written and interesting paper clearly falls within the scope of Biogeosciences. The ideas and concepts discussed in this paper are sufficiently novel to warrant publication. The following comments may be considered when preparing the paper for publication in Biogeosciences.

The rationale for the paper has been set out quite well. Firstly, it is noted that a number of empirical and energy balance methods exist for using satellite-based land surface temperatures for estimating land surface energy fluxes. However, considerable uncertainty arises from the use of absolute temperature measurements in such methods.

Several methods (such as the DTD model) have been developed which make use of temperature differences between early and late morning or early afternoon. Secondly, validation of temperature difference models by comparing model predictions based on data from polar orbiting satellites with day-time and night-time overpasses (such as MODIS data obtained with the Aqua and Terra satellites) with groundbased flux measurements obtained with tower mounted sensors has, in most cases, yielded satisfactory results. However, serious discrepancies have been shown to occur over more heterogeneous terrain.

The paper tests the hypothesis that discrepancies over more heterogeneous terrain are due to a mis-match between the footprint of the tower measurements and the size of the MODIS pixel. A disaggregation methodology, which is based on the DisALEXI approach, is applied to fluxes obtained with DTD model from MODIS data. This disaggregation methodology is used with high-resolution Landsat data and is validated with tower-based eddy covariance measurements at two high-latitude sites in Jutland: an homogeneous conifer plantation and a heterogeneous agricultural landscape.

The disaggregation methodology is evaluated as follows: (1) two different versions of the DTD model are used to test the main hypothesis given above and to assess the impact of the type of resistance network (parallel vs. series); (2) three (constant ratio) approaches are compared for up-scaling instantaneous fluxes to daily values; (3) tower measurements are also compared with data obtained with the two-source energy balance (TSEB) model which directly uses high-resolution Landsat data and low-resolution MODIS data; (4) recognizing that there are occasions when the footprint of the flux measured by the tower EC equipment is not fully covered by Landsat pixels, results are split in two sets: S75 includes all dates with at least 75% Landsat coverage within the MODIS pixels and S100 includes all dates with 100% coverage; and (5) the performance of the disaggregation algorithm is tested when local-scale tower-based meteorological observations are replaced by regional-scale ERA- Reanalysis data.

The methods used in this evaluation are clearly outlined but the following issues warrant

some attention

(1) 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)?

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

Results, Discussion and Conclusions are also well structured and presented. However, I draw attention to the following points.

(1) 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.

(2) 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.

(3) Tables and Figures: âĂć 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. âĂć 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. âĂć 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. . .

(2) Minor questions and comments (by page number and line number)

4861 13: Explain “which are not at the extremes of their distribution”. Can this be rephrased? 4861 25: “with certain heterogeneity”. Is it important or not important? 4861 3:”in most cases obtaining satisfactory results”. A few references to recent studies are needed. 29: “between the modeled canopy and soil fluxes”. Explain the type of (energy) fluxes we are concerned with. 4865 13: “Emissivity was linear scaled”. You mean it was scaled linearly between NDVI =0.15 and NDVI =0.70? 4865 18: explain “LST of 0 K” 4865 21: What is meant with “of the Corinne land cover”? 4866 7: “that it can be treated as blending height temperature”. How does spatial resolution impact on vertical mixing. 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"? 4870 24-25: How does one "weigh each modeled pixel according to its contribution to the overall measured flux"? This needs rephrasing/more explaining. 4872 10: What is meant with "native"MODIS resolution? 23-24: "containing at 75% of the flux footprint weights" needs rephrasing/more explaining. 26:"scaled by the fraction of the missing footprint" is not clear 28: Are we concerned about missing Landsat coverage within the MODIS pixel or about missing Landsat coverage within the tower flux footprint? Or both? 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? 4878 25: Explain "Once again points to underestimation of net radiation" 4879 11: Can this be rephrased? Figure 1 Flux towers at the two sites used for evaluating. . .. The red plumes originate. . . and are an example of . . . Figure 2 The satellite input was taken from. . . Figures 3, 5, 6 and 7 Is it useful to refer to S75 and S100 in the legends/captions?

(3) General editorial comments

The manuscript could be improved with consistency in hyphenation. I suggest that the entire paper be checked and corrected to achieve such consistency. Here are some examples which could be implemented: low-resolution, high-resolution data;within-canopy air temperatures; one-source and two-source energy balance models; tower-based and satellite-based observations; small-scale and large-scale observations; 30-min observations; 3-hourly data; night-time and day-time observations; cloud-free days; regional-scale observations; self-preservation

The entire manuscript needs to be checked on the use/mis-use/absence of definite articles (the) and indefinite articles (a, an). I have given a few examples below.

(3) Corrections (by page number and line number)

4857 Title: ..in a heterogeneous agricultural landscape and a coniferous plantation. . .. Coniferous or conifer? 4858 10: the fluxes 14: the first one. . . and the second one. . . 18: eddy covariance 20: but not perfect. 22: The latter. . . 4859 2-4: The reliable

estimation of. . .requires that. . .matches the.. 3: the model's spatial resolution. . . 8: ..in the order of 10:.. in the order of.. 11: ..on a regular basis.. 18:..due to the simple assumptions and.. 4860 5: the temperature difference 13: compared to (or with) 17: of the fluxes' spatial patterns 4861 6:.. and the size of the MODIS pixel.. 15: in large measure.. 25: which in contrast to VOU.. 4862 13: We conclude with the discussion in Sect.5 17: on the Jutland peninsula (or: in Jutland!) 22: Both sites.. 23:..with a temperate maritime climate.. 25: ..of the landcover type.. 4863 11: 30-min 15-16: The only setting that was different. . .. was. . . 21:..to further quality control.. 27: Both towers also have sensors. . . 4864 11: The use of MODIS data as input. . . 12: Briefly, the . . . 21: All Landsat data came from cloud-free. . . 4865 7: that the Landsat 7 has.. 26: We have therefore also tested. . . 4867 2: Priestley 11: This makes the models highly.. 16: couples the surface energy balance.. 4868 1:..which obtain data (or: make observations) over 2: ..during night-time and day-time overpasses. 12: ..on the DisALEXI algorithm.. 18: ..with a high-resolution sensor.. 24: necessarily 4869 1: ..of DisALEXI consistency between.. 21:.. It can therefore be assumed that.. 27:.. the constant ratio (CR), which is assumed to remain. . .,. . .is calculated.. 4870 2: set at some plausible initial value. 4871 2:.. with a standard deviation . . .. on the standard deviation of. . . 5: "Series" 11:..In the "parallel" DTD version. . . 12:..while in the "series" DTD version the new. . . 17:. . ..is reduced by a factor of almost 4. 4872 8: ..in the remainder of this study. 19: The latter fluxes. . . 21: A statistical comparison is . . . 4873 4:.. different from.. 9:.. and an increase. . . 4874 13:..In the case of GLU.. 18:.. for the correlation coefficient. . .for low-resolution 4876 21:..with a strip of missing pixels.. 4877 14: "it raises the possibility that" 4878 8:.. leading to its underestimation.. 29:. . . mounted on a tall tower.. 4879 5:.. they might still impact on.. 4881 12:. . ..at both evaluation sites.. 4883 22:. . . the von Karman constant (or von Karman's constant) 23:..Monin-Obukhov.. 4885 2:. . ..is a constant with a value of.. 4886 11:..Priestley.. 14:..Priestley.. 4888 5: ..and the equation A31 is based. . . 15: ..9.8 ms-2 4889 8: ..the latter.. 22:.. can also be. . . 4890 1: .. In this study a new formulation for. . . 4891 3:.. and solar noon.. 11:. . . Similarly to TSEB, if LEs,1 is negative,. . .

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