

Interactive comment on “Evapotranspiration over agroforestry sites in Germany” by Christian Markwitz et al.

Anonymous Referee #1

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This paper presents ET measurements from paired monoculture/agro-forestry sites throughout Germany. The results indicate insignificant differences in ET between the land use types, which appears to be a positive result. The writing is adequate, but I personally feel that the document overemphasizes the statistical comparison between the paired sites to the extent that the important message of the paper is obscured. The content of the paper is fine, but the text needs further refinement.

Comments on paper:

Page 1 line 23: Direct comparison of ET between wet and dry years is not very relevant because the available energy is likely different between the two years.

Page 1 lines 16-26: This is the most important point of the paper. However your

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description does not speculate or give guidance as to whether you expect higher ET at the AF or MC locations, no hypothesis.

Page 2 line 8: You note that SRC are comparable to monoculture (forestry) but you don't indicate what aspects are comparable - are you referring to energy partitioning and water use?

Page 2 Intro: Most of your references are relatively recent, you might gain some insights by reviewing earlier work. See references in Cleugh.

Page 3 line 10: The ECEB method is not really limited by closure of the energy budget because this is the default assumption for ECEB. It is, however, limited by the accuracy of your estimates of sensible heat flux, net radiation, soil heat flux and change in storage terms.

Page 3 line 20: Why do you partition the residual energy budget between just H and LE and not between H, LE and G - or possibly even Rn

Page 3 line 21: I would suggest being more specific in your hypothesis. Specify short-rotation coppice agro-forestry, as your results may not extend to other systems.

Page 5 line 4: How did you know if precipitation data were missing?

Page 5 line 10: Did you use the precipitation data from the AF plots? and if so how did you use them?

Page 7 equ 4: Technically, this conversion gives you units of mg/m² not mm/30 min. (assuming your lambda value is using milligrams and not the more usual grams. This needs to be explicit to avoid readers from incorrectly applying this equation. (i.e. give units for your variables)

Page 8 line 9-10: This sentence needs to be fixed. Also, it is an assumption that lack of energy budget closure reduces ET. That assumption is not necessarily true.

Page 10 line 2: Your Big-Leaf assumption may be appropriate for the MC sites but less

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so for the AF sites, can you address the potential effects.

Page 10 Equ 11: Here and elsewhere in the paper you use 'lambda' as the latent heat of vaporization but in the text you use 'L'. Best to use one or the other, not both.

Page 10 line 9: is 'ppp' a variable, if so it should be shortened to a single character.

Page 10, equ 14: don't use VPD as a variable name, reduce it to a single character (e.g. 'D', or a single character variable with a subscript or superscript (e.g. 'e_D')

Page 11 line 2-3: Did you account for wind direction. The AF site is inherently non-homogeneous, and similar to other row-structured crops may have strong directional dependencies.

Page 11 sec 3.1: This information might be more succinctly incorporated as a table - only referring in text to any atypical conditions.

Page 14 line 24-25: Water vapour concentrations are not a good indicator of spectral response - many other factors come into play.

Page 15 fig 4: Why is there no nocturnal data for some sites?

Page 17 sec 3.4: Instead of using "LE from EC", "LE from EC_{LC}", "LE from ECEB", might I suggest using subscripts LE_a = LE from EC LE_b = LE from ECEB LE_c = LE from EC_{LC} It will make reading the paper much easier.

Page 20 line 6-14: This is really interesting. I would cut down on the amount of stats provided and focus on the underlying concepts of what be causing this - which obviously is on scales much bigger than the individual sites

Page 20 line 17-18: This seems inconsistent with your preceding paragraph.

Page 20 sec 3.4.3: Not so sure about the usefulness of this section. As presented it is a simple algebraic exploration assuming linear relationships. In reality, changing one or more the components by +/- 20% may have non-linear effects on the other compo-

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nents, which can not be accurately captured by the your current analysis method.

Page 22 line 15: This is perhaps expected, by definition R_n is the sum of the other components.

Page 22 line 27-30: Is it correct that this is an assumption and you did not measure evaporation and transpiration separately.

Page 24 sec 3.5.2: Even though ET was measured by EC only for campaigns, it might be useful to compare sums of ET by all three methods for those campaign periods.

Page 27 line 3: how do you get a displacement height of 7 m with a canopy height of 5 m?

Page 27 line 7-8,13-14: Is these relationship inherent from the derivation of canopy conductance from ET?

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