

Interactive comment on “Responses of energy partitioning and surface resistance to drought in a poplar plantation in northern China” by M. Kang et al.

Anonymous Referee #2

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General comments

1. The researchers investigated an important topic related to the exacerbation of drought conditions due to large-scale afforestation of high water-use hybrid poplar in China. Four years (2006–2009) of continuous eddy-covariance flux and climate measurements were made above and within the Daxing Forest Farm located in Beijing, China. This appears to be a valuable dataset with fluxes achieving high energy balance closure. The climatic controls on surface resistance, calculated from the inverted Penman-Monteith equation, and the subsequent partitioning of net radiation into turbulent energy fluxes (latent and sensible heat) were discussed.

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2. The authors need to justify using the term “drought” when there was one year with below-average precipitation followed by two years with above average precipitation and a final year with below-average precipitation. Perhaps it should be described as four years with seasonal dry periods.

3. A further concern is that adding irrigation of 86 and 195 mm in the first and fourth years brings all four years above the long-term mean precipitation. Was this irrigation scheduling part of regular stand management? If so make it clear. Considering these amounts of irrigation water that were applied, distinguishing between “dry” and “wet” years becomes very difficult. The question arises: if the hybrid poplar had not been irrigated in 2006 and 2009 (the years with low precipitation) how would that have affected the conclusions?

4. It was found that the latent heat flux accounted for 62% and 53% of the available energy flux in the wet and dry years, respectively. The authors also report this in terms of the ratio of LE to LEEq (0.81 and 0.68, respectively). Certainly the fraction for the wet years is higher than for the dry years but it is not a large difference. Unfortunately it is not known how much lower the values in the dry years would have been with no irrigation. Could the same irrigation totals have been applied in different amounts at different times during the year resulting higher latent heat flux totals and presumably higher growth rates? This is an important question since the soil is coarse textured with limited water holding capacity. These issues need to be addressed in the introduction to the paper when addressing the objectives and the experimental design.

5. The conclusions need to be made clearer. For example, the authors concluded that “partitioning of available energy to latent and sensible heat differed significantly between wet and dry years” but also concluded that “overall low LE/LEeq and high surface resistance values in all years indicated that the study area was under water stress even in the wetter years”. The implications of these somewhat different statements need to be explained. The final concluding statement (repeated in the Abstract) needs to be rewritten. It states “In conclusion, the dry surface conditions dominated in this

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poplar plantation ecosystem regardless of soil water availability suggesting that fast-growing and water use-intensive species like poplar plantations are poorly adapted for the water limited region". I suggest that the authors make it very clear what is meant by "dry surface conditions". Are they referring to surface resistance or surface soil moisture content? What criterion is being used in determining that conditions are dry? How do these conditions lead to the conclusion that "fast-growing and water use-intensive species like poplar plantations are poorly adapted for the water limited region"? If less water is used (low LE), and the trees remain healthy (or are the trees dying?), doesn't this suggest they quite well adapted?

6. There is considerable poor grammar and composition which should be corrected by the native English-speaking co-authors.

7. After addressing the above concerns and the specific points below, the paper would make a valuable contribution to ongoing international hybrid poplar water use research.

Specific comments

1. Page 347, Lines 6 and 8: In line 6 you refer to "canopy resistance" and then to "bulk canopy resistance" in line 8. Throughout the paper and within equations you refer to "surface resistance". I think you should change both resistances mentioned in lines 6 and 8 to surface resistance to remain consistent.

2. Page 347, Line 6: Insert "fluxes" after CO₂.

3. Page 347, Line 7: What is meant by "true" ecosystem functions? Make this clearer.

4. Page 347, Line 11: I suggest using " R_n calculated as net radiation (R_n) minus soil heat flux (G), was partitioned into" rather than "(Net radiation R_n minus Soil Heat Flux, G)

5. Page 347, Line 12: Define what is meant by climatological drought.

6. Page 347, Line 13: 62%. I'd suggest this be written as 0.62 to be consistent with

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other ratios you've reported.

7. Page 347, Line 21: Start a new sentence "The LE/LEeq...".

8. Page 347, Line 21-24: This last sentence is not clear enough (dry surface conditions vs soil water availability...?); should be rephrased to make it clearer.

9. Page 348, Line 14: I suggest using "increased" rather than "a growing".

10. Page 349, Line 24-25: I suggest using "resistance" throughout the paper rather than "conductance" to keep consistent with equations and maintain clarity. An advantage of conductance is that tends to be proportional to LE, whereas resistance is very nonlinear with LE, e.g., you get a wide range of high Rs values that correspond to low values of LE.

11. Page 350, Line 23: Is the wind coming from OR going toward "the southern and northwest directions". Usually wind is described in terms of which direction it is coming from.

12. Page 352, Lines 7-8: It should be indicated at which depths the CS616 probes are placed. Were they all placed at 50 cm?

13. Page 354, Line 6: Gu et al. (1999) does not present the energy balance ratio in summation notation as you do in Eq (3). If the summation notation is kept, the interval over which the summation is calculated should be specified. 24 hr, half-hourly, etc.

14. Page 355, Line 16: "Equilibrium" should not be capitalized.

15. Page 357, Lines 2-4: "Long-term drought stress ($REW < 0.4$) occurred during period in late growing season of 2006 and 2009, in spring in 2007 and 2009, but not at all in 2008 (Fig. 2a-d)." would read better as "Long-term drought stress ($REW < 0.4$) occurred during periods in the late growing season of 2006 and 2009, the spring of 2007 and 2009, but not at all in 2008 (Fig. 2a-d).

16. Page 357, Line 9: The drought stress periods for 2007 should be introduced in a

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consistent manner, formatted as they were in on Page: 357 Line: 7 for 2006 (#2_06).

17. Page 358, Line 18-19: I don't think H can be a dominant factor controlling Rn. Explain.

18. Page 358, Line 21-22: "LE/(Rn – G) was significantly higher in 2008 (64.8 %) than in 2006 (57.1 %), 2007 (60.3 %) and 2009 (50.4 %)". These aren't really large differences.

19. Page 358, Lines 25-27: "The dominant part in energy partitioning" should be changed to "The dominant turbulent energy flux" throughout the paper.

20. Page 358, Line 27: "dominate" should be "dominant".

21. Page 360, Line 4: "Difference" should be "differences" and "year" should be "years".

22. Page 360, Line 14: "dramatic" should be "dramatically".

23. Page 360, Line 4: The sentence "Drought is also an expected source of interannual variation in Rs (Wilson et al., 2002b)." seems redundant and should probably be removed given you make this apparent in previous sentences.

24. Page 360, Line 14: "The mean ΔT_e for the studied years were 0.41, 0.46, 0.43 and 0.39". The magnitudes of these numbers are very similar.

25. Page 360, Line 17-18: The drought periods within the brackets (#1, #2, etc.) should be consistent with the format in which they were introduced on page 357.

26. Line 362, Line 16-19: "Our current findings corroborate that hypothesis." But you report you observed low rates of evapotranspiration (LE) on Page 364, Line 17. Regarding the statement "Growing poplar trees by irrigation in a water stress region is not sustainable, and the productivity of the plantation was water-limited even during the wetter years, as indicated by the resistance terms." What is your evidence of that productivity was limited by water supply? Why would there be an adverse regional region's groundwater reserves if evapotranspiration is low?

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27. Page 362, Line 22: quantifies?
28. Page 365, Lines 2-4: Delete sentence. It is unnecessary.
29. Page 365, Line 6: I think they are really seasonal drought periods rather than long-term drought periods.
30. Page 365, Lines 18-21: See General Comment 5.
31. Page 379, Fig. 1: Insert “respectively” after “brace”.
32. Page 380, Fig. 2: Were the soil moisture sensors installed at a location that received all irrigations? I see only a small effect of the 30 mm irrigation on DOY 290 in 2006. Is that because the moisture sensors are so deep?
33. Page 381, Fig. 3: Plotting 5-day running means or weekly numbers will likely show clear distinctions among years by reducing crowding.
34. Page 382, Fig. 4: Renumber the y axis with the inclusion of “0”. I’d suggest -2, 0, 2, 4, 6, and 8.
35. Page 383, Fig. 5: 5-day means or running means would make the 4 years more distinguishable from each other.
36. Page 384, Fig. 6: This figure is virtually the same as Fig. 9b. Delete Fig. 6 or Fig. 9b.
37. Page 386, Fig. 8: Unexpected values for the Bowen ratio and $LE/(R_n - G)$ for $WS < 50$ mm. Specify for what depth of the root zone does the 50 mm apply (1 m depth?). How is WS calculated when the sensors are all at 50 cm depth?
38. Page 388, Fig. 10: It can be shown that the data can be separated (stratified) into increasing values of R_i with higher values for dry years.

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