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Discussions**

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Interactive comment on "Leaf area controls on energy partitioning of a mountain grassland" *by* A. Hammerle et al.

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Summary

The papers present six years of eddy covariance data of a managed hay meadow. This paper is partly useful because it contributes to our understanding of managed land surfaces, which are generally ignored in land surface models in common use. In particular, it is challenging to maintain data continuity in managed fields because the management often dictates the flux tower be removed for field management.

The authors present results that show that management is a much more important control on land surface energy balance than driving factors such as light or temperature. This relevant is relevant for modelers because it implies that for managed systems

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it is at least as relevant to accurately model the management itself compared to, for example, accurately modeling light use efficiency in the plants.

General Comments

Overall, the paper could be tightened by removing the analysis of Omega, because it does not really add much to the results, and is hardly discussed. Perhaps replace this with simply a comparison of ga and gs. Please add a paragraph, even if it is short, giving specific details on how you calculated ga and gs. The references you point to are somewhat obscure or unavailable to many readers.

I don't know that LEeq suits your purposes. It is counter intuitive to get values of LEeq over 1, since LEeq is supposedly representative of evaporation from an open pan (into a saturated atmosphere). Perhaps calculate a pan evaporation using E=ga*(qsat(Tpan) - qair), which is essentially driven by vapor pressure deficit (assuming Tpan=Tair). Perhaps remove this comparison altogether because it does not seem to add alot.

I have a different interpretation for Figure 7. You present VPD as driving the Bowen Ratio, but if the moisture in the air essentially originates from evaporation, you may as well show a plot of VPD (Xaxis) vs Bowen Ratio (Yaxis) or VPD vs LE/Rn. This is consistent with boundary layer feedbacks in which a low bowen ratio leads to a cooler, moister, shallower boundary layer, partly due to the surface evaporation, and partly due to reduced entrainment of warm dry air aloft (see, for example, Betts, Helliker, Berry (2004) JGR).

I thought you spent too much time discussing G, when the more interesting result in my opinion was the saturating relationship of LE/Rn with increasing LAI. What determines the attraction toward this particular value of LE/Rn? I think partly this is explained by the modification I proposed for figure 7: the evaporative gradient becomes lower as LE/Rn (and LAI) increase, showing a stable feedback. Also, as LAI increases, photosynthesis should saturate (since the lower leaves are in the dark), so additional leaf

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area would not contribute additional transpiration (under the assumption that stomatal conductance is regulated to optimize transpiration with respect to photosynthesis). I think that this linkage between LE/Rn and LAI is at the heart of your intent with the paper, focused as it is on leaf area controls on energy partitioning, and it would serve the paper well to consider this more thoroughly.

Specific comments: p 3609, eq 2. If the surface is perfectly black (epsilon=1) would there be no absorption of incoming longwave? You should replace L*(1-epsilon) with simply L*epsilon.

p3613 line 9. Is Rg shortwave down only? State which terms of the radiation budget are represented by Rg. Related question: how was albedo measured? I could not find this in the ms.

p3615. I don't like the Twine correction. I understand that this probably does not affect your results much, but it should not be accepted as standard practice.

p3619. line 1. "Detrending for GAI (Fig. 6)" is ambiguous, particularly since Figure 6 does not depict Tair or LE/Rnet.

p3621. lines 3-4. You cannot possibly mean to suggest that soil texture changed over the course of this experiment. Strike this line.

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