Hydrol. Earth Syst. Sci. Discuss., 11, C6266–C6274, 2015
www.hydrol-earth-syst-sci-discuss.net/11/C6266/2015/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Diagnosing the seasonal land–atmosphere coupling strength over Northern Australia: dependence on soil moisture state and coupling strength definition" *by* M. Decker et al.

M. Decker et al.

m.decker@unsw.edu.au

Received and published: 1 February 2015

General comments: The authors present an offline model based assessment of connections between soil moisture, surface fluxes and LCL height with a single model in two drainage configura- tions and a suite of different atmospheric forcings. I think the authors' conclusions about the dominant role of transpiration over surface evaporation for this model in this study are well demonstrated. I think the authors have a point about the differentiation between surface and root zone SM, and between surface C4384 evaporation versus transpiration processes. However, I think the notion of coupling is not adequately demonstrated here; it has not been demonstrated what is

the cart and what is the horse. Too much has been presumed in this offline LSM study. Some things can be addressed diagnostically, as recommended below, but without a fully coupled study, and more realistic models, they must be very careful about making conclusions about coupling in nature. First, the word "coupling" connotes cause and effect. In particular, "land-atmosphere coupling" suggests the return leg of the feedback loop where the land surface state influences the atmosphere. Like correlation, the "Kendall-tau" metric does not prove cause and effect but points out correspondences. This distinction needs to be made clearly in this paper. That phrasing is used a bit in the discussion, but needs to be the central tone of the paper. It is possible (and has not been demonstrated otherwise here) that the correspon- dences between LCL and EF or LCL and SM are not cause-effect but effect-effect. Wet season humid conditions driven by moisture advection will lower the LCL without any land surface feedback. In a monsoon, the LCL is at its lowest level during the active phase rainy spells that correspond with adequate soil moisture (caused by the rain), which allow larger evaporation rates in an otherwise moisture-limited, energy-plentiful regime. The weaker correlations for CTRL (which does not drain well) in the wettest areas (sometimes even positive) keeps ET high (fig 2) and thus reduces day to day variability in EF; this could mean ET in CTRL is less responsive to precipitation, as opposed to the LCL being generally responsive to ET. The possibility that the local water cycle is all atmospherically controlled needs to be eliminated before the existence of coupling can be declared here. Perhaps Kendall taus with daily precipitation and 2m humidity need to be examined as well. What it comes down to, which could be evaluated offline, is whether, for CLM, the ET is con- trolled by SM or humidity? Since humidity deficit determines both LCL (absolutely) and latent heat flux (partially via both stomatal resistance for transpiration and the humidity C4385gradient term for direct evaporation), it should be that any loss in explained variance between the two, for which humidity is the main factor, should be taken up by the soil moisture availability. These runs are not coupled; the LSM is only driven by specified meteorology. Thus, any diagnosis of coupling is predicated on assumptions about the processes that have not already been adequately demonstrated for this place and time of year. Ultimately, in an uncoupled setting, estimates of such metrics must be based on a robust demonstrated process for coupling, which I think has not been demonstrated for monsoon regions in the wet season in general, and definitely not in this study. Thus, one needs to be careful. This has always been a difficult problem, to establish the effect of land-atmosphere feedbacks in monsoon climates where there is such a strong background of large-scale forcing and circulation. Most such work has focused on India, secondarily on West Africa - that work is not cited here (some studies cited here do address that, e.g., by Ferguson, Taylor, etc., but those aspects are not discussed in this study). Lastly, some of what the authors uncover are clearly model inadequacies in CLM (see specific comments below) - I would like to see these discussed more in Sec 5. I would say if the authors would like to maintain the theme of a "coupling" evaluation, major revisions including more analysis are necessary to justify it. On the other hand, if the tone were changed to showing "correspondences" with the focus shifted squarely to the differing role of subsurface soil moisture and transpiration on the demonstrated relationships (which is the current emphasis in the conclusions), then the revisions are more editorial in nature and rather minor.

Response: We agree with Pauls comments and accept that the use of a statistical measure in offline experiments cannot conclusively prove cause and effect and therefore coupling. While statistical association between SM and LCL is indicative of coupling it doesn't demonstrate the relationship is cause and effect. To remedy this shortcoming we have re-focussed the manuscript on the association between SM, EF, and the LCL rather than coupling.

Specific comments: Throughout: The use of the term "observations" for reanalysis products, GLDAS and the flux estimates (and to a lesser extend the AMSR-E retrievals) is bothersome. "Ob- C4386 servationally based estimates" would be better. There are no direct observations in these data for surface fluxes, and even the state variables are measured sparsely in this region.

Response: We have replace the term “observations” to be either estimates or observationally derived estimates throughout the manuscript when we are referring to GLDAS, MERRA, or AMSR-E. The manuscript now reserves the term observations for use with the flux tower data which we have added to the paper.

P10433 L17: How do you mean the word "decadal" here? Decadal usually means multi-year time scales; that is quite a jump from diurnal.

Response: We removed this sentence as it is out of place considering the context of the manuscript.

Sec 2.2: Are there any stations or soundings to validate the meteorological data in this region? I would feel a lot better about it if so, especially if they are independent from the assimilation stream. Likewise, are there any flux tower or eddy-covariance measurements of latent heat flux that could be used to validate ET? How about soil moisture measurements to validate the variability and profiles of soil moisture, even at only one location?

Response We have added measurements from two flux tower data sites to the manuscript. The soil moisture and ET observations are compared directly against the simulations, the AMSR-E data, and the gridded ET products. The two tower sites are also now used to compute Kt for EF-LCL, SM-LCL, and for SMrz-LCL for the site that has SM measurements at several depths.

P10437 L23: MERRA has some well-documented hiccups in its time series when new remote sensing data come into the assimilation stream, especially affecting moisture variables (humidity and precipitation) at lower latitudes. It seems like this might have significant impacts over your study area - impacts that cannot be removed by removing linear trends. Have you examined this?

Response: The MERRA data was evaluated when the forcing dataset was originally created in 2012. The data were compared against the AWAP data (a gridded daily precipitation product from the Australian Bureau of Meteorology) and no obvious discontinuities were discovered.

. Sec 3.1: It is clear why afternoon LCL is used for the Kendall-tau calculations, but why is morning soil moisture so critical? Is the index really much different if you use afternoon values at the same time as maximum LCL? Response: The morning time SM is used for several reasons. The first is that one cannot argue the LCL is controlling the SM when SM is sampled prior to the LCL. This doesn't eliminate the possibility that SM and LCL are controlled by an external factor (and thus both effects with the cause unexamined) but prevents direct control of SM by the LCL (through near surface humidity). Secondly, if SM is controlling the LCL one would expect SM to decrease during the day such that the mean afternoon SM will be smaller than the morning time SM. As the hypothesis is that high SM causes low LCL, "observing" SM in the afternoon may not be indicative of the relationship.

P10441 L22 and Fig 2: Normalized by what? Standard deviation? Since your correlation index Kendall-tau is non parametric, why use a normally distributed variance metric? Response: We have included an explanation that the SM in Figure 2 is normalized using the first two moments. The normalization follows previous work (Koster et al. 2006) that demonstrated how land model simulated SM varies substantially between models unless it is normalized. To highlight the effect of comparing SM with and without normalization we have included a new figure (Figures 4a and 4c) that directly compare SM from the simulations, AMSR-E, and observations at two tower sites.

Fig 2: What is the X axis? Presumably these labels are months, but there is no rela tionship to the calendar given. Response: We labeled the X axis to indicate that it is the date.

Fig 2b: How does this compare to the mean? Certainly there must be a lot of spatial variability. And what time of year is shown in Fig 2 - one season, both seasons? Finally, C4387the color scale is not good - shades on both sides are not well differentiated from

each other. Response: We have included the time period of the comparison in the Figure 2b caption as well as changed the color scale to highlight the spatial pattern of the differences. The differences in the mean SM between the simulations and AMSR-E seen in the new Figures 4a and 4c can be seen in Figure 2b near 12oS 131oE.

Discussion of Fig 3: Also point out DRY does better in the wet season, as CTL fluxes are too vigorous here. I can think of many possible causes; maybe there is too much infiltration in the wet season, the precip forcing could be too smooth in time, or CLM may be tuned to transpire too readily. Are there discharge data in this region to validate runoff? What is the underlying geology? I imagine there is not much karst there, so standard LSM drainage parameterizations should be able to handle the vadose zone flow adequately. Response: We have included the sentence "The overestimation of DJF ET compared to the gridded product is much more pronounced for the CTRL simulations (Figure 3a) than the DRY simulations (Figure 3c)." to highlight that DRY is closer to the gridded product in DJF than CTRL. The new Figures 4c and 4d show that the wet season ET from DRY and CTRL is not consistently over estimated at either flux tower site and is underestimated by 30-40 Wm-2 for the Adelaide River site.

Sec 4.2: Just an aside comment: I would love to see someone actually measure soil moisture profiles here. Worldwide there are very few such measurements in monsoon regimes. v We have now included SM observations from two flux tower sites. Unfortunately, while the Howard Springs site now measures SM at various depths, measurements only go back a few years and are not continuous.

P10443 L14-15 "...indicating that the surface evaporation is the dominant ET mecha nism." How do you reach this conclusion? Please elaborate. You explain the transpira tion argument below but not the surface evaporation argument here. Response: A previous version of the manuscript included a figure that showed the fraction of ET from transpiration for DRY and CTRL from SON and DJF. Both DRY and CRTL show ET comprised of 10-35% transpiration during DJF with the result being soil or canopy evaporation. We added the following to explain this point "The mean DJF ET is similar between CTRL and DRY, with differences between the two only 10-20 Wm-2, corresponding to roughly 10-20% of the mean value. The fractional contribution of transpiration to the total ET during DJF is roughly 10-30% for both DRY and CTRL (figure not shown) indicating that the surface evaporation is the dominant ET mechanism. "

P10443 L16-17: Two "however"s in a row. Response: Removed the first however.

P10446 L1: 90% of water uptake capacity in the top 1m is almost certainly unrealistic, especially in a wet/dry regime where the woody species must have deep roots to survive the dry season. They will tap the shallow moisture during the wet season when it is easy. Such dynamic root responses are not part of CLM or most other LSMs, and are a shortcoming for simulating transpiration in semi-arid and seasonally arid biomes like this. Response: We agree that the actual depth of water removal in the simulated system is not realistic, as some Eucalypt species have been shown to have rooting depths in the tens of meters. We have further acknowledged this fact and added the following to the manuscript. "Applying Equation (4) using SMrz imposes a different set of problems, as the rooting depth is model dependent and generally only approximately known. There is substantial evidence that eucalypts have rooting depths exceeding 20 meters, however neither CLM4 or the direct observations in this study extend that deep. Due to these limitations, SMrz is computed as the weighted mean of the SM observations at 10, 40, and 100cm for the Howard Springs site. We assume that the SMrz consists of the soil layers between the surface and a depth of 1m, as greater than 90% of the prescribed roots in CLM4 are within 1m of the surface (Oleson et al. 2010). This assumed rooting depth is consistent with the model formulation but not realistic given the rooting depths of eucalypts."

P10446 L19-26: Fig 8 seems tacked on; the figure is merely described but the conse quences are not explained. Response: We have added discussion of the consequences of the previous Figure 8 (now Figure 9). We now explicitly state that the use of multiple estimates of the LCL may cause Kt to be uncertain so the uncertainty is examined.

Sec 5: I would say the unrealistically wet SM profile in dry season in CTRL makes up for the overly shallow rooting profile in CLM for this biome; the right answer is reached C4388 for the wrong reasons. By removing one of the two compensating errors (in the DRY case) the results deteriorate. Response: We agree that the rooting depth within the model is much more shallow that what the rooting depth physically is. To acknowledge this point we have added "Within the model, the soil column-groundwater interactions parameterized in CTRL inhibit the large, ET limiting SMrz reduction present in DRY. In reality the inability of DRY to maintain ET during SON may result from the shallow rooting depths assumed in CLM4. The depths are substantially more shallow than the rooting depths of eucalypts. Realistic rooting depth profiles (reaching 20 meters) and corresponding soil layer depths may negate the impact of the parameterized soil column-groundwater impacts current in CLM4."

P10447 L13-14: The similar Kendall-tau between EF-LCL despite model configuration, ET or SM is an indicator that the atmosphere (humidity) is in control, not the land surface state. Response: The figure isn't shown however the SM-EF Kendall-tau is statistically significant in both DJF and SON for DRY and CTRL. Therefore the SM is always imparting significant control on ET in the modeling system. Similarly, the Kendall tau of SMrz-LCL demonstrates that SM is significantly associated with LCL, just not always SM1. P10447 L18: Here the term "coincidence" is used - this kind of neutral verbiage should be used throughout unless "coupling" can be more rigorously demonstrated.

P10448 L1-2: Very little area looks "positive" to me. This might have to do with the lack of magnitude dependence in the Kendall-tau, discussed by the authors. That is why indices like the terrestrial coupling index were developed (Guo et al. 2006, Dirmeyer 2011). Response: To aid in the readers ability to follow the text we have included two locations as examples of both positive and negative association. While several different terrestrial coupling indices exist, the purpose of this manuscript is to simply highlight that measures of association must be treated carefully as the results are dependent on how one specifies the SM term.

P10448 L5-8: The changes from SON to DJF are still the same for SMrz as for SM1, just a bit weaker. Response: We have explicitly added the location of a region where the results from SMrz and SM1 differ.

P10448 L12: The study of Jasechko et al. (2013) has been strongly refuted by several subsequent papers (e.g., Coenders-Gerrits et al. 2014, Sutanto et al. 2014, Wang-Erlandsson et al. 2014) and they have subsequently backed off from their original claim (Schlesinger and Jasechko 2014). Also, Haverd et al. (2013) estimate half of Australia's ET is bare soil evaporation. Response: We have removed the Jasechko et al. (2013) reference and now cite Coenders-Gerrits et al. 2014 and Schlesinger and Jasechko 2014. The manuscript was originally written prior to Jasechko et al. 2013 being so thoroughly refuted and is now updated to include the new papers.

P10448 L16-17: I would say this study is likewise limited. Referring also to Eq 1, this study neglects that ΔPBL can also occur due to large scale non-local influences, which strongly drive the EFatm term in monsoon regimes. Response: We were trying to explicitly point out limitations of our method. We have altered the opening sentence of the paragraph to state that we are discussing our study as well as others that utlize similar methods.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10431, 2014.