

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.

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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 configurations 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

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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 correspondences 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 controlled 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

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gradient 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.

Specific comments:

Throughout: The use of the term "observations" for reanalysis products, GLDAS and the flux estimates (and to a lesser extent the AMSR-E retrievals) is bothersome. "Ob-

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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.

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

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?

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?

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?

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?

Fig 2: What is the X axis? Presumably these labels are months, but there is no relationship to the calendar given.

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,

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the color scale is not good - shades on both sides are not well differentiated from each other.

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.

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.

P10443 L14-15 "...indicating that the surface evaporation is the dominant ET mechanism." How do you reach this conclusion? Please elaborate. You explain the transpiration argument below but not the surface evaporation argument here.

P10443 L16-17: Two "however"s in a row.

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.

P10446 L19-26: Fig 8 seems tacked on; the figure is merely described but the consequences are not explained.

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

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for the wrong reasons. By removing one of the two compensating errors (in the DRY case) the results deteriorate.

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.

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).

P10448 L5-8: The changes from SON to DJF are still the same for SM₂ as for SM₁, just a bit weaker.

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.

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 E_{Fatm} term in monsoon regimes.

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