

Reviewer #2:

Summary:

The authors have revised their paper mostly by reframing their analysis and playing down the “coupling” aspect of their study in the presentation and discussion of their results (talking of “correspondence” instead – cf changed title).

Yes – these changes were made in response to Reviewer 1’s advice.

They also added some more observation data from flux towers (I believe as a response to other reviewers’ comments). I essentially still stand by my first review: mostly, the experimental set-up is not well suited for the intended analysis. Part of the analysis should be dropped, and part, while okay, could be attained more simply. I also question some of the claimed corrections. Please see below for details.

We do not agree (and we note Reviewer 1 does not agree either). We acknowledge that the Reviewer thinks we should omit part of our analysis but this advice is in conflict with Reviewer 1 and clearly our views. We have attempted to further clarify our paper, and add further material to resolve the reviewers concerns, as you would expect.

In my first review I essentially pointed out that because the authors are using an offline land-model, they could not really investigate land-atmosphere coupling in their study: the land surface states and fluxes simulated by their land model (CLM4) over Northern Australia did not feed back on the atmosphere, so they were not really investigating “coupling” when looking at correlations between simulated soil moisture, or evaporative fraction, and LCL derived from the forcing dataset(s).

More specifically, there were two aspects in the study:

i) Whether including root-zone soil moisture (SM<sub>rz</sub>), versus surface layer SM (SM<sub>1</sub>), in the statistical metric the authors use to evaluate soil moisture-atmosphere coupling, matters for the diagnosed coupling;

ii) Whether the mean background soil moisture content in the root-zone (varied between two simulations using different configurations of CLM4) matters for the diagnosed coupling.

I pointed out that, in particular, they could not investigate question ii), since the atmosphere does not “see” the changes in land model configuration.

The authors have tried to address this general problem (lack of L-A coupling in an offline land model simulation) by reframing their study as an analysis of the statistical “correspondence”, or “association”, between the land (SM) and the atmosphere (the LCL) – a correspondence that they indicate is necessary but not sufficient for actual land-atmosphere coupling to occur.

Yes – that is correct. We addressed these issues to specifically (Section 6)

This may be fine for question i) – although there are still a few places in the manuscript where the authors speak of “coupling” that should be edited (e.g., p.20 l.561).

We have removed the word coupling from that location. Our results are described using correspondence and association throughout the manuscript. However in several locations we retain the use of the word “coupling” where it is appropriate. For example, in Section 6,

“The wet season onset (SON) shows strong EF-LCL association that contrasts the weak SM<sub>1</sub>-LCL association demonstrating the SON coupling is not properly characterized with SM<sub>1</sub>. “

Here we define our results as the EF-LCL and SM<sub>1</sub>-LCL associations and highlight that they contradict each other during SON. While our methods doesn't allow the direct evaluation of coupling, it follows from the contrasting association between EF-LCL and SM<sub>1</sub>-LCL that the SM<sub>1</sub>-LCL coupling strength won't be adequately described by SM<sub>1</sub>.

Of course, this makes for a weaker main point of the study. Essentially, the story boils down to ET in SON being mostly transpiration and being driven by SM<sub>rz</sub> instead of SM<sub>1</sub>, in contrast to DJF where ET is mostly evaporation and therefore SM<sub>1</sub> can be used to diagnose land-atmosphere “correspondence”. I have two comments here: first, I am not sure this point warrants an entire study on its own.

We obviously do not agree, and note that Reviewer 1 does not agree either. As we highlight in the introduction, Ferguson et al. used K<sub>t</sub> to evaluate the land-atmosphere coupling from observations and model output of LCL, EF and SM<sub>surface</sub> during 30 day periods within the convective season. Our results demonstrate that applying the same technique to a different period, such as a wet season onset would result in no coupling strength if SM<sub>surface</sub> is used. We demonstrate the result would be erroneous and solely dependent on not including the SM that drives the LH during the considered period.

I will leave that for the editor to decide. Specifically, note that the exact same point could have been addressed using the forcing datasets only, by looking at soil moisture and surface fluxes and their connection to the boundary layer in GLDAS or MERRA – except for sampling slightly different (corrected) versions of MERRA, I don't really see the need to use an offline land simulation here. Second, even if it did (warrant a study), this point is still poorly demonstrated in the study, in my view. The authors could easily show the seasonal cycle of the different components of ET (evaporation, transpiration, interception), for instance – I recommended they did in my first review, but it is still not the case. I note that p.14 lines 373-384, they indicate that transpiration is only 10-30% of ET in DJF, but they do not indicate how much transpiration contributes to ET in SON. They should also show explicitly that transpiration is governed by SM<sub>rz</sub>, not SM<sub>1</sub> (e.g., with time series and correlations), and that the variability of SM<sub>1</sub> and SM<sub>rz</sub> is decoupled during SON.

To demonstrate the relative importance of soil and canopy evaporation versus transpiration differs between the two seasons we have added Figure 6 back into the manuscript. It was removed from an earlier draft of the manuscript during the editing process but has been included together with added discussion in Section 4.2 and Section 5 to explicitly demonstrate that transpiration is a minor contributor to the total ET during DJF but is important during SON. We included the following text in Section 4.2 to discuss Figure 6 and support our argument that the differing background states alter the mean ET and transpiration:

“Despite the similar mean and temporal behavior of SM<sub>1</sub> shown in Figure 2, SM away from the surface differs substantially between the two model configurations (Figure 5). The mean DJF ET is similar between CTRL and DRY, with differences between the two only 10-20 Wm<sup>-2</sup>, 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 6) indicating that the evaporation is the dominant ET mechanism. The enhanced mean SM in CTRL causes the CTRL ET to be greater than the DRY ET during DJF, yet both compare reasonably well to the observationally based estimates (Figure 3). However the lack of SM at depths below several centimeters for DRY during SON causes the reduced ET as compared to

CTRL during this period. The mean ET during SON is sensitive to the mean SM away from the surface, indicating that transpiration significantly contributes to the total ET during this period as can be seen in Figure 6. The large contribution of transpiration to the total ET in CTRL (Figure 6b) is facilitated by the moist subsurface soil moisture (Figure 5b). The reduced root zone SM in DRY leads to an increase in water stress and reduced transpiration, causing both the lower mean ET and transpiration fraction in DRY relative to CTRL. This reduction during SON is large relative to the mean ET during the period (Figure 3).”

As for question ii), I recommended the authors drop this part of the analysis and focus on i) instead. They have chosen to retain this part of the analysis. I don't think this is very fruitful. In the absence of land-atmosphere feedback, the fact that SM<sub>rz</sub>-from-CLM4/LCL-from-the-forcing correlations are similar in the CTRL and DRY CLM4 runs (with and without soil-groundwater interactions) only goes to show that the variability in SM<sub>rz</sub> and L-A fluxes is similar in both runs –despite the different mean SM<sub>rz</sub> and fluxes. This conclusion could (and actually, should) be reached separately, with time series and correlations between both runs, without computing correlations with a third variable (i.e., the LCL) from the driving dataset. I don't think it is fair to say, as in the conclusion p.21 lines 591-592, that “the diagnosed L-A coupling is insensitive to the mean vertical profile of soil moisture”. If the land were truly coupled to the atmosphere, the difference in mean ET in SON between CTRL and SON, for instance (up to 30 w/m<sup>2</sup>, or 100% of the CTRL value...) could very well impact land-atmosphere interactions. The similarity in SM and fluxes variability between CTRL and DRY may be interesting for its own sake with respect to the land model behavior; but it does not really inform on “the dependence of L-A correspondence on soil moisture state”, as the paper's title implies. I note that this aspect of the analysis is actually not even addressed in the abstract, which I take it reflects the fact that no real conclusion can be derived from this part of the study.

We agree that one could test how the background mean soil moisture impacts the correspondence between SM<sub>RZ</sub> and the surface heat fluxes without utilizing a third (here LCL) variable. In the context of solely looking into how EF varies with mean SM one wouldn't need the LCL. However the result that the SM<sub>RZ</sub>-LCL K $\tau$  is largely independent of the soil moisture background state emerges from the analysis due to the model experiment setup that explicitly forces the model to be comparatively wet or dry. This study intentionally focuses on the association between SM, EF, and the LCL. The lack of control the deep SM asserts in controlling SM<sub>RZ</sub>-LCL K $\tau$  in our analysis is evident from comparing the results from DRY and CTRL. To this end we have altered our conclusions to add context to our statements regarding the insensitivity of our computed values of SM<sub>RZ</sub>-LCL K $\tau$  and EF-LCL K $\tau$  to the background soil moisture state. The second paragraph of the conclusion now reads

“Our results also show that the statistically diagnosed land-atmosphere correspondence in our offline simulations is insensitive to the mean vertical profile of soil moisture. It is however, sensitive to the depth of the soil column considered. The implication of our findings therefore indicates a need to include the root zone in order capture periods when the ET is dominated by transpiration. We recommend that future studies of land-atmosphere coupling should include groundwater and focus on root zone soil moisture rather than surface layer soil moisture.”

Finally, the differing background soil moisture states is explicitly referenced in the abstract as:

“The simulated evaporative fraction and the boundary layer are shown to be strongly associated during both SON and DJF despite the differing background soil moisture states between the two

seasons and among the ensemble members.”

Finally, I asked in my previous review that the authors address the issue of seasonality potentially driving the SM/LCL correlations diagnosed here (a potential reason why CTRL and DRY might have shown similar signals, too). The authors claim to have detrended the data “separately over each season” (p.9, line 248) prior to computing the correlations. First, I am not sure I understand what this means – a linear detrending each year over three months? Why not classically and simply remove a mean seasonal cycle from the whole data? If this is not the case, I strongly recommend the authors do the latter. (Here I would also encourage the authors to describe more explicitly how they compute the correlations: with daily data, monthly data? I thought I understood daily, but p.9 lines 249-242 got me confused, this time).

But more importantly, I am strongly skeptical of whether the data was appropriately “de-seasonalized”, given that figures 6-8 in the revised version are exactly, pixel for pixel as far as I can tell, the same as figures 5-7 in the first version (except for the different color keys). I find it unlikely that removing the seasonality in SM, EF and LCL, and, e.g., computing correlations on anomalies, would result in exactly the same results.

To address the reviewers concern about the data processing we have further described our process to fully clarify how the data is processed. The original text read as follows (Line 189 in the original submission):

“ $K\tau$  is calculated between the de-trended three hourly modeled  $SM_1$  during the morning and the estimated three hourly LCL from the afternoon at each grid cell for each season.”

During the first round of reviews the reviewer failed to realize that the data was de-trended such that the seasonal cycle wouldn't drive the values of  $K\tau$ . To clarify our methods we changed the description to (Line 248 in the first revision):

“The data are necessarily de-trended separately over each season prior to deriving  $K\tau$  to prevent the strong seasonal cycle (Figures 2 and 3) from controlling the statistical relationship. “

The reviewer still struggled to grasp our data processing methods – we are in fact doing what the Reviewer wants us to do but we are clearly not communicating this clearly enough. Therefore we again seek to clarify our procedure and had changed the description to

“The least squares linear trend is removed from the data by calculating the trend over each season individually. The data are de-trended instead of removing the monthly mean annual cycle to ensure we don't create discontinuities within a season. Removing the mean annual cycle could possibly subtract very different mean values from points that are continuous in time, causing artificial discontinuities between the data from last day of a month and the first day of the subsequent month. De-trending the data over a season ensures the methods don't introduce artificial discontinuities between months within a given season. ”

We acknowledge that removing the mean annual cycle as opposed to de-trending the data would be an alternate way to ensure the correspondence is not dominated by the seasonal cycle, however we chose an alternate method.

To investigate the difference between removing the mean annual cycle and detrending the data over each season, we compared the timeseries of LCL that result from either detrending the data or removing the mean annual cycle at several locations. We found that the correspondence between the  $LCL_{\text{detrend}}$  and the  $LCL_{\text{anom}}$  ranged from 0.78-0.96. Both techniques yield similar results.

The reviewer highlights that the figures are identical to the previous version of the manuscript. Our methods haven't changed between versions; only our description of the methods has changed to clarify the steps we have taken. Therefore it is expected that the figures remain the same. Our revisions enhance the manuscript to more explicitly describe how the data were processed.

Reviewer #1 (Paul Dirmeyer)

The changes to the manuscript are acceptable and I feel my comments have been addressed. The shift in emphasis to the association between SM, EF, and LCL in these offline land model experiments makes the study more robust. I am glad to see more comparison to observations as well.

The title retains the previous emphasis on coupling strength. I would suggest the title be changed to be more in line with the current emphasis of the paper. Otherwise I think this is a well-written paper worthy of publication.

**We are obviously pleased by Dr Dirmeyer's comments.**

**We fully accept the suggestion on a title change. We had changed the title of the manuscript to "Diagnosing the seasonal land-atmosphere correspondence over Northern Australia: Dependence on soil moisture state and correspondence strength definition". However the title change was only reflected on the manuscript and not on the website.**

**We have therefore submitted a request to Svenja who changed the title on the website.**