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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.

Anonymous Referee #2

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(Page and line numbers in this review are from the printer-friendly version of the manuscript.)

In this study, the authors use offline simulations from a land surface model (CLM4) over Northern Australia to study land-atmosphere coupling during both the dry (SON) and wet (DJF) seasons – more specifically, they investigate:

i) Whether including root-zone soil moisture (SM), versus surface layer SM, in the statistical metric they use to evaluate soil moisture-atmosphere coupling, matters for the diagnosed coupling;

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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.

The study addresses an important issue: the dependence of diagnosed SM-atmosphere coupling on season, background land surface state (here, mean SM) and SM depth definition. In particular, the latter matters as satellite-based SM retrievals of SM, and thus associated diagnoses of SM-atmosphere coupling, often only correspond to the top-surface layer. It is thus important to understand the impact of this limitation on the estimated coupling (in particular when satellite-derived diagnoses differ from other observation-based assessments). However, I have some significant concerns with the study as it stands, which I believe warrant quite major revisions. My concerns have to do with the methodology used, as well as the presentation and interpretation of results.

— Methodology

The authors drive an offline land model with different atmospheric datasets, and then essentially correlate the simulated soil moisture (at different depths), as well as evaporative fraction (EF), with an estimate of the lifting condensation level (LCL) derived from the atmospheric forcing. I could see this framework being used to evaluate observed SM-atmosphere coupling, using hourly model-simulated SM and EF as surrogates to observations (given that such observations are not widely available). This would assume, though, that the land model simulates "perfect" (given the forcing) soil moisture and land-atmosphere fluxes – this would certainly need to be discussed. But the authors go beyond that, and assess coupling in different land model configurations. I don't see how this offline framework can be used to evaluate the sensitivity of the coupling to model configuration – here, the parameterization of groundwater and resulting mean deep soil moisture levels – since the atmosphere is always the same in every simulation and does not "see" the fluxes produced by the land model in different configurations. Given the difference in soil moisture between the two simulations (figure

4), I would expect the daily "sequences" of simulated surface fluxes (or deep soil moisture) to be different between both runs. In real life these different sequences would be associated with different atmospheric "sequences" (of LCLs), but here they are associated, by design, with the same atmosphere. I don't' see how land-atmosphere coupling can then be assessed in a relevant way. Unless the authors can explain otherwise, I fail to see how this experimental set up is suitable for investigating the question the authors want to address (i.e. the impact of model configuration on coupling). In general, I would also like the authors to acknowledge more clearly the model-dependency of their results: they are not analyzing observations, they are analyzing CLM4 outputs. For instance, the behavior of the top surface layer compared to the column average SM could be largely model dependent. Another methodological issue, potentially, is that, to evaluate the SM (or EF)- LCL coupling, the authors use a Kendall correlation coefficient, following Ferguson et al. (2012). I am not familiar with the latter study, but I have the following concern: it looks like the authors are correlating absolute values of SM (or EF) and LCL – not anomalies. I appreciate that Kendall correlation coefficients are better suited than, e.g., Pearson correlation coefficients, for non-linear relationships. However, I am concerned that, in a region with strong seasonality like the monsoon region of Northern Australia, correlations between absolute values are mostly going to capture the seasonally-forced co-evolution of the corresponding variables (i.e., SM, EF and atmosphere). This co-evolution happens without land surface feedbacks on the atmosphere. The strong correlations on figures 5-7, and the fact that there is overall very little difference between the CTRL and DRY simulations on figures 5-7 - despite, like I indicated above, probably different daily sequences of surface fluxes and SM- suggests to that this might be the case (i.e., seasonality dominating the signal). I strongly recommend the authors address and discuss this point.

In short, I think the authors cannot really investigate question ii) (see first paragraph) in the present framework. I think they should either drop this part of the analysis and produce a more restricted paper on the different between SM1-EF and SMrz-EF, or use fully coupled simulations instead.

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— Presentation, significance and interpretation of results

Beyond this first order comment, I also take issue with how the authors are describing their results. A lot of the paper consists in qualitative comparisons maps of correlations in different seasons (SON and DJF) and different model configurations (CTRL and DRY). The characterization of these differences is sometimes not consistent throughout the paper. For instance, p.10444 l.5 “DRY is generally more strongly coupled than CTRL during DJF” and p.10447 l.13 “the coupling between EF-LCL is similar in both model configurations”; also, p.10447 l.3 “The ET from CTRL and DRY are similar”, p.10447 l.14: “despite the mean ET. . . differing considerably between CTRL and DRY”. Sometimes differences are mentioned, but then deemed insignificant whereas they appear about as large as differences deemed significant (e.g., p.10444 lines 15-16). This all makes it look as though the analysis lack objectivity and coherence. To clarify things and let readers evaluate differences objectively, result presentation and statistical significance should be made clearer. In particular on figures 5-6-7, the authors should i) indicate significance levels on the maps, either by whiting out non-significant points or maybe with contours; ii) present map of differences between runs CTRL and DRY (also for figure 3); iii) indicate significance levels (for differences) on these difference maps.

Another concern, which falls a little bit along the same lines as the one above, has to do with the role of transpiration (Tr) in total evapotranspiration (ET). Here as well the authors appear to contradict themselves several times: p.10443 l.14 “DJF. . . indicating surface evaporation is the dominant ET mechanism” p.10445 l.21: “acknowledging the importance of transpiration during the wet season” p.10446 l.7: “DJF. . .despite evaporation dominating the simulated ET” p.1046 .14: “during DJF . . . transpiration is partly governed by the water availability within the root zone” which implies that Tr plays a role in DJF ET and coupling. These seemingly contradicting statements reflect a lack of clarity in the corresponding processes that, it seems to me, could easily be alleviated by showing the different components of ET in the CLM outputs: soil evaporation, interception, Tr. In particular, the authors need to show this to back

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up their claim that ET in DJF is mostly soil evaporation, which in the manuscript rests essentially on Figure 3 and the claim that DJF ET is similar in both simulations despite different root-zone soil moistures (although ∼10 W.m-2 differences can be noted).

Another point where I thought the manuscript could be improved, was in discussing the physical processes diagnosed in the correlations: for instance, the sign of the EF-LCL coupling, or SM-LCL. This could easily be discussed in the manuscript. Similarly, there is no suggested explanation for why SM1-LCL coupling is positive in SON (significant positive correlation) while EF-LCL and SMrz-LCL are negative: what is happening in terms of SM1-EF, SM1-SMrz, etc.? This should be analyzed, so readers can have a better sense of why the SMrz-atmosphere coupling differs from the SM1-atmosphere one: how, why do SM1 and SMrz become uncoupled?

— Other comments

The use of several datasets to drive CLM4, although explained in detail in the Methods section, is never really exploited in the analysis. Figure 8 and text p.10446 lines 19-26 start to address inter-ensemble member differences, but do not draw any conclusion: what is to be concluded from figure 8? Differences are not very clear and, here as well, statistical significance should be addressed.

Finally, I found the introduction to be long and lacking focus. I recommend the authors identify the problem they want to address and "zoom in" on it more clearly and rapidly. As it is now the issues addressed and the goal of the study do not stand out clearly.

—Comments along the text:

p.10432 line 24: "temperature" should go with atmospheric states.

Figure 1: panels b c d are not discussed, thus should be removed.

Section 2: presentation of datasets and methods felt a bit backwards, as model validation is discussed before model and simulations: I would recommend reorganizing as: forcing datasets; model; obs; methods.

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p.10441 line 13: ”GLDAS. . . MERRA.BT” should be named earlier

p.10441 line 18; “ensemble” the four members from 4 forcing datasets ?

Section 4.1

Figure 2a: what time span is the X-axis ? What is the time resolution (monthly?) ?

Figure 2b: would be more informative if showing % instead.

Figure p.10442 l.25: this statement feels awkward after a whole paragraph discussing differences between simulations. If SON ET is much lower in DRY than in CTRL, how can they both agree with observations?

Section 4.3: the physical meaning of these correlations should be explained (higher EF, lower LCL, etc.).

p.10444 l.21: “negative coupling” should be explained.

p.10444 l.22-24: this statement should be “unpacked”, it is a bit unclear.

p.10445 l.1 : “slightly higher” where ?

p.10446 l.4 : probably “figures 6 and 7”.

Figure 5 caption: “morning time EF”: the text indicates it is afternoon EF (p.10439 l.4).

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