Reviewer #2:

Summary/overall impression:

This article uses regional climate simulations to test the sensitivity of land-atmosphere coupling in Europe to changes in atmospheric moisture and temperature profiles, by applying a well-known land-atmosphere coupling metric. I genuinely enjoyed reading this article and feel that the results have important implications for considering the influence of L-A coupling in a changing climate. The article is well-organized and uses novel methods that are based upon previous studies to address the authors' hypotheses. I feel that this study will make a valuable contribution to the scientific literature. Moreover, the authors do an excellent job with the use of visuals to tell their story. As the authors' results are so clearly valuable, I have a few suggestions that I hope will help ensure the authors' main points are communicated clearly.

First of all, we would like to thank the reviewer for the supportive and constructive review. We hope we have satisfactorily addressed all suggestions and comments. Please find our responses below each corresponding comment.

Specific comments:

L24-25: I may suggest rearranging the first two sentences, leading off with what L-A coupling is. i.e. "Land-atmosphere coupling describes the covariability between land and atmospheric states, and plays a key role for understanding…." An additional suggestion here may be to specify which states in the climate system.

Response: We will revise the text accordingly.

L26: Schumacher et al. 2019 would be another relevant source for the influence of coupling on heat waves:

Schumacher, D. L., Keune, J., Van Heerwaarden, C. C., de Arellano, J. V. G., Teuling, A. J., & Miralles, D. G. (2019). Amplification of mega-heatwaves through heat torrents fuelled by upwind drought. *Nature Geoscience*, *12*(9), 712-717.

Response: We will add the citation.

L81: When you say. "The approach is based on they hypothesis…" it implies (to me) that this hypothesis was presented by numerous members of the broader scientific community and thus, supporting references should be provided. Though I am guessing you mean the hypothesis *you* are presenting specifically in this paper, in which case I might reword this to read "The approach is based on our hypothesis…"

Response: We agree with the point here and will make the change accordingly.

L84: It's not entirely clear what you mean by "the differences in the mean and vertical distribution." An extra sentence or two could improve clarity so that the reader knows what to expect in the rest of the analysis, especially if they are the type of reader that skips around sections and doesn't necessarily read the methods in depth. Are you meaning to say that you are considering how the mean changes, or how moisture and temperature deviate *from* the mean? Additionally, it may be helpful to clarify whether the change in vertical distribution going to be considered separately from your analysis of changes in the mean state (or deviation from the mean state) here.

Response: We agree that the wording is not entirely clear. We will edit and amend the text.

Section 2.1.1: This is well-organized, concise and easy to understand. Nice job.

Figure 1: I also really like how this figure is presented and summarizes your past work with respect to the metric you are using.

Figure 5: Once again… great use of visuals.

Response: Thank you these comments.

L343-346: Forgive me if this is beyond the scope of the current study, or if I missed something here. I find it interesting that the hot and dry divergence factors increase CTP, but also increase the surface inversion. While we generally associate higher CTP with dry soil advantage, could a greater inversion strength over wet soils also lead to more moisture buildup in the PBL, and thus a lowering of the LCL to the PBLH, that may also trigger convection? In this case, would we expect the CTP-HI metric to be sufficient for diagnosing coupling potential? Papers by Ek et al. (1994 and 2004) may be relevant to a discussion of impact on surface inversion here. You may ignore this comment if I'm missing the point.

Ek and L. Mahrt, 1994: Daytime Evolution of Relative Humidity at the Boundary Layer Top. Mon. Wea. Rev., 122, 2709–2721. doi: 10.1175/1520-0493

B. Ek and A. A. M. Holtslag, 2004: Influence of Soil Moisture on Boundary Layer Cloud Development. J. Hydrometeor, 5, 86–99. doi: 10.1175/1525-7541

Response: Thanks for this insightful and interesting comment. It is a nice line of thought to pick up.

First we would like to quote Ek and Mahrt (1994), who described a specific case in which subsidence "traps" surface moisture in a thin boundary layer, which increases the surface relative humidity. The timing of when the subsidence is overcome determines the development during the day. They state that "… *if this time* (to overcome the subsidence)

is comparable to, or large compared to the period of mixed-layer development, then the main influence of the subsidence is the decrease the boundary-layer depth leading to smaller relative humidity at the boundary layer top compared to the case of no subsidence." (Ek and Mahrt, 1994, p2713). In this case, the subsidence/inversion would inhibit any coupling event.

Taking now the hot and the dry perturbation cases, they show on average stronger inversions as compared to the reference which inhibit boundary layer growth in the morning hours until they are overcome. Further, the temperature gradients above the inversion are stronger which hints to weaker stability, and this would support more rapid boundary layer growth after the inversion was overcome in these perturbation cases. The boundary layer growth could then support the mixing of the trapped moisture and potentially decrease the LCL to the PBLH. Given a wet soil, we would expect high surface evaporation moistening the boundary layer below the inversion in the morning hours, but also weak sensible heat fluxes and thus a weak "force" pushing against the inversion. Hence, weak BL growth would be expected in the morning hours over wet soils. This would hint to a longer timescale to overcome the inversion, which – following Ek and Mahrt (1994) – would lead to less clouds. Over dry soils, we would expect higher sensible heat fluxes and thus a higher probability to overcome the inversion and foster BL growth. Although, there is less moistening of the boundary layer from the surface expectable, it is more likely that the inversion is overcome and thus a coupling event could occur.

So, the case you referred to might indeed occur, but a dry coupling event seems more likely. However, whether a coupling event could occur over wet soil is expected to be strongly dependent on the inversion strength and whether there is still sufficient boundary layer heating to overcome the inversion early enough to enable the development of clouds. Also the entrainment flux is expected to play a role for the boundary layer development and the L-A coupling signal (van Heerwaarden et al., 2009). This information is not included in the framework without further extensions, as it neither considers the lowest 1000m of the BL, entrainment, nor the energy partitioning at the land surface. Hence, the CTP-HI_{low} framework would not capture the effects of changes in inversion strength in any case.

van Heerwaarden, C. C., Vilà-Guerau de Arellano, J., Moene, A. F., and Holtslag, A. A. M.: Interactions between dry-air entrainment, surface evaporation and convective boundary-layer development: DRY-AIR ENTRAINMENT, SURFACE EVAPORATION AND CBL DEVELOPMENT, Q.J.R. Meteorol. Soc., 135, 1277–1291, https://doi.org/10.1002/qj.431, 2009.

L355-357: Since you are discussing the influence of temperature in the "hot case," it can be a bit confusing when you then say "fraction of nAC-days within the hotspot" as my mind

first thought hotspot in a literal, temperature sense. I would suggest changing this to read: “within the L-A coupling hotspot.”

Response: Thank you for bringing this point up. We agree that the term hotspot can be misinterpreted in association with the hot case. We will revise the text according to your suggestion.

L360-362: Is this where CTP is increased, but so is the temperature inversion? So perhaps the likelihood of convective triggering over wet soils could be tied to the comments associated with L343-346 above?

Response: In the north, the increase in CTP - implying a destabilization of the atmosphere above the inversion - rather bumps stable (and eventually wet) atmospherically controlled days to the non-atmospherically controlled regime.

Following our argumentation above, the framework likely does not represent inversions, because the integration of CTP starts at 100hPa AGL. Hence, the process mentioned above would not be captured by the framework. Nevertheless, it still might occur, but we think that further analyses are necessary, which e.g. involve CIN to represent the inversion strength and additionally the surface fluxes as an indicator of whether the inversion can be overcome before the end of the mixed-layer development.

Broader comment regarding discussion: How might overall warming of the climate impact the length of the season in which we consider L-A coupling to be most influential? Your results imply that warming enhances coupling strength, so would that also mean that L-A coupling might be and important driver of hydroclimatic variability over a longer warm season? For example, instead of JJA, perhaps the “coupling season” would now be MJJAS?

Response: Dirmeyer et al. (2013) suggested an earlier springtime onset of L-A feedbacks over the US in the future. So given that warming enhances the coupling strength, a longer warm season might indeed also imply a prolonging of the “coupling season” to MJJAS over Europe. However, our analyses were not tailored to investigate this effect and do not provide enough evidence to give a sound answer to your questions.

Dirmeyer, P. A., Jin, Y., Singh, B., and Yan, X.: Trends in Land–Atmosphere Interactions from CMIP5 Simulations, 14, 829–849, https://doi.org/10.1175/JHM-D-12-0107.1, 2013.

L466-468: I wholeheartedly agree that we need more vertical resolution, everywhere, however, I do believe you can argue that while Wakefield et al. (2021) shows that vertical resolution is a limiting factor, you can still get representative estimates of the L-A coupling pre-conditioning even when vertical resolution is unfortunately limited. Therefore, I think you can use this reference to argue both points… your limitation in vertical resolution

introduces uncertainty, but that uncertainty is not so large that it substantially impacts the validity of your results.

Response: Thank you for bringing up this line or arguments. We will gladly pick it up in the discussion section.

L479: I'm not sure about the use of the word "reliable." My mind immediately jumps to an operational use of the word and thinking about model reliability. I do like that you say the feedback class is *insensitive* to changes though. Maybe "wherefore a consistent regime can be expected," if that's the message you are trying to convey.

Response: Thank you for this suggestion. We will revise the sentence.

Technical:

L123: Change "but maybe limit investigations" to "may limit investigations."

L142: Change "deep convection is inhibited by an inversion, only shallow clouds…" to "deep convection is inhibited by an inversion and only shallow clouds…" or separate into two sentences.

L382: Typo "Please not that…" should say. "Please note that…"

Response: We will incorporate all of your technical suggestions.