Review of the manuscript

# Soil moisture—atmosphere coupling strength over Central Europe in the recent warming climate

Submitted for publication by Thomas Schwitalla, Lisa Jach, Volker Wulffmeyer and Kirsten Warrach-Sagi

## General comments

*I am reviewing a revised version of the manuscript. As I have not seen the manuscript before, I comment on both the author responses of the previous review and the manuscript.*

The authors have provided point-by-point responses to comments raised by two referees. The coverage is comprehensive and the manuscript was amended accordingly. One key-point that has been raised by both referees is the choice of datasets (ERA5 + E-OBS) in relation to the scope of the study: RC1 and RC2 raise concerns regarding consistency of the reanalyzed model state and true precipitation fields and demand further explanation. While the authors have provided an analysis based on ERA5 precipitation (which is supposed to be a more consistent approach), the key points raised by RC2 are not covered adequately in my opinion:

- The fundamental question, 'what is the value of a feedback study based entirely on model data'? Is not answered. In other words, it remains unclear, what exactly is under investigation here: LA coupling in the ERA-5 reality (i.e. the ERA-5 land surface model and its interplay with the atmosphere), the actual physical coupling of the real atmosphere with the real surface or something hybrid in between?
- The authors advocate a rather simplistic view of model performance and representation of reality which I consider critical in this context: "An improvement of the representation of metrics must directly propagate in improved forecasts and vice versa". In general, this is not true: Sometimes, in particular with respect to the nonlinearity in surface-layer models, improvements in some metrics cause performance degradation, in particular for coupled set-ups where error-compensation is likely to occur.

After evaluating the manuscript on my own, I further identified related major issues that touch upon the core of the manuscript, including the definition and description of coupling metrics, selection of data and sampling issues (see major issues). These need to be covered for the manuscript to become publishable. I leave it to the editors whether this happens within major revisions of the manuscript or is better carried out outside the peer-review process which would result in a rejection.

## Major issues

**M1 The concept of land—atmosphere (LA) coupling as it is introduced in the manuscript is improper and incosistent.**

a) LA coupling is not trivially determined and I would wish for a framing of how it is defined/quantified (namely: by means of co-variability) already in the abstract as the determination of coupling is central to the entire manuscript. Regarding the definition of coupling by covariability, there is further the major caveat of correlation vs. causality. Further, the definition of of coupling in terms of magnitude of the co-variability may hide compensating coupling mechanisms. A possible way to come around these conceptual limitations is considering surface-layer fluxes (as many of the studies cited in the manuscript do) and I urge the authors to also look at the fluxes acting as physical mediators between the land and atmosphere properties.

b) The concepts of LA coupling and LA feedback are not the same (I understand that a coupling can also have dampening effect versus a feedback that is strictly amplifying) and the authors should exert greater care when defining these two concepts in lines 30-50 to avoid ambiguities.

c) Definition of the coupling indices (Eq. 1) is incomplete. Operator σ is not defined; Variables used in calculating the ACI are not given explicitly.

d) The analysis is constrained to approximate day time (06 – 18 UTC). While it has been shown that night-time failure of numerical models has little impact on convective boundary layer during the next day for idealized cases (van Stratum and Stevens, 2015), constraining of the LA coupling to daytime only is certainly biased and the impact on the results should not be underestimated as evidence from measurement shows that day-time and night-time effects compensate to a large degree. The fact that incorporating nighttime results into the analysis has "detrimental impact" (l. 150) may simply demonstrate that the coupling effects are far less important than the authors suggest as indeed much of the positive coupling feedback during daytime is reversed at night, which is a physical aspect of the problem.

**M2 The role of artificial and self-correlations is largely ignored.**

a) As an example, consider TCI (according to Eq. 1): Latent heat flux crucially depends on soil-moisture availability (most likely via parameterization of the Bowen ratio). An index defined on modelling data as suggested by Eq. 1 suffera from two levels of unwanted correlation: First, it will reproduce the Bowen-ratio parametrization. Second, LH depends on soil-moisture and thus on the surface-layer closure. I wonder to what extent, we look at an analysis of the surface-layer and Bowen-ratio closures rather than a real physical-coupling analysis. This can easily be checked by evaluating the TCI theoretically based on the underlying parameterizations which will serve as a baseline. It would be fair to talk about physical coupling only whenever this baseline correlation is exceeded.

**M3 Statistics on [very] small samples**

a) The reference period (30 years) is further split up into wet, dry, warm and cold summers which results in an extremely small sample size. In my view, this makes this study more a multi-case study rather than an assessment of climatological shifts as the abstract claims. While both approaches have merit, in my opinion the general conclusions that can be drawn from such a small sample (where individual events like the anomalous summer of 2021 etc.) do not reach as far as the authors suggest. The results, discussion and conclusion sections should be amended accordingly.

b) The findings presented in the discussion section are actually interesting, but I do not see how – in the generality suggested by the authors – they can be hold up in view of the diverse results. It would be interesting to underpin these by a quantitative analysis (for instance a pdf analysis that strips of the geo-location and would isolate the physical processes at the core of coupling). [In this way, the sample size would be increased by also sampling space, on top of time!]

**M4 Presentation of results**

The number of figure panels is not appropriate given the limited interpretation. I suggest to rather focus on individual representative figures and give them more space rather than reproducing patterns for each year and parameters. If relevant at all, they could be given in the supplement (as is already the case for some parameters).

**M5 The analysis is missing the final link from the surface to the atmosphere**

The authors claim to investigate land—atmosphere coupling. In fact, they, however, separately assess the terrestrial coupling (soil-to-surface layer) and the atmospheric coupling (surface-layer to atmosphere); the final link that one would expect based on title, abstract, and introduction (soil – atmosphere) is neither carried out nor discussed in the analysis.

# Minor issues

- Abstract: "[…] for exacerbating durations": inappropriate verb (surface conditions may also act in the other direction + bad style → please rewrite!
- l. 106-111: I do not see why it is necessary to discuss the disadvantages of other reanalyses not used here. It diverts from the main path.
- The choice and weighting of the layers used for root-zone soil moisture has crucial impact on the time scales for which coupling can be investigated. In fact, it can be shown that higher weights on deeper layers act like a filter (Liu and Shao, 2013; van der Linden, 2022). This needs to be discussed an it should be carefully checked if the weighting and choice of layers selects appropriate time scales.
- The extensive discussion of TCI and introduction of SH—LH correlation belongs to the introduction rather than to the results section.
- Using CAPE as coupling metric is rather dangerous as it is a potential (and not an eventually released) kind of energy. The huge values over Easter Europe probably result from the fact that in these dry regions, convective inhibition is large and CAPE often "lives" for a relatively longer period of time.
- Fig. 9 and associated text: If I understand correctly, the LCL "deficit" is not really a deficit, but an anomaly.
- l. 389-394: This is not related to the present work and should either be removed or moved to the introduction.

# Technical comments

- l. 76: "The in the preceding paragraph described" → The preceding paragraph describes
- l. 118: remove "seasons"
- l. 121-123: move predicate before object in the sentence!
- l. 264: why "could be"? Why spectulate? This can easily be checked based on available data
- l. 341: remove second period ('.').
- l. 342: "2018 [...]" → "The year 2018 [...]"

# Referencens

van der Linden, S.J.A., Kruis, M.T., Hartogensis, O.K. et al (2022): Heat Transfer Through Grass: A Diffusive Approach. *Boundary-Layer Meteorol* **184**, 251–276 https://doi.org/10.1007/s10546-022-00708-7

Liu, S and Shao Y. Soil-layer (2013): configuration requirement for large-eddy atmosphere and land surface coupled modeling (2013). *Atmospheric Science Letters* **14**, 112–117 https://doi.org/10.1002/asl2.426

van Stratum, B and Stevens, B (2015): The influence of misrepresenting the nocturnal boundary layer on idealized daytime convection in large-eddy simulation. *J Adv Model Earth Syst* **7(2)**, 423–436 https://doi.org/10.1002/2014MS000370