Revision on: Schwitalla et al., "Soil moisture-atmosphere coupling strength over Central Europe in the recent warming climate"

The manuscript provides an analysis of the inter-annual variability in land-atmosphere coupling focusing on the summers from 1991 to 2022 in Europe. The analysis uses the ERA5 reanalysis product and combines different coupling metrics and atmospheric and soil conditions to try to explain the drivers of summer conditions. Although the main idea of the manuscript sounds interesting, the text and explanation of the results are very confusing and in my opinion is not solid enough as it is to be published.

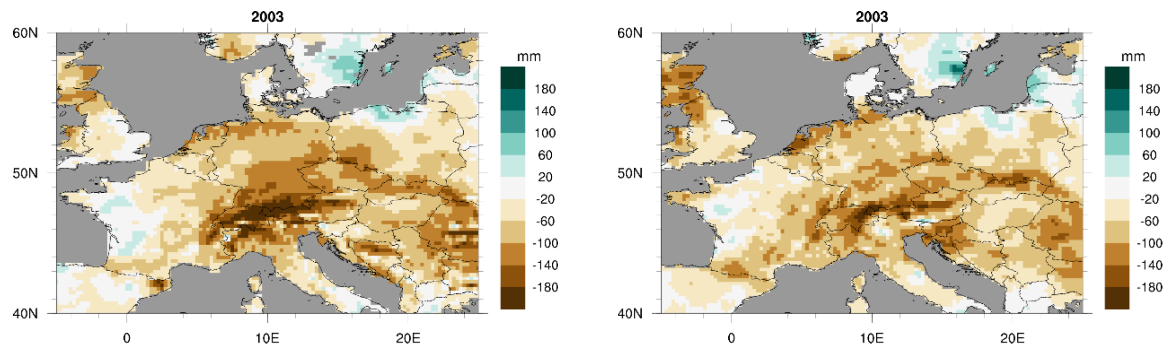
We thank you for carefully reviewing our manuscript. You suggested to focus on central Europe (see below), therefore we revised our investigation for the region of 5°W-25°E, 40°N-60°N. We refer from now on to this region unless stated otherwise. Please find our responses to your comments as marked in blue.

Specific comments:

The authors decide to use a reanalysis for the study of LA coupling, soil conditions, surface pressure and air temperatures but then they use the E-Obs product for precipitation. Despite the good performance of E-Obs in precipitation, if the aim of the study is to analyze the interconnection between variables and LA metrics in the "reality" of the ERA5 product, I do not agree with the use of E-Obs in precipitation, when all the rest of variables are related to the ERA5 simulated precipitation. The change of E-Obs by ERA5 precipitation may not change the results, since they may agree in the classification of dry and wet summers but in my opinion it is a required step for adding consistency to the analysis.

Different from temperatures, humidity, pressure, and soil moisture, precipitation is not assimilated into the ECMWF model for the ERA5 product and therefore is a pure diagnostic variable of the model with a strong dependence on the convection and microphysics parameterization.

For our study we require summer anomalies in precipitation to identify warm dry summers. The magnitude of the mean summer precipitation 1991-2020 is larger in ERA5 than in EOBS throughout the domain with maxima of 30 % in mountainous regions, especially the Alps. Much more important for our investigation are summer precipitation anomalies in each year. These show the same general patterns and magnitudes in ERA5 and EOBS. Below in figure R1 please find example plots of precipitation anomalies from ERA5 and E-OBS for the very wet and warm summer 2010 (not used in our study) and the extremely dry summer 2003 showing minor precipitation differences in extreme years.



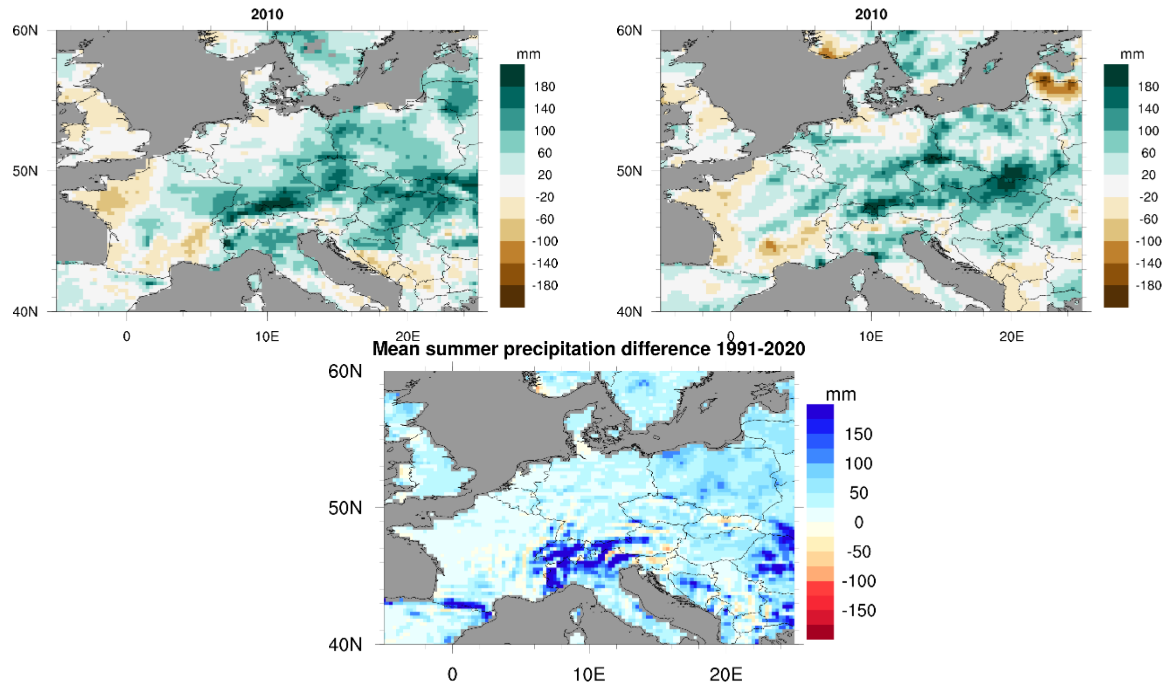


Fig. R1: Precipitation anomaly for summer 2003 (top row) and 2010 (middle row) derived from ERA5 (left column) and E-OBS (right column). The bottom row shows the mean precipitation difference of the summer months between 1991-2020.

Therefore, we had decided to use E-OBS in this study. However, following your suggestion, we complete the analysis adding ERA5 precipitation.

The use of ERA5 product for the study of LA coupling is also controversial per se, since land surface models like the one employed in ERA5 have several difficulties in simulating LA interactions (e.g. Dirmeyer et al., 2018) and the authors have not validated the ERA5 simulation of the coupling metrics employed in the analysis.

Standalone land surface model simulations do not impact the atmospheric variables. Dirmeyer et al. (2018) refer the “difficulties in simulating LA interactions” of such stand-alone model simulations. Further it is not clear which specific difficulties in the LSM HTESSEL you are referring to in your comment. Fig. 9 and Table 2 in Dirmeyer et al. (2018) display reasonable results of the stand-alone land-surface model HTESSEL (EL) which is applied in ERA5. In section 2b they mention that this HTESSEL stand-alone simulation is forced by altitude corrected atmospheric variables from the coarse ERA-Interim product as only relation to a Reanalysis product.

Reanalyses data provide the three-dimensional consistent land and atmosphere gridded multidecadal timeseries of diurnal cycles closest to observations and therefore allow to study on the regional climate scale LA coupling beyond studying surface variables. It is not the goal of this study to evaluate reanalysis data or a model simulation. It is the goal to study the land-atmosphere interaction based on the best available 3-dimensional long-term gridded data set for Europe which is also applied in very recent studies of Rousi et al.(2022) and Rousi et al. (2023) to investigate feedback effects between the atmosphere and the land surface.
Coupling metrics are based on variables that are currently not available at the same temporal and spatial resolution from observations but from model simulations or reanalysis data (Findell et al., 2023). Therefore, the metrics themselves cannot be evaluated. The ERA5 variables used in this study (soil moisture, 2-m temperatures, surface sensible and latent heat flux) were successfully applied in LA feedback studies of Sun et al. (2021), Qi et al. (2023), and Rousi et al. (2023).

Given that the validation of the ERA5 product simulation of LA coupling would correspond to another article, I recommend adding another reanalysis product or model to the analysis and focus on the agreement between models. If the authors still consider that adding more products is too complicated then I would recommend to add another section to the article discussing the possible effect of ERA5 uncertainties (e.g. in precipitation, LH and SH) reported in the literature on the results (not in general as it was done in line 391).

Following your suggestion further below, we investigated the study of Martens et al. (2020) which relates to the study of Muñoz-Sabater et al. (2021). As shown in both studies, the representation of the surface fluxes, soil moisture and net radiation in ERA5 is very reasonable compared to in-situ measurements over Europe.
We added the following to the end of the discussion section:

“Martens et al. (2020) evaluated surface latent heat fluxes from ERA5 against FLUXNET stations (Pastorello et al., 2020) for the period 1991-2014. Their analysis revealed that ERA5 performs well in moderate temperature climate which is the case over Europe. ERA5 soil moisture over Europe shows a reasonable correlation of up to 0.7 over Europe (Muñoz-Sabater et al.,2021) while LH in ERA5 tend to be overestimated on average by about 9 W/m^{-2} when compared to all stations. SH in general shows almost no bias. This overestimation could be related to an overestimation of wet days in combination with underestimated sub-daily precipitation rates (Beck et al., 2019). The overestimation of precipitation resulting in higher LH estimates could lead to an increased atmospheric instability and thus affecting the ACI and the LCL deficit.”

Regarding your point of using other/additional reanalysis products. Of course, in the preparation of this work, we performed a detailed review of available data sets:

- UERRA is only available until July 2019. Its sensible and latent heat fluxes are only available in six-hourly intervals and cannot be used for our scientific analyses (https://confluence.ecmwf.int/display/UER/Issues+with+data).
- COSMO-REA6 (Bollmeyer et al., 2015) is only available from 1995-2019 and does neither make use of a sophisticated data assimilation scheme nor of an ensemble approach.
- CFSR (Schneider et al., 2013) is only available until 2010 and thus does not cover the recent climate change period.

Therefore, ERA5 provides the most advanced long-term high-resolution reanalyses data for Europe and there is no added value to our study by adding another reanalyses product. A short paragraph has been added to section 2.1 to clarify why we used ERA5 and no other reanalysis data sets.

The classification of summers in warm dry or wet was done using averages of the whole Europe as domain, although most of the time the authors only comment on the results over Germany and around countries (e.g. section 4.1.1). I think it would make more sense to classify the summer with the averages of an area centered in Germany avoiding the effect of the different regions in this classification and the different patterns that you obtain for the same category (e.g. warm and dry).

The selection of the domain also complicates the description of the results. The authors focus the analysis over Germany, thus in some part of the text they comment only results over Central Europe (e.g. section 4.1.1) and in other all Europe (e.g. section 3.2), which makes it very confusing because the patterns and results in Europe are specially diverse. I would recommend the authors to focus the analysis in central Europe, avoiding the discussion of north and south areas, or to divide the results section into paragraphs dedicated to the phenomena happening in each region.

Thank you for raising this point. We agree that it is beneficial to set the classification region to a smaller area. Therefore, we decided to exclude areas south of 40°N, north of 60°N, west of 5°W, and the areas east of 25°E in our analysis, reprocessed all the analyses, and modified the figures accordingly.

Right now, the structure of the results includes the explanation of each variable over the whole Europe is very difficult to follow, especially when the authors try to connect results between variables, because it is not clear to which region they are referring. Something similar happens with the selection of warm and dry summers. The chosen criterion leads to very different results for the same category (perhaps because of the spatial variability in the whole Europe used as average for the classification). And then the description of results seems incomplete since the explanations of the authors do not apply for all areas and all summers in the same category. The two results sections need to be revised and re-organize to improve the clarity of results for different regions and years. Perhaps a good idea is to show the results by year (including all maps of the same year in the same figure) and reduce the number of selected years so we can better follow the story that the authors are suggesting.

Following your suggestion, we reduced the number of years by setting a 2-m temperature anomaly median threshold of 0.5°C, i.e., for warm years the anomaly median of the domain needs to exceed the threshold. This together with focusing on central Europe slightly changes the selected of years in our analysis for the summer seasons. 1994 and 2013 dropped out and 2017 is added, i.e., 2003, 2006, 2015, 2017, 2018, 2019, 2020, 2021, and 2022 are classified as the warm and dry years. 2002 has now changed to a cold and wet year and is shown, together with 1997, in the supplement.

For clarity we reorganized section 4. We start with the terrestrial coupling strength. This is followed by the correlations between LH and SH in section 4.2. Section 4.3 shows our results of the ACI in connection with the LCL deficit.

The results (sections 3 and 4) are also not clear and not well supported. For example, the analysis is incomplete (e.g. the reader is sent to section 4.3.2 and 3.3 in lines 202 and 203 but they do not exist) or it includes wrong references (e.g. Figure 14 in line 300, line 303 referring to high SH in Fig. 10 when Fig10 shows correlations, and other missing references in the paragraphs like in lines 248 and 249 Fig 6 and Fig8 should be cited).

We carefully revised sections 3 and 4. This was also required due to the smaller domain of the analyses and focus on warm dry summers in the revision. We also added additional references the figures at the appropriate positions.

The results in section 3.0 are also supported by references mainly to abstracts in conferences (e.g. line 232) instead of on the results from the ERA5 product.

The references related to our C3S references are not conference contributions but press releases from official reports of the Copernicus Climate Change Service (C3S) published since 2017 online. Since 2019 they also have a doi. We therefore chose to change the references to the reports themselves 2021 and 2022 and refer to the report websites for 2017 and 2018.

The main justification for using ERA5 data for this analysis is that you have all the environmental variables more or less consistent with each other, so for example if in line 206 you are saying that the dry anomaly in summer is related to dry spring season you should support that with a map based on ERA5 in the supplementary information that supports that claim.

Thank you for your suggestion. We added the spring season soil moisture anomalies for the summer seasons to the supplementary material.

The same happens with other explanations of results based on some heavy precipitation or drought events (e.g. lines 169, 238, 353,357-359), are these events really represented in the ERA5 data? Because if not the results that we are seeing are not related to that event.

In figure R1 above we show exemplary for 2010 and 2003 the comparison of ERA5 and EOBS for large scale strong precipitation and drought summers. To show that ERA5 reasonably well simulates heavy precipitation and drought events, we added the ERA5 precipitation anomalies to section 3.3 of the revised manuscript.

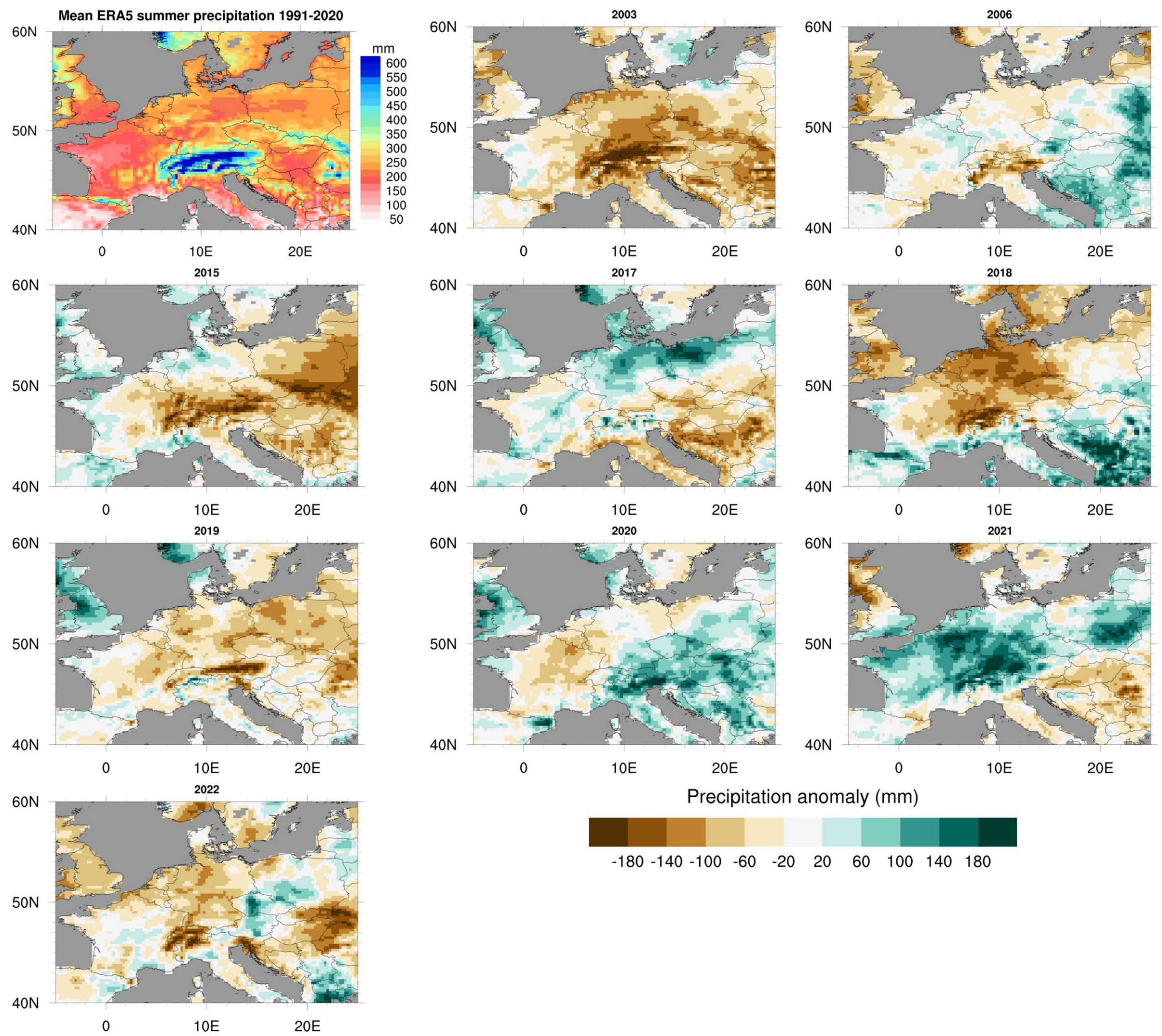


Figure R2. ERA5 summer precipitation anomalies with respect to 1991-2020.

The patterns show that the events are presented in ERA5 in both, precipitation and soil moisture anomalies. In 2021 ERA5 shows stronger precipitation than EOBS in the region of Germany and Benelux that was hit by the severe large scale precipitation event in July (Mohr et al., 2023) .

Some of the claims based on the results are not easy to follow or see in the maps (e.g. line 200 “By comparing Fig. 6 and 7…”). Perhaps a statistical analysis of spatial correlations between variables could help to reach more robust conclusions.

We calculated the correlation coefficients between temperature and precipitation. In the analyzed reduced domain mentioned above, the correlation is always negative with values between -0.25 and -0.65 indicating that in most cases, a positive temperature and negative precipitation anomaly are associated with each other. This statement has been added to the new section 3.3.

Another example is in the paragraph starting in line 277, there are more coupling hot spots over central Europe for example in 2019 and 2006 but the soil moisture anomalies sometimes are negative and sometimes are positive. The authors should make an effort to explain the results that we are seeing or reduce the number of maps included in the manuscripts explaining the processes leading to warm conditions in particular years and areas.

The corresponding paragraph in section 4.3 now reads:

Coupling hot spots are observed over East and Southeast Europe with ACI values of more than 250 J kg^{-1} (Fig. 8). They are connected to higher values of LH over these regions due to neutral or positive root zone soil moisture anomalies in 2006, 2019, 2020, and 2021 (Fig. 5). These coupling hot spots agree with the sensitivity between temperature and moisture change signals in Europe found by Jach et al. (2022).

Also the interpretation of land atmosphere coupling should be revised in the manuscript. I am not sure the authors explain clearly the role of soil moisture deficits in the restriction of latent heat flux and the induced increase in temperature. For example, this is the case in the mentioned paragraph (line 277), since both strong and weak coupling are associated with positive soil moisture anomalies. We need more information about what is happening there.

We added the following paragraph to our analysis in section 4.3:

"In case evapotranspiration is not limited by soil moisture, the incoming radiation is allowing for potential evapotranspiration and surface latent and sensible heat fluxes are partitioned accordingly. In case evapotranspiration is not limited by incoming radiation but by available soil moisture evapotranspiration is below the potential rate leading to higher Bowen ratios. The increasing Bowen ratio leads to an increase in temperature. This enhances evapotranspiration and therefore a gradual decrease in soil moisture towards wilting point. According to Benson and Dirmeyer (2021) this ultimately leads to the situation that latent heat fluxes almost vanish and the incoming radiation mainly transforms into sensible heat which can exacerbate heatwaves and droughts."

The conclusion sections could be reorganized, separating the discussion from the conclusions. In this way perhaps it is easier to identify the real conclusions of this study and the new information that we have learned about land-atmosphere coupling, which at the moment is not clear.

Following your suggestion, we separated the discussion from the summary to enhance readability of our manuscript.

Minor comments:

Line 111: "To complement our analysis, seasonal mean anomalies of 500 hPa geopotential (Lhotka and Kyselý, 2022) and ERA5 volumetric root zone soil moisture were calculated" This is not clear, do you mean geopotential height? Also are both variables calculated from ERA5 or do you use another database for the 500 hPa geopotential?

Both variables are calculated from ERA5. We will make it clear in the manuscript.

There are some minor spelling errors. The text should be revised (e.g. line 64 “suggests”, line 75 “In the preceding…”, line 213 no new paragraph, line 228 “The very…” is a very long sentence and the connectors are not used well, line 169 “caused by the severe'', line 189 “previous” no “precious”, line 199 revise connectors, line 258 remove “in addition”, line 294 “suggests”, line 299 “is more often in...”, line 302 “a considerable increase in the HLCL”)

Thank you for spotting this. We will recheck grammar and spelling throughout the whole manuscript.

Please, correct the order of maps (2003 and 1994) in figure 4.

As the classification of the summer seasons has changed, Figure 5 now contains different years and in the correct order.

Line 236 “preventing a moisture limitation”, please check this sentence. Do you mean here “leading to moisture deficits”?

We agree. We revised the sentence in the new section 4.2 according to your suggestion to: "During the most hot and dry summers 2003, 2018, and 2022, the correlation LH-SH became negative over Central Europe which is related to the anomalously warm and dry conditions during these seasons leading to a moisture deficit in the soil."

Line 254 “show non-significant values” please avoid the “significant” word if you did not apply a significant test. If you did apply a significant test, please provide the details on the text.

As we now focus our analysis on Central Europe, this part of the sentence relating to Spain and Portugal has been deleted. We did not apply any significance tests in our analysis.

Line 246 “almost weak” Please replace it by another expression e.g. “mostly weak” or just “weak”.

As we removed the warm and wet summer seasons from our analysis, this error is not present anymore.

Line 249 “the average soil moisture availability” do you mean the “high soil moisture availability”?

Thank you for spotting this. Indeed, we meant “high soil moisture availability”.

Fig 14, please add an explanation of why this selection of warm and dry years.

As our classification has changed to warm years only, we added all subfigures to this panel plot.

Section 6 should be section 5.

After the separation of the discussion and conclusions, the section numbering will be adjusted accordingly. The discussion is now section 5, and the summary is now the final section 6.

Paragraph starting in line 384, this is just the justification for using ERA5 in the analysis but not the discussion of how uncertainties in ERA5 like the overestimation of LH (Martens et al., 2020) could be affecting these results. This paragraph should probably be placed in section 2.1.

Following your suggestion, we moved the first half of this paragraph to section 2.1.

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