

I'm pleased to note that the authors have made a great effort to improve some sections of the manuscript following my suggestions. The scientific quality and interest of the analysis has been improved and the conceptualization of the research problem refined. I'm pleased to see that they have included an interannual analysis that complements the results that were already in the manuscript. The metrics are now better described in the data and methods section and the Figures have also been improved.

However, the narrative is still very confusing. It's not only the poor English writing that represents a barrier for an effective and enjoyable reading process. The manuscript text lacks a fluid narrative, a structured reasoning and a solid storyline. Many sections throughout the manuscript appear completely out of context. Redundant statements are often found and the reader finds himself lost trying to follow the narrative several times. There isn't a proper connection between the results and conclusions and so, in the end, it's difficult to wrap up all things and to get an overall picture of the analysis. Unfortunately, these are all aspects that were pointed out in my last review.

Another major concern is related with the manuscript's size. An effort should be made to release the narrative from analysis and discussions that are not strictly necessary to fulfil the objectives. They can be either removed or moved to the supplementary material. More details about this issue are provided in my minor comments below.

My last major comment relates to the poor structuring of some chapters of the manuscript. For instance, the Abstract covers in a very superficial way the goals, the main results and the relevance of the paper. More details about the motivation and the importance of the analysis given the results obtained is absolutely essential. The second part of the Introduction chapter is supported in isolated paragraphs with a fragile connection between them. Some sentences are extremely confusing and written with a poor scientific language. The Discussion chapter presents a muddled narrative that fails to highlight the main findings of the analysis and their implications. Similarly to Introduction, it's not easy to find a connection between paragraphs. An effort must be made to reformulate all these chapters and to adopt a clear, fluid, convincing, objective and logical speech.

The authors mentioned that the text was proofread by a native English speaker, but many writing problems are still there. Considering all this, I think the manuscript is still not ready for publication. I've made a great effort to highlight all these problems in the following minor comments and so, I think it's fair to demand a last effort from the authors. The manuscript text must be deeply reformulated following the above describe suggestions and considering the minor issues described below. Otherwise, I will reject it for publication.

### **Minor Comments:**

**Line 10:** Change accordingly: "(...) intensity of these events, ~~their~~ its influence is typically (...)"

**Line 13:** Consider the following suggestion: "(...) between 1991-2022 over Central Europe ~~by means of ERA5 data.~~"

**Lines 28-31:** "This was also shown by Rousi et al. (2023) and Dirmeyer et al.(2021) for 2018, who suggest that these extreme conditions will be more likely under climate change conditions during

2020-2049 where two out of three summer seasons will experience hot and dry conditions in a +1.5°C warmer world which is already the case” The sentence is too long.

**Lines 33-34:** “(e.g., planetary boundary layer (PBL) height, convective available potential energy (CAPE), lifted condensation level (LCL)” a closing bracket is missing.

**Lines 53-54:** Change accordingly: “(...) simulations for the ~~period 1989-2008 for the~~ European summer seasons **between 1989 and 2008**.”

**Lines 59-60:** Change accordingly: “They identified a ~~coupling~~ hot spot region for the surface coupling ~~between~~ sensible and latent heat fluxes and **between** latent heat flux and 2m temperature in South Europe ~~while a~~. A transition zone is present over larger parts of Central Europe”.

**Line 64:65:** “While there was only little sensitivity over the northern part of this area, Central Europe and the British Isles showed a change in the coupling regime based on the convective triggering potential and low level humidity index (CTP-Hllow)”. This needs to be better explained.

**Lines 81-82:** “The analysis of Dirmeyer et al. (2021) for the 2018 European heatwave revealed enhanced soil moisture – near-surface feedback coupling under drought conditions”. What is exactly a “soil moisture–near-surface” coupling?

**Lines 86-89:** “According to Ossó et al. (2022), Europe already faced an increase in climate extremes since 2000 and will remain a hot spot for severe droughts (Huebener et al., 2017; van der Wiel et al., 2022) impacting not only summer’s crop yields (Toreti et al., 2022) but also affecting the generation of renewable energy”. Out of context. This paragraph is focused on discussing the soil moisture-temperature coupling. Try to find a better way to fit this information in the Introduction.

**Lines 91-93:** “Shifts in the hydrological conditions from energy- to moisture-limited conditions originating from droughts and heatwaves (Dirmeyer et al., 2021; Duan et al., 2020) or severe flooding (Lo et al., 2021) imply temporal variability in LA coupling at sub-seasonal to interannual time -scales.” What are these “hydrological conditions”? An effort must be made to make use of the right concepts.

**Lines 94-96:** “Additionally, the critical soil moisture thresholds (Dirmeyer et al., 95 2021; Rousi et al., 2023) suggest not only an intensification of the heat and drought conditions by LA coupling over Europe but also a strengthening of the coupling itself” I’m not getting what you’re trying so say here. Please clarify.

**Lines 101-102:** “However, a quantification of the temporal variability in different coupling relationships and the associated impacts of the variability still lack, as LA coupling strength on other time scales than climate periods has been barely investigated over Central Europe so far. The same applies to shifts between coupling regimes due to variability in the climatic conditions.” Try to be more explicit here and to use a proper scientific language.

**Lines 125:** Remove the word “framework”.

**Lines 130-131:** Change accordingly: “For our analysis, we used volumetric root zone soil moisture  $\eta$ , defined as weighted sum of the soil moisture in the top three soil layers of ERA5 ~~down to 1 m below the surface~~, LH and SH, CAPE, and PBL height (PBLH).

**Lines 133-135:** “As HLCL was not available from ERA5, we used the approach from Georgakakos and Bras (1984) and Bolton (1980) which is based on surface pressure, 2m 135 temperature, and 2m dewpoint to derive HLCL which is also applied in Dirmeyer et al..” This needs to be rewritten.

**Lines 145-149:** Be more precise and objective. There’s no need to give all these details. It only brings more confusion.

**Line 158:** Change accordingly: “(...) The summer seasons **of** 2003 and 2022 are (...)” This works for all the other parts of the manuscript where this issue occurs.

**Line 161-162:** “A trend towards larger dewpoint depression is also observed here since 2015”. Visually I’m not sure about this. Also, you must have in your hands some results to prove this. I suggest the authors to compute the linear trends before getting these conclusions. This also applies for soil moisture and temperature.

**Line 166:** Remove “which will become more likely in the near future (Huebener et al., 2017; Rousi et al., 2022)”. This sounds more like a discussion of your results.

**Lines 168-170:** Why do you start the analysis of Figure 2 with panel b)? I suggest changing the order of panel a) with panel b) if you want to keep the text as it is right now.

**Lines 172-173:** Change accordingly: “The median of TCIn-LH (Fig. 2a) shows higher values for the warmer summer seasons (see Fig. 1b)”

**Line 176:** “However, during the warm and dry years a trend of ACILH-HLCL approaching values around or below zero is evident.” The word “trend” assumes a long-term changing pattern... Here you’re just saying that for specific periods the ACILH-HLCL reaches low values...

**Figure 2 caption:** “The bold-faced numbers indicate the fraction of grid cells exceeding the 75th percentile of the respective index”. The numbers that are not in bold refer to what?

**Lines 187-188:** “For the ACILH-CAPE (Fig. 2d) no clear trend for an increase or decrease can be observed which could give a hint that also the large-scale weather pattern can play a reasonable role in this case.” I get what you’re trying to say here but this needs to be explained more clearly.

**Lines 180-183:** Change accordingly: “(...) Based on the interannual variabilities shown in Figs 1 and 2, we therefore decided to focus on summer seasons which have a median 2m temperature anomaly of more than 0.5°C (Table 1). ~~which is proven to be a realistic estimate for changes of the maximum temperatures over land in the last decade (Forster et al., 2023). All anomalies were calculated using the Climate Data Operators (CDO) version 2.0.5 (Schulzweida, 2022).~~”

**Table 1:** These are annual anomalies or anomalies just for the summer periods?

**Lines 186-187:** “combined with a reduced atmospheric water availability” How do the authors know this?

**Lines 188-190:** Change accordingly: “Although the median 2m temperature anomaly for summer 2020 was only 0.4 °C, it ~~was~~ considered in our analysis ~~considering that it as this~~ was the only summer ~~since 2015 witnessing a with a moderate observed~~ positive precipitation anomaly according to both ERA5 and E-OBS datasets (Table 1)~~bias since 2015~~”

**Line 191:** Consider changing to: “3.2 Meteorological characterization of warm and dry summers”.

**Lines 192-195:** Remove this text section.

**Line 196:** I would remove the analysis of the 500hPa Geopotential. The goal of this paper is to characterize the land-atmosphere interactions and not the anomalous circulation patterns associated to droughts and heatwaves. It only makes sense to keep this if a strong connection with the land-atmosphere processes is made (which is not the case). The contribution of these results for the overall analysis is residual. If you want to keep this, I suggest moving it to supplementary material. This would also contribute to a slight and welcome reduction of the manuscript size.

**Line 196:** It is the geopotential height.

**Figure 4 caption:** “The top left panel shows the mean summer 2m temperature 1991-2020 from ERA5”. Rewrite this please.

**Line 224-225:** “with a median precipitation anomaly between -34 mm and -63 mm”. This is a spatial median right? If so, It needs to be explicit.

**Lines 225-226:** I would move the E\_OBS anomalies to supplementary Material. It only makes sense to keep in the main body text the results obtained using ERA5 considering that the LA coupling metrics were only computed using data from this reanalysis product. It’s a matter of keeping some coherence. Of course, it’s always nice to have results from E\_OBS but they should only be used as a complement to prove that ERA5 follows quite well observations.

**Line 245:** Remove the word “amount”.

**Lines 246-248:** Super confusing.

**Figure 7 caption:** Similarly to Figure 4 caption rewrite the following sentence please: “The top left panel denotes the summer mean root zone soil moisture 1991-2020 from ERA5”

**Line 259:** “spatial patterns and the spatial extent of warm or cool as well as moist or dry anomalies” Rewrite this please

**Lines 260-261:** “Firstly, 2003, 2015, 2018, 2019, and 2022 stand out the most” It reads weird

**Lines 261-262:** “They are characterized by large (**warm?**) temperature anomalies, dry anomalies in soil moisture and precipitation extent over most of the land areas in our study domain”. The dry anomalies in SM and precipitation are regarding the absolute values or the spatial extent? It is not clear.

**Lines 270-272:** “A positive TCI<sub>η</sub>-LH denotes that LH is limited by the root zone soil moisture and the soil moisture variation results in LH variation while a negative TCI<sub>η</sub>-LH indicates that the

development of LH is energy limited, i.e., the incoming energy determines the LH development”  
Very long sentence. Break in two pls.

**Lines 275-276:** Change accordingly: (...) ~~the analysis was all analyses based~~ on daytime means computed for the period 06 UTC and 18 UTC of each day (Yin et al., 2023)

**Lines 277-278:** Change accordingly: “Figure 8 shows the **mean spatial pattern of** TCI  $\eta$ -LH **observed for the previously selected** ~~of all~~ warm and dry summer seasons. ~~shown in Table 1 which became the dominant situation over Europe since 2015.~~”

**Lines 282-284:** The authors need to explain this better.

**Lines 300-301:** “This is related to the anomalously warm and dry conditions in the atmosphere and a soil moisture deficit during these” (...???).

**Lines 301-303:** “The SH increases due to a reduction of the evaporative cooling effect at the surface, and the consequent increase in the temperature gradient between land surface and atmosphere”. Please structure your reasoning better.

**Lines 306-307:** “In 2017, the spring season showed a positive soil moisture anomaly over Germany, East Europe and the British Isles which is reflected in the strong correlation over these regions. What are the supplementary figures and the manuscript figure showing this?”

**Line 315:** Remove the following: “building a bridge toward convective processes”.

**Line 318-324:** Read carefully this section. I think there’s an incorrect use of the acronyms LCL and HLCL.

**Line 327:** “(...) Simultaneously, the LCL deficit is negative (Fig. 12) leading (...). Should be Fig. 11 right?”

**Lines 331-332:** “This is the area in the study domain facing considerable interannual variability, which is reflected in sign changes, among other things”. Explain this better. What are the “among other things”?

**Line 336:** (...) negative values in the ACILH-HLCL (Fig 11a, e, f, i)”. Should be Figure 10 right?

**Line 341:** This is also shown by the negative values of the TCI $\eta$ -LH-HLCL. What is the TCI $\eta$ -LH-HLCL? I would suggest to remove this from the paper. Your results and already self-explanatory and the manuscript is super extensive.

**Line 342-348:** “Please note that the SH is always positively correlated with the PBLH over land and doesn’t experience strong interannual variability (not shown). This implies that a strong increase in the SH due to the LH limitation causes strong PBL heating and growth. This in turn pushes both the PBLH and the HLCL upward. Due to the combination of strengthened PBL heating and decreased PBL moistening the HLCL rises further, which leads to an intensification of the LCL deficit and thus inhibiting deep moist convection (Santanello et al., 2011). The areas with the strongest changes in the signal converge with the regions experiencing the strongest warm and dry anomalies (compare Fig. 3j, Fig. 5j, and Fig. 7j).” Remove this. It only brings more confusion.

**Lines 357-370:** Please rewrite all this section. In fact, I would suggest a deep reformulation of all this chapter. The reader finds himself lost frequently and it's super hard to follow a logic narrative or a solid storyline. The authors really need to structure better their ideas and made a real effort to expose them in a more effective and organized way. There's a total confusion of Figures, metrics that are not defined and you're continuously switching the region or the period under discussion. This is not the right way to interpretate the analysis. Also, and as I suggest previously, there's a lot of supplementary material that honestly, I think it's unnecessary and it only brings more complexity.

**Line 373:** Change accordingly: "This index aims ~~at~~ to assessing the(...)"

**Lines 381-383:** Please remove the following sentence: "CAPE depends on the atmospheric humidity which is, among others, related to LH while LH is related to the atmospheric temperature, humidity, soil moisture and LAI."

**Lines 387-388:** "Together with a temperature gradient of up to 30 °C or more in the Mediterranean between 850 hPa and 500 hPa (not shown), this leads to stronger atmospheric instability and thus reduced coupling to LH." However, you mention that these regions are defined by large evaporation rates... Thus, a correlation with LH should be visible, right?

**Lines 394-395:** "Over Germany and France, mostly only weak coupling is seen with stronger signals during 2003 and 2019". Poor English

**Lines 395-405:** Rewrite all this section please. There's a total mixture of concepts, physical relations, etc. In fact, it sounds a bit out of context... An effort should be made to better connect this information with the link between LH and CAPE.

**Line 408:** remove the following sentence: "We now discuss the key findings".

**Line 409-415:** Please rewrite all this paragraph. Again, try to expose your arguments more clearly and in a logical way.

**Line 417 and 421:** No trend was discussed or presented in Fig.2 Please use another word to describe your point of view. This follows one of my previous comments.

**Lines 25-426:** "atmospheric stratification which is not only impacted by the surface conditions but also by the large-scale weather pattern and atmospheric stratification". Rewrite this pls.

**Line 424-426:** Rephrase it.

**Line 435-437:** These precipitation deficits could only be **partially** explained by changes in the soil moisture-evaporation regimes and soil moisture-precipitation coupling. Only a small fraction of precipitation results from local moisture recycling processes. The other fraction is explained by moisture convergence and transport of water vapour from remote regions. Also, this is something observed over a long-term period of for some specific years/summers? Please clarify this.

**Line 339-341:** How can the authors get this conclusion from what is said in the previous sentences? Is this supported by any kind of climate projections?

**Line 450:** “These regions are usually water-limited leading to limited evapotranspiration thus further reducing LH”. In addition to the poor writing quality, I’m not seeing how a water-limited regime leads necessarily to lower LH....

**Line 454-455:** “Though not yet represented in the model, in reality, this results in a low LAI which is often the case in South Europe” I got lost here.

**Line 471-475:** Rewrite all this please.

**Lines 489-510:** All this sounds out of context and extremely confusing. Most of this information doesn’t add anything relevant. The authors really need to reformulate this section and to find a better way to fit this information in the context of the analysis.

**Line 516-518:** “Firstly, the interannual variability between all years of the period was examined in the context of prevailing temperature and moisture anomalies in the light of a warming climate and a projected increase in hot and dry periods until 2100”. Rewrite