The influence of soil moisture on the heavy precipitation event in July 2021 in Western Europe

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Response to Comments by Reviewers

We thank both reviewers for their insightful comments. We have responded to all comments individually below. The reviewers' comments are repeated in bold font, our responses are given in regular font and (modified) parts of the manuscript are set in italicized font.

Reviewer 1

1. **Role of moisture on low pressure system: I think this could be investigated more thoroughly. I am concerned that Fig.5 is misleading. I took the main message as the differences between the CTL, WET, and DRY ensembles – but this is not what is most obvious in the way the figure is presented. I am not sure the linear fits are useful – as a majority are not significant fits. Perhaps instead showing the difference in the spatial patterns of SLP and Z500 (in a similar way to Fig. 3) would be more useful. I would also be interested in seeing the difference in vertical velocity between the ensemble. In Line 213 processes are hypothesised – this could be tested more quantitively. Further assessment of the differences between SLP, Z500, and vertical velocity in the three ensembles could strengthen the conclusions.**

Reply: We refrained from mapping these changes in the preparation, because we expected that the locational shift of the low across members would prevent meaningful composites. However, it turns out that we do find clear signals in pressure and vertical velocity. We would add these as a new figure and collapse Figure 5 into a single point cloud, because the differences between experiments are still interesting without the fits.

With respect to line 213, we performed some computations on the focus region of the deterministic runs: During the event, i.e., 13th and 14th of July, boundary layer specific humidity only decreases marginally in the dry run, because of moisture convergence, but the decreased evaporation means that the mixed layer warms by about 1.5 K. Therefore, relative humidity drops and the lifting condensation level rises by about 200 m. Consequently, the level of free convection increases by a comparable amount (where it exists). The effect on parcel buoyancy depends on the date. As can be seen in Figure 4c, during the first 3 days of the simulation CAPE is reduced in the dry run precisely because the air is drier. With the onset of the event moisture convergence into the area of interest compensates for the moisture deficit. So parcel buoyancy does not decrease where it matters, but convective initiation is still suppressed. The entrainment argument is difficult to assess quantitatively, but since the changed surface flux partitioning and the warming of the boundary layer suggests a deeper boundary layer, one would expect increased entrainment. We would add these details to the cited lines.

2. **A statement about the wider implications of the findings of this study in relation to our current changing climate would be useful. As summer soil moisture over Europe is projected to decrease, this study would suggest that we should see less extreme rainfall - contrary to much other literature. Comments about what the results mean for society / policy makers, and greater explanation of outstanding research questions, would be beneficial. This could fit both within the conclusions, and to better motivate the study within the introduction.**

Reply: This is a good point. According to cited literature, evaporation was at normal levels. Consequently, the event was not extreme because of unusually high moisture supply. While the system is close to the energy limited regime in this case, the expectation is (as you say) that evaporation will be moisture limited more often in future European summers. This is precisely when continental moisture supply plays a larger role compared to the rest of the year. For these reasons, one might indeed see an opposing effect. However, our conclusions are of course limited to this case only and we are not sure to what extent this may generalize. Therefore, we do not think that we are at a point where we can draw conclusions for society. This feeds into the outstanding research questions, where we also see a need to test realistic soil moisture scenarios in suitable events and put that in relation to pseudo global warming storylines. We will add these aspects to the introduction and conclusions.

Minor comments:

- **Consider the use of ‘rainfall’ rather than precipitation throughout, as in this case study it is only rainfall, in situations of snow the conditions/processes may differ.**
- **Abstract – line 14: ‘more precipitation’, this could be more accurately stated – greater magnitude (as opposed to greater area / greater timescale).**
- **Introduction – line 20/23: Tradowsky et al (2023) and Kreienkamp et al (2021) are essentially the same study – just one is the rapid WWA report and one a**

slower, extended, study. In the text it makes it appear as if the two are more independent than they truly are.

- **Introduction line 37 - Relevant reference (apologies for self-citation): Thompson et al 2026 The role of subtle dynamical changes in the European extreme rainfall of July 2021 | Climate Dynamics | - we included moisture tracking using LAGRANTO. This study includes analysis of the case study with several different model set-ups (ensemble boosting, PGW, ECMWF forecasts) so may be relevant elsewhere too – but we did not assess the role of soil moisture itself.**
- **Fig.2 caption: it's should be 'its'.**
- **Fig.2 It took me a moment to realise 1.7 etc. Meant 1 Jul, perhaps label as 'date'? Or re-label as 'number of days before event'**
- **Line 146 : its source region (no apostrophe)**
- **Line 168 – As the ensemble is initialised from a specific state it will not fully incorporate the plausible range of natural variability. I do agree that including multiple ensemble members is very useful, but not for the reason outlined here – more because very small differences in the dynamics could cause larger differences in precip.**
- **Line 199: grammar (colon followed by capital)**

Reply: We agree with all minor comments and will make changes accordingly. Also, your 2026 paper is indeed a good reference, which we were not aware of at the time of writing the introduction.

Reviewer 2

Specific examples of statistics:

- **While I understand the use of the total accumulated rain over both time and space (giving a number order 10^13) -- since the aim is to track the total amount of water to it's source -- it is also quite difficult to understand and since I could not find the area size, I was also unable to estimate how much rainfall that is in mm units. Despite that observations are uncertain, I still think it is good to give an observational estimate.**

Reply*:* This relates to Figures 2, 4 and 5 . We will change the timeseries to show area averaged rainfall in units of mm/h as requested. In terms of observations, the best possible comparison here is the German national weather service's RADOLAN dataset. Unfortunately, it does not cover the full focus region, so we can only compare where both data sets overlap. Therefore, and also because we would like avoid adding another line to the already busy time series in Figure 4, we would add the comparison with RADOLAN as a statement:

"ICON-DREAM slightly overestimates rainfall amounts in the region compared to observational data. As comparison, we use DWD's RADOLAN data set, which is based on radar data, which was adjusted to gauges. It covers about 80% of the focus region and shows about 6.5% less rain than ICON-DREAM. This is in accordance with Kracheletz et al. (2025, Figure 4)."

In our opinion, the above statement best fits the description of Figure 3, because this is the first time we discuss a comparison of rain amounts in the different scenarios.

- **I could not interpreted the soil moisture values in Fig 4d. A value of 1 kg/m2 corresponds to 1 mm of water. With typical volumetric values of soil moisture capacity (10-40%), this means you are looking a 1 cm of soil at most. Why not plot a soil wetness index of the first 1m of the soil, which is a typical parameter used in many studies.**

Reply*:* That is because we only considered the top level values, which is exactly 1cm deep in our land surface model. However, since transpiration is also important, we should indeed show a vertical average. Changing Figure 4d as suggested is no problem and we will do so in the revised version.

- **I could not figure out / understand the units of evaporation in Figure 4. I would expect the number to be much larger since if I multiply this by time I do not get the accumulated area summed rainfall amounts (order 10*13).**

Reply*:* This remark touches upon multiple aspects. First, the aggregation area in Figure 4e are regions that contributed moisture to the event, but we are showing the full time series of the relevant regions here. Therefore this is not one component of the tracking budget and it does not close with precipitation. Second, tracking only accounts for about 60% of water, the rest is outside of our tracking domain, but that is then also not related to European soils anymore. Third, since we are changing the rainfall units to mm/h, it would make sense to also change the evaporation units to mm/h for the comparison.

One thing we tested following your comment is to compute a weighted sum (instead of average) to check the orders of magnitude. That gives values on the order of 1e10 to 1e11 kg/h, which is close to the rainfall amounts and seems reasonable given the caveats we mentioned above. As we now changed the rainfall units though, we think it would be best to also have the evaporation in the same units. We would then also add the area information to the description, so that one can assess the plausibility of these numbers.

Specific examples of physics/physical interpretation:

- **I am unsure about the interpretation of the wet experiment. Setting soil moisture to saturation (in the whole root zone?) is in typical soil schemes not so different than setting it to the field capacity. In both cases, there is no stress from the plants, so you are in a energy-limited regime. The question is how far is the control experiment from field capacity, since if it is close than you would not expect a large increase in evaporation from the wet experiment. So, as stated above a soil wetness index (normalised soil moisture between field capacity and wilting point) could be more useful.**

Reply*:* In line with your previous comment we recomputed soil moisture as an index in Figure 4d, such that 0 corresponds to permanent wilting point and 1 to field capacity. Also, there was a bug in the evaluation code, where the regional mask had an error. In the corrected version, the actual soil moisture values in terms of a wetness index are about 0.9, i.e., quite close to field capacity and about half way between wilting point and saturation. We agree that setting the moisture to field capacity and saturation should be quite similar and we also argue that we are close to an energy limited regime. While after the correction of the bug the gap between control and wet is not as large as it seemed and we need to correct the related passages, we think that the general argument still holds.

- **While I liked the moisture tracking in the paper, as an outsider to this field, I am also a bit confused on how it is actually done and what it means as explained in section 2.3. What are the main assumptions there? Somehow, I do not understand how you get from rainfall accumulation (something like 100 mm) to tagged moisture (a portion of the actual moisture?), to tagged moisture transport, and evaporation from the source area. What do you assume here ? I also tried to look at the reference to method, but that did not help me very much without going to the details of that paper. So, if you could provide a bit more informatation that may guide the reader who is not familiar with the method. I guess that the procedure assumes the same dynamics (a suggested on line 114), meaning that subtle changes in dynamics could strong alter the**

result. In that sense, the moisture leading to precipitation should come from somewhere, but if you remove that source of moisture the atmosphere will subtle adjust to get the moisture from a different area (e.g. because of dryer air leading to more evaporation elsewhere or subtle dynamical adjustment). In that sense I liked the comparison between the moisture tracking showing that the EU continent is a main source, and the model experiment showing indeed large response of rainfall to dryer soil conditions.

Reply: We believe the point about the dynamical adjustment might be a misunderstanding, but it highlights that we should indeed take better care to explain the method. We suggest the following expansion of lines 111 to 115 (new text is set in italic):

"As the name suggests, the vertical extent of the atmosphere is divided into an upper and a lower layer. The model then calculates the amount of water exchanged between grid cells as well as the two vertical layers and keeps track of the moisture~~, which was "tagged" at the start of the tracking procedure.~~

Since we are tracking the moisture back to its origin, precipitated moisture is attributed to evaporation that occurred earlier along the moisture's path. Since WAM2layers uses the Eulerian perspective, it is not following discrete parcels, but tracks a tagged fraction of the moisture in each grid cell and layer.

It is assumed that the ~~horizontal~~ transport of tagged moisture is proportional to the overall moisture transport.

At the start, this fraction increases as precipitation occurs within the tagging region and then monotonously decreases wherever evaporation occurs outside the tagging region along the moisture's path."

We hope this sufficiently clarifies the connection between rainfall, tagging, transport and evaporation. One of the main assumptions is the propagation with mean flow, which is stated in line 114 and you reference. Another implicit assumption is that the simplification into two layers is sufficient. Others are discussed in lines 123 to 128. However, there are no assumptions about dynamics. The tracking is done individually on data produced by the simulations of the three scenarios. The respective atmospheric fields are self-consistent so dynamic adjustments caused by removing moisture are accounted for within the simulation. The tracking is applied on the results. To clarify this we would add this statement to line 122:

"We apply WAM2layers separately to each simulation, so that the tracked moisture transport reflects the circulation realized in that particular simulation."

- **I think it would be benificial to also show a few more normal statistics, like evaporation, for instance by showing a map of evaporation alongside Figure 6 (or in the supplement). Figures of sensible heat flux and temperature would also help to understand the results better as well as perhaps humidity. A 5% drop in relative humidity and a 2 degree warming would likely be associated to**

increases in absolute humidity (not decrease), so I was also a bit unsure here on how the results looked.

Reply: The tracking results in Figure 6 aggregate over a span of about two weeks and are not the result of a standard statistic. Although it is not a problem to show additional information, we then would not want to include it in Figure 6 to avoid confusion. In terms of what we can show we are limited by the saved simulation output. Information used in the tracking, like evaporation and precipitation was saved for the whole simulation, additional variables only starting on the 10th to save disk space. Following your comment we produced the following figures, which we would add to the supplement:

1. Evaporation, Wet - Ctl and Dry - Ctl averaged for the whole simulation period
2. Evaporation - Precipitation (to show atmospheric moisture sinks and sources), separate for all the experiments averaged over the whole period
3. Lowest level specific humidity, Wet - Ctl and Dry - Ctl averaged over 10.7. to 15.7.
4. 2m temperature, Wet - Ctl and Dry - Ctl averaged over 10.7. to 15.7.

With respect to your comment, the figures show the following patterns: In the wet (deterministic) run, specific humidity increases while the temperature signal is mixed but small in magnitude. Relative humidity mostly increases by a small amount. In the dry (deterministic) run, specific humidity mostly decreases, temperature increases and relative humidity decreases.

- **line 253-257. see also first remark. Given that you do not know how far the control spoil moisture is from field capacity it is hard to make an interpretation here.**

Reply: These lines need to be updated in the light of what was discussed above. We think the general conclusion remains that we are close to an energy limited regime. Since soil moisture is actually quite close to field capacity, one should not expect a large response from adding water as you pointed out above. We would keep the citation of the paper by Senerviratne et al. (2010) in this passage. It references a critical soil moisture value as transition between energy and water limited regimes, which we understand to not be identical with field capacity. The critical value would have to be estimated from a longer time series, we do not know the actual value. We think this is a useful caveat. We suggest this new version:

“Seneviratne et al. (2010) describe the transition between energy and water limited evapotranspiration regimes. While we do not know the critical value at which the transition takes place, we can expect that it should be close to field capacity. Since Fig. 4d demonstrates that actual soil moisture is close to that level, increasing soil moisture further does not increase evapotranspiration as much as drying the soils does. Since we only consider regions that contribute to the event's precipitation, this relationship also explains why the precipitation signal is stronger in the DRY scenario.“

Minor remarks:

- **Table 1. The table is really detailed (perhaps too), but hardly discussed in the text, and is basically very similar to the integral of Figure 6 over de different PRUDENCE areas (except that is percentage of the total). Do the areas include both land and sea ?**

Reply: The PRUDENCE regions are land only while the maritime regions only contain sea grid points. We would update the table caption to explicitly state this.

It is true that the table is not discussed much, but we think it is useful to quickly ascertain the importance of regions. Since the maps are simple geographic projections, areas are distorted and (for us) it is hard to estimate how much percentage of moisture individual regions might contribute based on the figure alone. Other studies of this event see different seas as large influences for this event. Referencing the table, one can quickly verify these claims. For this reason, we think that the table has some merit.

- **Figure 3. I am not a particular fan of the colour coding here as it is so hard to see differences in the order of a factor 2. And difference plots mainly show a spatial shift for the wet versus control. As I understand the box area is adjusted to compensate for a small shift as stated on line 141, but perhaps good to remind the reader here because it looks like that in the wet scenario the peak moves outside the box area.**

Reply: We chose these intervals initially, because they are the same as DWD's report on the event and it was easy for us to compare. There is no problem with changing the intervals to factors of two. Much of the signal for the wet run is a locational shift indeed, but not for the dry run. The idea is to make it easier to assess how large the differences are in actual numbers. The variation of the box was only done for Figure 2, because the free simulations develop substantial spatial deviations if the events happen at all. In the constrained runs we do not feel this is necessary. While the event is shifted in the wet run, we would not agree that the peak moves outside the box area.

- **Figure 4. I think it is quite interesting that the event basically comes into two burst on the 13'th and 14'th of July, and that in the second burst the WET simulation produces quite a bit more precipitation than the CTL (and DRY). Is that explainable in term of tagged moisture or perhaps just the normal evaporation, or can you difference in the dynamics (pressure) on the 14th? In this respect it is also good to remark the value of the ensemble. Some of the members are actually closer the observation, but the mean is rather low, which corresponds to the large sensitvity of system to relatively small pertrbations as other studies have found.**

Reply: You raise an interesting point. We have looked into the individual ensemble members, because the best comparison can be drawn member by member. Three of the wet ensemble members indeed show a much larger increase compared to the corresponding control run on the second day compared to the first day. Interestingly, those are members with strong intensification of the low pressure system on the 14th, but not on the 13th, which explains the rain response. In the other members, the effect on the two days is similar or in a few cases even slightly reversed. Since this is not a systematic effect, we think this is a consequence of the high sensitivity you referenced and the signal in Figure 4 is influenced by the large magnitude of the three mentioned members. We will include these aspects in the discussion of the simulation results where we discuss the dynamical changes. Since we are also changing the related Figure 5 by request of the other reviewer, this section will be partly rewritten so we do not have a concrete suggestion for how we plan to incorporate this discussion of your comment at this time.

- **L 262. I think the logic here is that it is energy limited since increasing the soil moisture has no effect. If it is already in partly soil moisture limited regime, removing more soil moisture will further decrease evaporation.**

Reply: Making the argument from the opposite direction is better, yes. We would change line 262 to:

"…and that evapotranspiration is energy-limited, as increasing soil moisture has little effect on surface moisture flux and ultimately in precipitation."

- **L 35. Related also to the discussion on the method. The suggestion that moisture comes from the American continent is hard to physically understand, and makes you doubt these methods (although you show later that this is not the case and results look perfectly fine). Also I do not understand the connection between discussing the role of American forrest, and finding no increase in evaporation in Europe.**

Reply: We are not quite sure we understand the first part of this comment correctly. Do you mean "makes *you* doubt these methods" as in "this seems hard to believe to *readers*"? Both papers (one is a response to the other) use different tracking algorithms both showing that water evaporated from North American forests contributed towards European rain. While it is a large distance it does not seem implausible to us that water travels such a long way. We would have the opportunity to check, but we do not track moisture all the way (we only account for about 60-70%). This would require extending the simulation period, but we care about European land first and foremost. Of course one can have an argument about how important that contribution is and this is also the debate between the two groups of authors. As we understand, their argument mostly is about whether evaporation amounts were unusually high or low, but that is besides our topic. What they do agree on is that evaporation rates across Europe were not abnormally large. There is no logical connection between the points, to us it was interesting to see how different studies highlight different source regions. That they find no elevated evaporation levels above Europe tells us that the contributed amounts we find are not unusual. This is interesting to us, because that means that soil moisture and the associated evaporation were necessary conditions, but “high precipitation amounts were not caused by high evaporation amounts”.