

Subject: Rebuttal for manuscript egusphere-2025-289

Dear Andrew Feldman,

Thank you for giving us the opportunity to revise our manuscript “Forest favours conditions for convective precipitation in the Mediterranean Basin”. We are grateful for all the time and work the reviewer put into their constructive and valuable feedback and we were happy to hear only minor revisions were necessary. The comments helped us to improve our manuscript.

Based on the reviewers' comments we made several textual changes. These changes help to improve the transparency and clarity of the manuscript. Second, we included a new plot in the appendix of our manuscript that highlights the dependency of the impact of land cover type on soil moisture content.

We hope that the revisions and clarifications we have made in response to the reviewers' comments have sufficiently addressed their concerns and improved the overall quality of the manuscript. We believe that the revised version is now suitable for publication in *Biogeosciences* and we look forward to your evaluation.

On behalf of all authors,
Kind regards,
Jolanda Theeuwes

Reviewers' and editor's comments:

First, we would like to state that we are very grateful that the reviewers and editor took their time to review our manuscript. We thank the reviewers and editor for their constructive feedback, which helped to improve the quality of our manuscript. Below the comments of the reviewers and editor are presented in blue. The authors' response is presented in black.

However, I have some concerns that the discussion of sample size in Section 2.4 is not appropriately balanced and needs to more accurately represent the results that are being presented. While the added breakdown of the sampling methodology is extremely helpful, it is misleading to claim a sample size of "57,360" when the majority of the figures in the paper are plotting average values for individual grid cells. In Figure 3, for example, the values being shown on the map are CAPE averaged over only 20 days in a 10 year period. While it may be true that the experiments have obtained a total of 57,360 data points, that is not the same as having 57,360 samples for the results being presented. Please be more careful about this.

We agree with the reviewer that we could be more transparent about this. In section 2.4 we included the following lines to improve this transparency (Lines 174-176):
“..., resulting- in a total of 20 model runs for each grid cell. The results are calculated for all samples after filtering (see Section 2.6). For the analyses on spatial variation, all samples per grid cell are used.”

Line 256: For ten different output variables, the anomalies between the forest scenario and bare soil scenario show the same spatial pattern (Fig. 3) which overlaps with the spatial variability of soil moisture content (Fig. 2).” Throughout the manuscript, the authors refer to “relatively dry regions” or “relative wet regions” between Figure 2 (soil moisture variability) and Figure 3 (anomalies of variables). I wonder if there is a better way to show explicitly differences between these dry or wet regions with their anomaly variables rather than visually comparing between the two figures? I recommend the authors to consider defining different thresholds for dry vs. wet soil moisture from Figure 2 and filtering out these dry vs. wet regions in their anomaly maps in Figure 3 to strengthen their arguments and conclusions within the Results section, especially when they step through each output variable's takeaways.

We agree with the reviewer that it would be valuable to explicitly show the differences between dry and wet regions. To account for this, we included a new figure in the appendix that shows the dependency of the data shown in Fig. 3 on soil moisture. For each of the plots in Fig. 3 we created an xy-plot with on the x-axis soil moisture and on the y-axis each of the variables shown in Fig. 3. This figure, which we include in the appendix as Fig. A8, shows a relationship between soil moisture and the difference between the forest and bare soil scenario for different output variables. In this figure, the title of each plot indicates the spearman rank correlation coefficient and its corresponding p-value. These statistics show for each of the variables a statistically significant relation. These correlation coefficients vary from 0.16 (weak correlation) to 0.61 (moderate correlation).

We introduce this figure in the main text in lines 263-264: “The difference between the forest and bare soil scenario for the different output variables of CLASS all statistically correlate to soil moisture, with the magnitude of the spearman rank correlation coefficients varying between 0.16 and 0.61 (both positive and negative correlations) (Fig. A8).”

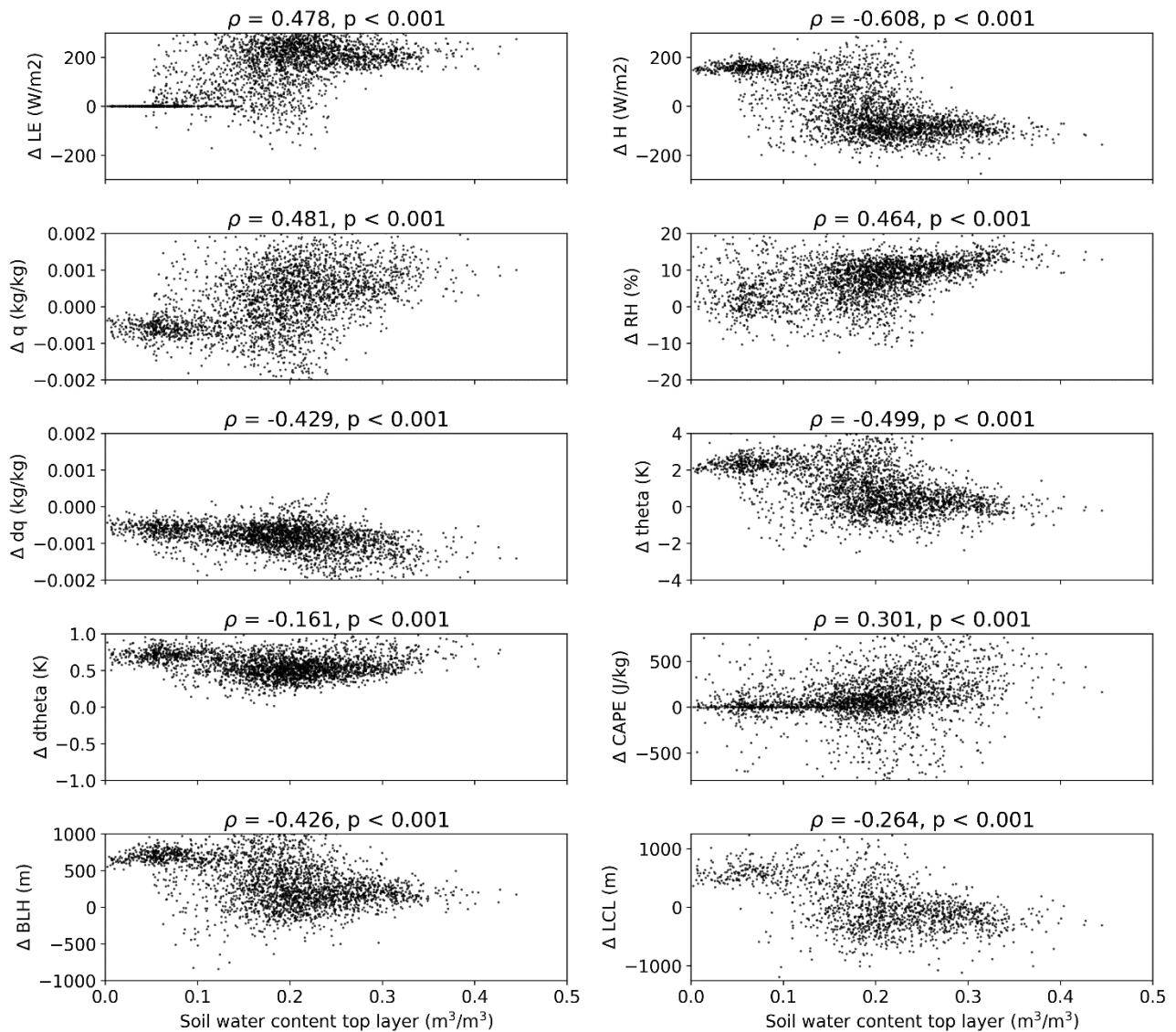


Figure A8: The difference between the forest scenario and bare soil scenario for different output variables of CLASS plotted as function of soil moisture content of the top soil layer. Each dot represents one model run. The title of each plot shows the spearman rank correlation coefficient (ρ) and the corresponding p-value. The output that is shown is the latent heat flux (LE), sensible heat flux (H), specific humidity (q), relative humidity (RH), jump in specific humidity at the top of the boundary layer (dq), potential temperature (theta), jump in potential temperature at the top of the boundary layer (dtheta), convective available potential energy (CAPE), boundary layer height (BLH), and lifting condensation level (LCL).

Figure 4 and 5 refer to percentage of samples as a diagnostic to their conclusions between Bare Soil and Forest to conclude that “convective rainfall potential are larger over forest than over bare soil (Line 517)”. Can the authors provide more discussion on the significance of these results?

We thank the reviewer for this comment. For Figs. 4 and 5 we see that most grid cells do not have a rainfall potential. The percentage of grid cells that has a rainfall potential is in the order of 1% and 10%. Despite that the number of samples with a rainfall potential may be low, there seems to be a clear relationship between the number of samples with a rainfall potential and the land cover type and soil moisture. This is also supported by the bottom plots in Figs. 4 and 5 that show the spatial variation. These plots show clearly a larger number of grid cells with a rainfall potential over forest compared to bare soil and over the high soil moisture scenario compared to the low soil moisture scenario.

To address this comment, we added the following lines in the results section (lines 312 - 317): “Nevertheless, we see that most grid cells do not have a rainfall potential. The percentage of grid cells that has a rainfall potential is in the order of 1% and 10%. Despite that the number of samples with a rainfall potential may be low, there seems to be a clear relationship between the number of samples with a rainfall potential and the land cover type, i.e., a higher number of samples with rainfall potential over forest than over bare soil. This is also supported by the plot that shows the spatial variation, which indicates a larger number of grid cells with a rainfall potential over forest compared to bare soil.”

Line 313: “Additionally, most grid cells have a CIN well below 100 J kg⁻¹ for both land cover scenarios (Fig. A11), suggesting that inversions do not play a major role in preventing deep convection.” This sentence is potentially misleading as it attributes the lack of inhibition to the absence of inversions, rather than to their strength. Inversions can exist with varying magnitudes: weaker inversions correspond to smaller CIN values and and convective potential, whereas strong inversions can yield large CIN and effectively suppress convection. Also, for grid cells with rainfall potential, the presence of low CIN is expected, as parcels that overcome the inhibition can access large CAPE. I recommend the authors to consider rewording this statement, and others on CIN in the Discussion section, to clarify that it is the weakness of the inversions (reflected in the small CIN values), rather than their presence/absence, that allows convection to occur.

We thank the reviewer for pointing this out. We agree with the reviewer that this should be rephrased. We rephrased the sentence as follows: “Additionally, most grid cells have a CIN well below 100 Jkg⁻¹ for both land cover scenarios (Fig. A11), suggesting that inversions may be too weak to prevent deep convection.”

Line 367: As for most grid cells CIN is small, it is expected that it affects the convective rainfall potential only little.” Please specify what “small” CIN means here.

We agree with the reviewer that referring to it a s “small” is unclear. We included “(<100 J/kg)” to clarify what is meant by small.

it would be best to qualify the statement about 400J/kg of CAPE. It is stated with too much certainty and it is unlikely this is always the case given cloud microphysics and other conditions.

We agree that there may be other processes that affect convection such as microphysics. To account for this comment we emphasize that this is an assumption in our study. We included the following lines in the introduction of our manuscript (lines 66-70):

“For the development of deep convective clouds that can produce rainfall, it has been suggested that CAPE needs to be equal to or larger than 400 J kg⁻¹(Yin et al., 2015). However, different processes may affect the amount of CAPE that is necessary to trigger deep convection.

Nevertheless, to determine the convective rainfall potential we also evaluate CAPE and assume that 400 J kg^{-1} is a sufficient amount of CAPE to trigger deep convection."

Finally, one remaining issue that is still not clear to me with Figures 4 and 5 bottom maps: are these tested conditions the case where all else is held equal? For example, in figure 4, does each grid cell have soil moisture and radiation dynamics similar to its true environmental conditions, but only with the land cover altered? Similarly for figure 5, does the land cover map reflect reality, but only soil moisture is altered? It is a bit challenging to find this in the methods and, in general, these critical details should be stated in the caption.

We thank the editor for pointing this out. Fig. 4 shows the rainfall potential for the bare soil and forest scenario. These results are obtained from the model runs in which only the parameters that are related to land cover are varied. All other variables remain similar to the realistic situation and are obtained (directly or indirectly) from ERA5. Fig. 5 has similar conditions. However, for Fig. 5 the soil moisture content is varied across the different scenarios. As a result for each of the scenarios soil moisture is constant within the study region. Between the different scenarios soil moisture content varies with scenario. The results in Fig.5 are obtained for a full coverage in forest. Hence all parameters related to land cover represent forest. Finally, all other variables remain similar to the realistic situation and are obtained from ERA5.

To clarify this in the manuscript we add the following lines to the captions of Fig.4 and Fig. 5.

Caption Fig. 4: "These results are obtained from the model runs in which only the parameters that are related to land cover are varied. All other variables remain similar to the realistic situation and are obtained (directly or indirectly) from ERA5."

Caption Fig. 5: "These results are obtained from the model runs in which soil moisture content is varied among the different cases. Land cover parameters represent the forest scenario. All other variables remain similar to the realistic situation and are obtained (directly or indirectly) from ERA5."