Response to reviewer comment from reviewer #1 for manuscript egusphere-2025-289

In this initial response, we aim to briefly address the main issues raised by the reviewer to facilitate a prompt exchange and encourage discussion. Our focus is on the key concerns to ensure an efficient and constructive dialogue. If the discussion concludes positively and we are invited to revise our manuscript based on the reviewers' feedback, we will then provide a detailed response addressing all the points raised in the rebuttal.

> This study simulates the impact of land cover change and soil moisture availability on boundary layer development in the Mediterranean Basin using the Chemistry Land-surface Atmosphere Soil Slab (CLASS) model. By comparing CAPE and ABL height across different land cover and soil moisture scenarios, the authors determine that convective rainfall potential increases when vegetation fraction is increased over wet regions and increases linearly with soil moisture content. While the results of the experiment and its design are interesting and compelling contributions to the land-atmosphere interactions literature, I have some major reservations about the framing of the study around "forestation" as a potential climate mitigation strategy. I recommend that the manuscript undergo major revisions to reevaluate and clarify its research goals and interpretations.

We are thankful that the reviewer took the time to provide us with constructive feedback that will help to improve the manuscript. We also thank the reviewer for expressing this study is an interesting and compelling contribution to the land-atmosphere interactions literature.

Framing and language

> Motivation: The abstract motivates the study by first identifying the Mediterranean Basin as a "climate change hotspot" that may be "prone to future drying." The authors then follow up this statement by noting that "Previous studies indicate the effect of forests on precipitation remains unclear for the Mediterranean Basin" before diving into a description of the study. The link between climate change, future drying, and vegetation-precipitation coupling is not clear at all from these two sentences. The introduction does marginally better in explaining "forestation may increase freshwater availability" and "forestation… may enhance rainfall." What's missing is the underlying implication that either A) we expect forestation to occur in this region or B) forestation is being considered as a climate mitigation strategy. If you decide to stick with the forestation angle, please explain and expand on this instead of leaving the readers to try to connect the dots. Assuming B based on the discussion of regreening later in the paper, how realistic is this strategy and how seriously is it being considered for the region? How confident are we that the Mediterranean will experience drying given that hydrological trends tend to experience large disagreement between models? Some important context is missing.

We agree with the reviewer that the motivation could be clarified both in the abstract and introduction and we thank the reviewer for this comment. We will clarify the motivation for this study and include that forestation is being considered as a climate mitigation strategy. We will clarify that the hydrological effects of forestation should not be overlooked as they may have positive or negative effects on local freshwater availability. With our study we aim to get a better understanding of where forest may have positive hydrological effects (i.e., an increase in convective rainfall potential), and where is may have negative hydrological effects (i.e., a decrease in rainfall potential and expected drying due to enhanced evapotranspiration).

Furthermore, the reviewer raises several questions about the confidence of the projected drying in Mediterranean regions and whether regreening is seriously considered for this region. First, while we agree that the projections of climate models have uncertainty, it is highlighted by the IPCC that ongoing warming has been observed in the Mediterranean Basin already affecting the ecosystems here (Ali et al., 2022). Furthermore, with high confidence it has been estimated that warming in this Mediterranean region has exceeded global average rates as well as that temperature extremes and heatwaves have increased in intensity, number, and length, particularly during summer (Ali et al., 2022). We will add this reference to the discussion. Second, while covering the entire Mediterranean Basin in forest is highly unrealistic and undesirable, there are projects that aim to restore forests in the Mediterranean Basin (e.g., https://www.decadeonrestoration.org/restoring-mediterranean-forests, https://www.unep.org/news-and-stories/story/fighting-fire-forests-across-mediterranean). Additionally, research shows that forest management (including afforestation) can improve carbon sequestration to cope with climate change (Ruiz-Peinado et al., 2017). We will include this information and these references in our manuscript. We believe that this will improve the motivation for our research, and therefore, we would like to thank the reviewer for their comment.

> **Defining and Interpreting "Forestation":** It is not clear to me whether the study actually addresses the question of how forestation would affect rainfall in the region. "Forestation" and other terms like "regreening" and "restoration" that are used liberally in the paper are 1) not well defined and 2) typically imply that there is a gradual increase in vegetation cover over some timescale that takes into account the planting and growing process. Instead, what this study does is answer the question of "how convective rainfall potential would be different over the Mediterranean Basin if the region were covered in forest" by dramatically altering the land cover properties of the grid cells across different model runs (i.e., a sensitivity study). There are no dynamical considerations in the experiment setup, so I am not confident the results can be interpreted as the climate response to "an increase in forest cover," at least not in the context of any real-world replanting strategy. In other parts of the paper, the authors describe the results using phrases like "The differences in boundary layer characteristics between the forest and bare soil scenarios show significant spatial variation," which is much more accurate (L199). While these results certainly have important implications for the use of forestation as a climate mitigation strategy, the title and language misrepresent the study's scope. Perhaps I am misunderstanding what "forestation" means to the authors, but I think the study would fit much better in a vegetation-precipitation/land-atmosphere coupling context rather than with the current climate change angle.

The reviewer points out that "forestation", "regreening", and "restoration" are used liberally in the paper and that they are not well defined. We thank the reviewer for pointing this out and agree that we should clarify these concepts when revising the manuscript. In addition, the reviewer correctly states that forestation results in a gradual increase in vegetation cover and not in a sudden increase in vegetation cover as was modelled in this study. We agree that during the development of a forest the land atmosphere interactions will vary over time. To fully understand the impact of a specific land use change, this gradual development should be considered as well as other effects, such as ecological succession, which are also not included in this study as this is beyond the scope of this manuscript. The aim of our manuscript is to get a better understanding of where forest may have positive hydrological effects (i.e., an increase in rainfall potential), and where it may have negative hydrological effects (i.e., a decrease in rainfall potential and expected drying due to enhanced evapotranspiration). We believe that the results

of this study show, once a mature forest has grown, the regions in the Mediterranean Basin that may contribute to more rain locally. We will highlight the implication of our results better in the manuscript. We do want to note that for the regions where a mature forest may contribute to more rain, a more detailed study is necessary. Such an analysis could include the gradual development of a forest.
In summary, we agree with the reviewer that the term “forestation” may contribute to the expectation that this study models the maturing of a forest. To address this issue we will first, more clearly state the aim. Second, to prevent the misuse of the term forestation we will introduce the term “forestation” in the introduction when we provide context of the study, and include it in the discussion to discuss the implications of our results. Throughout the manuscript we will use more accurate phrases such as, L199 as pointed out by the reviewer, and the term “mature forest”. Finally, we will reconsider the use of “forestation” in the title of this manuscript.

Science clarifications

> **Sampling:** 10 years seems like an insufficient length of time to establish a climatology for the region (L83). The description of the sampling method in 2.4 is extremely unclear to me. What does it mean to run the model 20 times for each grid cell with two random days being sampled for each year? Is the study only simulating the atmospheric conditions during 20 random days over the 10-year time period? This, in addition to the high number of samples that had to be filtered out, is very concerning. Please also specify that CLASS is a single column/grid cell model.

We thank the reviewer for pointing out that the sampling method is not described clearly in our manuscript. We will make sure to clarify the sampling method in the manuscript. To answer the question from the reviewer about the number of runs per 10-year period, for each grid cell we run the model 20 times. This results in a total of 57360 samples.

In this manuscript we identify spatial patterns in land-atmosphere interactions for the Mediterranean Basin. As we were mainly interested in spatial patters, we decided to have a full spatial coverage of the study region. To get statistically significant results we decided to select 20 samples for each location. We agree with the reviewer that filtering out a large number of samples raises uncertainty, specifically in some parts in the south of the study region where soil moisture is low. However, more samples do not improve the statistical significance of our analysis as for the relatively dry regions, the same percentage of samples to pass/not pass the filtering step. This would result in a similar uncertainty. We would also like to stress that the results of this study do not provide an actual prediction of what may happen to convective rainfall over a mature forest, the results give an indication of the impact of soil moisture and land cover on convective precipitation. We will discuss the implications of filtering out a large amount of the samples in more detail both in the methods, and discussion sections.

Furthermore, a longer climatology has both advantages and disadvantages. Within a 30-year period, the warming trend would be more pronounced, which may offset the results. We will clarify our motivation and implications for a 10-year study period in the manuscript.

Finally, in the methodology we will specify that CLASS is a single column model. We thank the reviewer for pointing this out.

> **CAPE Analysis:** Why was a threshold of 400 J/kg chosen for the CAPE analysis? Given that the authors calculate CAPE using the metpy cape_cin (typo in L186) function, CIN should also be included in the analysis and would provide a more robust standard for

> determining the likelihood of convective initiation. Recent studies (Emanuel, 2023; Zhang et al., 2023) have also shown that the development of high CIN over wet soils is essential to explaining the development of high CAPE in both models and in observations. Given that the relationship between CAPE and soil moisture is one of the study's main results, the discussion in L364-373 could be expanded and some of that literature should be mentioned earlier in the introduction (L64).

We thank the reviewer for their helpful suggestion. If we are invited to submit a revised version of our manuscript we will include CIN in our analysis. We will discuss in more detail how CIN will be included in the rebuttal letter.

> **Spatial Correlation with Soil Moisture Regimes:** Given that the interpretation relies heavily on an understanding of the different wet and dry locations in the basin, Figure A1 should be included in the main paper to be more easily accessible. The spatial variations in Figure 2 for CAPE are quite scattered compared to the more coherent patterns in BLH and LCL. Could the authors perform some sort of correlation between the ABL/CAPE changes and the soil moisture variations across the region? That is, how well do the ABL/CAPE changes map onto the soil moisture regimes? Also, given that the study is currently framed in the context of future climate change, please comment on how we expect those regimes to change in the future. If moisture decreases in the region, will these results still hold? Please also discuss how realistic an average ΔLCL of over 8km is and rescale the plot in Figure 2.

We thank the reviewer for this suggestion. We will include Figure A1 in the main text of this manuscript. Additionally, we will study correlations between soil moisture and differences in ABL and CAPE for the two different land cover types. This will provide insight into how the variations in ABL and CAPE due to different land cover types overlap with the different soil moisture regimes. Finally, we will discuss in more detail how the future climate may affect the different outcomes for bare soil and forest.

> **P-E and Moisture Recycling:** Given that the study's goal is to understand "future drying" and the changes in precipitation potential are accompanied by changes in evapotranspiration, there should be some considerations of P-E in the discussion of "wet gets wetter" (Abstract).

This would be an interesting addition and we thank the reviewer for thinking along. However, as this method does not allow to quantify the change in precipitation, it is not possible to study how the different land cover types affect P-E across the region. We agree with the reviewer that this information would be highly valuable to better understand whether a forest would contribute to drying or wetting. To account for this issue, we will more carefully phrase the subsentence "wet gets wetter". We will take a close look at the manuscript to see if we should make similar changes in other parts of the manuscript as well.

In addition, we would like to clarify the goal of this study. The goal is not to study future drying specifically. The goal is to study the impact of land cover on the convective rainfall potential to asses where in the Mediterranean Basin a mature forest may contribute to more rainfall locally.

Finally, we would like to thank the reviewer once more for their valuable feedback.

References:

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Ruiz-Peinado, R., Bravo-Oviedo, A., López-Senespleda, E., Bravo, F., & Del Río, M. (2017). Forest management and carbon sequestration in the Mediterranean region: A review. *Forest Systems*, *26*(2), eR04S. https://doi.org/10.5424/fs/2017262-11205