Author’s response to reviewer comment from reviewer #2 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.

> The study examines the influence of forest cover on convective precipitation potential in the Mediterranean Basin, using the CLASS model to simulate the atmospheric boundary layer (ABL) response to changes in land cover (bare soil vs. forest) and soil moisture. While the paper is well-organized and offers valuable insights into land-atmosphere interactions, there are some major reservations I have regarding the methodology, underlying assumptions, interpretation of results, and the clarity of key definitions that should be addressed and revised in the manuscript before publication.

We are happy to hear that the reviewer believes our manuscript offers valuable insights into land-atmosphere interactions and that they find our manuscript well structured. We are thankful the reviewer took their time to provide us with valuable feedback that will help us to improve the manuscript.

1. Definition of key terms

- The terms "forestation," "regreening," and "land restoration" are used throughout the manuscript but lack precise definitions. Please clarify what these terms mean in the context of this paper.

We thank the reviewer for pointing this out, we will carefully reconsider the use of those terms. The terms that we will use will be clarified in the introduction of the manuscript. Finally, we will be more consistent our terminology. Reviewer #1 gave similar feedback, indicating the importance to properly address this point.

- **L285:** The term “parcels” is introduced for the first time in the Discussion section without prior definition, though it is an important concept in the calculation of CAPE. To ensure clarity, the authors should define this term earlier in the manuscript, preferably in the Methods section, and briefly explain its relevance to the study's atmospheric processes.

The reviewer raises a very good point. In the methods section we will describe in more detail how CAPE is calculated, which offers the opportunity to introduce the term “parcels”.

- **L55 and L186:** Given the study’s focus on CAPE and since CAPE is a calculated quantity rather than a height like the ABL or LCL, the authors should explicitly provide the equation used for its computation in the main text. Simply referencing the MetPy Python function is insufficient, as it does not clarify the exact formulation or assumptions applied in this study. Including the full equation and associated assumptions will improve transparency and reproducibility.

We thank the reviewer for this suggestion. We will include the calculation, full equations and their assumptions of CAPE in more detail in the methods section of our manuscript. This also allows us to properly introduce the term “parcels” in the methods section, as raised by the previous comment.

2. Study setup

- **L57: "When both the ABL and LCL cross and the CAPE is at least 400 Jkg-1 there is convective rainfall potential."** The phrasing "both the ABL and LCL cross" needs clarification: does this mean the ABL height exceeds the LCL? Additionally, while CAPE represents atmospheric instability, a threshold of 400 J/kg does not inherently indicate precipitation without considering other key factors such as CIN, mid-tropospheric moisture, and large-scale forcing (Emanuel, 2023). Can the authors clarify the reasoning behind this threshold and account for additional necessary conditions for convective rainfall?

We thank the reviewer for pointing out how we could improve our study set-up. We will more clearly explain the definition of a crossing between the LCL and ABL in our manuscript. To clarify the definition in this discussion, the ABL and LCL cross when ABL ≥ LCL. Hence, there is also a crossing when the LCL and ABL have the same height.
Based on boundary layer dynamic theory and model studies, we came to the conclusion that the parameterization of ABL>=LCL and CAPE>400 J/kg gives a good representative of potential convective precipitation. We will clarify this better in the method section. However, taking into account the comments of both reviewers we believe it is important to add convective inhibition (CIN) in our analysis as well. CIN is the amount of energy (in J/kg) that prevents an air parcel from rising freely through the atmosphere to reach the level where convection starts. CIN halts convection unless overcome by sufficient forcing. In addition to studying the relation between land use type and CAPE, we will also study the relation between land use type and CIN.

Finally, we will include a paragraph in our discussion that describes what other factors (e.g., mid-tropospheric moisture, and more) affect convective precipitation to put the results of our study better into perspective. We would like to note that our set-up of the CLASS model includes an advection term. Therefore, the large-scale forcing is accounted for in our model. We will describe this more clearly in our manuscript.

- **L47: "To isolate the local effects of changes in land cover on local precipitation, a different model approach is necessary."** Please clarify what is meant by "a different model approach." Specifically, elaborate on the key differences between this approach and existing methods, and justify why the proposed model is better suited for isolating local effects on precipitation.

What we refer to with "a different model approach" is using a model that does only simulate the local processes that are affected by land use changes. In climate models that use land cover scenarios, a change in land cover upwind from location X may affect precipitation, or other variables, in location X. This makes it challenging to isolate how a land cover change in location X itself may affect the local processes. We will explain this more clearly in line 47 and use this to motivate our model selection. We thank the reviewer for pointing how we can improve the clarity of our manuscript.

- **L83: "This simulation is done for early summer as this is the start of the dry season."** It is not clear to me why the early summer (May and June) time period is significant for study when it is stated in L93 that there is late spring and summer convective precipitation in the region. Have the authors considered expanding the period of study (which may also improve the study sampling rate)? Please elaborate on this.

During spring/early summer the coupling between the land surface and the atmosphere is stronger compared to other seasons (Ardilouze et al., 2022; Benetó & Khodayar, 2023; Gates & Liess 2001; Lombardo & Bitting 2024). This implies that vegetation relates most to convective precipitation during this time of year. We will add this motivation in the method section. We will also support this statement with the following literature:

"Local evaporation seems to be the most important moisture source during the dry season (10 days integrated contribution is 310.70 10 12 mm yr 1 km 2)." (Gómez-Hernández et al., 2013, p. 6787)

"When comparing Tables 3a and 3b, it is seen that the role of local evaporative processes is more relevant during the dry season." (Gómez-Hernández et al., 2013, p. 6789)

"As mentioned in section 1, large interior extensions present arid or semiarid characteristics and attain the maximum of precipitation in spring, making the recycling contribution essential to describe the precipitation regime locally. On the contrary, the lack of a clear link between recycling and precipitation from October to February confirms that land-atmosphere mechanisms do not play a relevant role on rainfall in the winter half of the year." (Rios-Entenza et al., 2014, p. 5905)

"In Iberia, both conditions (that is, a synoptic configuration favoring convection and sufficient soil moisture availability) reach their maximum coupling in spring and early summer and can be effectively assessed in early spring (March)." (Rios-Entenza et al., 2014, p. 5909)

- **L93: "Although precipitation falls predominantly in winter, during late spring and summer there is convective precipitation in the region."** Given that the study relies on historical data from ERA5, it would be beneficial to include a figure illustrating which grid cells recorded observed precipitation and which are currently bare soil vs. forest. This would help assess the spatial distribution of convective precipitation and clarify how well the modeled convective rainfall potential corresponds to observed precipitation events given the current state of the region. This would be relevant to strengthen the study's conclusions and recommendation on the need for forestation in different areas to "potentially enhance local rainfall through forestation". Can the authors provide such a figure to support this statement?

We thank the reviewer for this suggestion, we believe that such a figure to be a valuable addition to our manuscript and we will include it in the appendix.

- **Section 2.6 Postprocessing.** I am concerned about the disproportionate exclusion of dry regions compared to wet regions due to the filtering process. The bias introduced by the increased sampling size in wetter regions undermines the generalizability of the authors' conclusion that both forestation and an increase in soil moisture can contribute to convective rainfall potential. Given that nearly half of the samples—primarily from dry regions—were excluded, can the authors clarify how this bias affects their findings? Additionally, how do the authors justify applying these conclusions across the Mediterranean Basin area of study when dry regions are underrepresented in the results? (particularly for Fig. 3). Can the authors comment on this?

We agree with the reviewer that our conclusion should be formulated more carefully and that we should explain the implications of the filtering process more clearly. We will discuss the resulting bias in more detail in the discussion section of our manuscript. We will more carefully phrase the conclusion and discuss their uncertainty for the drier regions in the Mediterranean Basin taking the bias into account.

To summarize the bias here shortly: The results have a higher uncertainty in the south of the Mediterranean Basin due to the smaller amount of samples that pass the filter. However, within these relatively dry regions there are quite some grid cells that have 10 (50%) or more samples passing the filtering step (Figure A2). Still, there are some regions where our results have a relatively high uncertainty, i.e., in Libya, Lebanon, and Syria. The results in these regions should be interpreted very carefully. We will carefully review the manuscript to determine whether it is necessary to include statements to inform the reader that a careful interpretation is necessary. In addition, we will discuss the uncertainty of our results and its variability across the study region in more detail in the manuscript.

- **Section 2.7 Validation.** The authors mention some numbers to classify "short and tall vegetation cover", but it is not clear to me how these values are retrieved or calculated. Please clarify the source and methodology used to define these classifications.

We thank the reviewer for pointing out this is not clear. These data are obtained from the ERA5 dataset. In the ERA5 dataset tall vegetation is considered as: evergreen trees, deciduous trees, mixed forest/woodland, and interrupted forest; Short vegetation is considered as: crops and mixed farming, irrigated crops, short grass, tall grass, tundra, semidesert, bogs and marshes, evergreen shrubs, deciduous shrubs, and water and land mixtures. Not all these vegetation types are found in the study region. We will include these details in section 2.7 of our manuscript.

- **Section 2.8 Model output interpretation**, **L195: "To analyze the uncertainty of the convective rainfall potential we also study the convective rainfall potential for a change in BLH, LCL and CAPE of ±10%."** The authors state that they analyze the uncertainty of convective rainfall potential by varying BLH, LCL, and CAPE by ±10%. However, it is unclear why these 10% variations are chosen and if there is any statistical significance towards the conclusion that the "inaccuracy of the exact values may be of less importance" in L344.

These 10% variations are chosen to get more insight into the robustness of the spatial patterns we identified. We were interested in the sensitivity of the model output to small variations in BLH, LCL, and CAPE. However, we were not interested in whether this sensitivity is linear or non-linear. Therefore, we decided to study the implications of 10% variations. The decision to study 10% variations is somewhat arbitrary, yet it suffices to analyze the relative sensitivity of the model output to the three variables. The statement in line 344 is based on the uncertainty of the ERA5 dataset. The exact values in ERA5 are subject to uncertainty as mentioned in the official documentation of ERA5 (https://confluence.ecmwf.int/display/CKB/ERA5%3A+data+documentation). However, the spatial resolution in the ERA5 data has a smaller uncertainty. The uncertainty in the ERA5 data was the main motivation to study the uncertainty in the spatial patterns. We selected variations of 10% to better understand if a change in either BLH, LCL, or CAPE, due to uncertainty in the exact values of ERA5 would have effects on the spatial patterns that were obtained in our study and whether these patterns are robust despite small variations in BLH, LCL and CAPE.

  - It also seems to me that this uncertainty is related to the "inaccuracy of the exact values" and assesses the sensitivity of results to minor perturbations in key variables. How do the authors account for the impact of sampling bias and dataset exclusions (particularly in dry regions) on the robustness of their conclusions (particularly for Fig. 3 and 4 and associated discussion)?

We thank the reviewer for pointing this out. We believe that we can improve the discussion on the impact of the filtering step on the results in Fig. 3 and Fig. 4. We will describe the impact of the filtering step in more detail in the discussion. In this discussion we will refer to how the results presented in Figs. 2-4 may be affected by this filtering step.

- **For all figures referencing the rainfall potential color scale.** Please include in the caption how rainfall potential is defined. Specifically, what constitutes a grid cell to "have a convective rainfall potential" or "have no convective rainfall potential" in the sample?

We thank the reviewer for pointing this out. We will include the definition of rainfall potential in the captions of the figure and describe the difference between "a convective rainfall potential" and "no convective rainfall potential".

3. General manuscript proofreading

- Throughout the manuscript, there are multiple instances of missing commas and periods, which affect general readability and clarity. I recommend a thorough grammatical review to improve sentence structure, punctuation, and overall flow. In particular, some sentences lack necessary commas for readability, and certain sections contain run-on sentences that would benefit from clearer punctuation. See L27, L47, L48 for some (not all) examples. Also, see L186: "cape_sin" should be corrected to the correct function name "cape_cin", and L394: "mediterranean" should be capitalized. A careful proofreading by the authors would enhance the manuscript's clarity.

We thank the reviewer for pointing out that the readability and clarity of the manuscript could be improved overall. We will make sure to conduct a final review by multiple authors in which we pay close attention to the grammar. We thank the reviewer for already pointing out some of the lines which we should revise.

Finally, we would like to thank the reviewer for taking the time to provide valuable feedback on our submitted manuscript.

References:

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