1 RC1: 'Comment on egusphere-2025-5329', Anonymous Referee #1, 23 Jan 2026

This study presents a systematic framework for tuning and modifying the cold-pool parameterization in LMDZ using the High-Tune-Explorer (HTE), with large-eddy simulations (LES) serving as the physical reference. Motivated by insights from LES, several key parameter assumptions are revised, and the use of HTE to optimize free parameters leads to a substantial reduction of biases in single-column model (SCM) simulations.

A major strength of the manuscript lies in its clear and well-structured description of the LMDZ convective parameterization associated with cold-pool processes. The results demonstrate clear benefits for representing cold-pool effects and are potentially valuable for future LMDZ CMIP7 simulations. Overall, this is a solid and well-executed study.

I recommend publication after the following points are addressed or discussed.

1.LES cases used for tuning

The LES cases employed in this study include radiative–convective equilibrium (RCE) simulations and a diurnal cycle over land (AMMA) case. While these configurations are appropriate for examining cold-pool impacts on convection initiation and diurnal timing, cold-pool dynamics are also closely linked to the organization of mesoscale convective systems through mechanisms such as those described by RKW theory. It would be helpful for the authors to discuss whether incorporating LES cases of squall lines or other organized mesoscale convective systems could further constrain or refine the parameterization, or whether such regimes lie beyond the intended scope or intrinsic limits of the current cold-pool formulation.

2.Remaining bias in diurnal cycle timing

The simulated diurnal cycle remains delayed by approximately three hours. Previous studies (e.g., Khairdinov and Randall 2009) have suggested that cold-pool processes may play an important role in regulating convective timing. It would be useful to clarify whether this timing bias could potentially be reduced through further tuning of the cold-pool parameters, or whether it reflects a more fundamental limitation of the parameterization in capturing timing as opposed to intensity or spatial structure.

3.From SCM tuning to coupled simulations

The SCM experiments provide a clean and controlled framework for evaluating the impact of the revised parameterization. However, it is well known that parameterizations optimized in SCM settings may degrade performance once fully coupled to a three-dimensional model. Before proceeding to fully coupled CMIP-style simulations, it would be valuable to consider an intermediate hindcast-type evaluation (e.g., following Ma et al. 2015). The authors are encouraged to comment on whether such an approach is planned or feasible within the scope of this work.

4.Effectiveness and physical interpretation of the tuning framework

The manuscript would benefit from a more explicit discussion of the effectiveness of the HTE tuning framework itself. In particular, further elaboration on how the adjusted parameters relate to the underlying physics of cold-pool processes—such as their fundamental structure, triggering mechanisms, and interaction with convection—would strengthen the physical interpretation of the tuning results and improve the broader applicability of the methodology My suggestion would be replacing Figure 1 with more coherent parameters of cold pools).

Citation: https://doi.org/10.5194/egusphere-2025-5329-RC1

2 RC2: ’Comment on egusphere-2025-5329’, Anonymous Referee #2, 26 Jan 2026

2.1 General comment:

This paper addresses an important problem of validating cold-pool parameterizations in coarse-resolution models, on the example of LMDZ model. The study is very interesting and presents an insightful LES-based analysis of downdrafts and cold-pool effects. I recommend the paper for publication after the authors address a number of technical issues, primarily aimed at improving the clarity of the manuscript, through a major revision.

2.2 Major points:

Please add a schematic explaining how your cold-pool parameterization works and what it changes in your convection parameterization. I suggest using either one LES model for both numerical experiments, or the same pair of models for both experiments, for consistency. Please show a comparison of the results without the cold-pool parameterization to demonstrate its impact on the solution. Please comment on the changes in the behavior of convection due to cold pools.

2.3 Specific comments:

Title: Specify which model this is for to avoid confusion.

Abstract: Briefly explain which cases are used for validation (quasi-equilibrium and time-dependent).

Equations: Could you please double-check all equations for correctness? They may be fine, but it would be useful to verify them once more. For instance, Eq. 1 seems to be missing the theta/T term, which may be unimportant for shallow convection but becomes important for deep convection where cold pools are present. Also, please unify the notation: you use symbols in some instances and letters in others, such as “div”.

Equations are part of the text and should include proper punctuation as well.

L5: This reads unfortunately and suggests that the model developers have not accurately evaluated their parameterization before implementing it. Please rephrase.

L20: There are papers in which cold pools are not referred to as cold density currents when they form wind gustiness. For a cold pool to expand, a non-zero velocity must be present. “Density currents” appears to be a more general term from hydrodynamics. Please rephrase. I believe the authors use the term “cold pools” for both downdrafts and cold pools, which should either be changed or clarified.

Furthermore, the literature review needs to be improved. A number of papers on cold-pool parameterizations for weather and climate models have been published and are worth mentioning, for example Park et al. (2024), Suselj et al. (2019), Rooney et al. (2022), and Freitas et al. (2024).

L49: This statement is unclear. See Suselj et al. (2019) for an example of how LES was used to develop a cold-pool parameterization within a convection scheme. They use a no-cold-pool solution as a reference to which the SCM must converge, and only then extend it to account for cold-pool effects. In the current study, you appear to compare the SCM only to full-physics LES, which should be clarified.

L76: Is it possible to specify which ones?

L93: Are Q1 and Q2 relevant to Yanai et al. (1973; JAS)?

L99: Unclear. Which term is discussed here?

2.4 Subsections 2.2.1–2.2.3:

It appears that the beginnings of paragraphs are missing.

L111: It may be sufficient to state that this is a bulk-plume approach applied to the subcloud layer, if that is the case.

I find this description somewhat confusing. You first describe organized thermal plumes in the boundary layer without specifying whether this refers to a dry or cloudy layer. From the context, it seems to be a shallow convection scheme. Please clarify.

L124: This suggests that your shallow-convection parameterization represents cumulonimbus clouds. Please double-check this description. Is it also a bulk-plume scheme?

L128–140: Please be precise and avoid this type of jargon. It is unclear what is meant by "exchange matrix" and by "compartments" in your scheme. Is this a description of a bulk updraft that is diluted with height using a buoyancy-sorting mechanism?

There is no need to use bold fonts here.

L140: Here you already use "cold pools" and "density currents" interchangeably. I suggest sticking with "cold pools," which is the more common terminology in the literature.

L145: Is this the modification proposed here? Please clarify. Since the cold-pool parameterization was already proposed in GL10, I assume this aspect is not novel to this paper.

Fig. 1: Using a figure from another paper may require copyright permission. I suggest removing it and preparing a new one, explaining both the details of the cold pool parameterization and how it affects the convection parameterization. This is the essence of this paper.

L151: The cold-pool model description is difficult to follow. Why is it designed on an infinite plane, and how does this relate to the model grid-box size? Why do all cold pools have the same radius, and does it change with time or remain fixed? If their centers are statistically distributed with a uniform density Dwk, does this imply a constant distance between adjacent cold pools? Does each cold pool have an associated downdraft, or is this only a conceptual model? What is the relationship between cold-pool area and the environment? When are cold pools initialized? What determines their lifecycle, from initialization to decay? Is it linked to the amount of evaporating precipitation?

I suggest preparing a new Fig. 1 that explains how your cold-pool parameterization works.

L170: Using "probably" casts doubt on the approach. Please rephrase. It is acceptable for a parameterization not to account for all processes.

L173: What is meant by the spread rate of cold pools? Are they initialized at the surface with a fixed radius and then allowed to grow?

L180: "Hope" is not a scientific term. Please rephrase.

L189: What is the role of lateral entrainment here? Does it reduce WAPE?

L195: Why is this important?

L199: Please clarify whether your cold pools are also part of downdrafts. If so, this would be an important distinction from other approaches. For example, Suselj et al. (2019) use separate parameterizations for downdrafts and cold pools.

L226: If cold pools are part of downdrafts, how should the horizontal size and spacing be interpreted at higher altitudes? Do these properties change with height?

L225: This is a risky statement. The model needs additional explanation to link downdrafts with near-surface cold pools.

L263: This appears to be an assumption of the model that is difficult to validate. Please clarify.

Eqs. 20–21: These equations appear unexplained.

Eq. 22: Once you derive an equation that is important for the model, please provide some interpretation. For instance, what are the consequences of this formulation, and which parameters dominate?

Overall, this section would benefit from a schematic explaining how the parameterization works, from initialization through intermediate stages to decay. Please also interpret the major components to help the reader understand the model.

L265: What does this mean? Is this truly power (or energy), and how does it enter the convection parameterization? Does it affect entrainment, organization, or other aspects, or does it help initiate stronger updrafts?

L270: There is no need to explain this.

L273: This statement is risky. You cite only two papers for shallow convection. LES has been used to simulate a wide range of PBL regimes with both finer and coarser resolutions, depending on the problem. Please rephrase or remove.

L275–277: In papers such as Tan et al. (2018) or Suselj et al. (2019), previous LES-based studies are cited systematically. You can simply cite earlier work.

L278–294: Please edit this section carefully, explaining why three cases are needed and why two different LES models are used. I suggest either using two LES models for both cases, for example to address uncertainty, or using a single LES model for all cases. Ensure the sentences are clear and avoid vague phrasing such as "The destabilization leads to convection."

It should be clearly stated that the LES provides full physics, including cold-pool effects, and therefore does not allow validation against a no-cold-pool reference solution. Moreover, the RCE approach simplifies the time dependence of cold pools because the mean state does not evolve. Only the second experiment appears to introduce time dependence.

L294: What surface fluxes are used here, fixed or time-dependent? Table 1 shows that AMMA data were averaged over five hours. Does this correspond to a quasi-steady state? The role of cold pools in the diurnal cycle over land is strongly time-dependent.

L308: Please remove "fairly immediately" and check the manuscript for similar phrasing.

Fig. 9b: Why are the LES results much moister than LMDZ? The differences appear large. Do they also affect cloudiness? If so, please describe this.

Figure 11 shows a significant improvement after tuning, but it would be helpful to understand what changes in the convection lead to this result.

Although the paper focuses on downdrafts and cold pools, it would be useful to briefly discuss the behavior of the convective updrafts as well, since these processes are strongly coupled.

Temporal bias (delay) in triggering the diurnal cycle of convection is typical of convection parameterizations. One potential way to alleviate this bias is by linking cold-pool effects with entrainment. Have the authors explored this approach?

Conclusions: Consider simplifying this section so that the main messages can be understood by a general reader. For example, when you state (L611) that the value of coefficient k was increased from 0.33 to 0.56, this result is difficult to interpret without carefully reading the entire paper. I suggest rephrasing the conclusions so they are accessible even to readers who have not engaged with all the technical details. Think of a general picture here.

References: Freitas, S. R., Grell, G. A., Chovert, A. D., Silva Dias, M. A. F., de Lima Nascimento, E. (2024). A parameterization for cloud organization and propagation by evaporation-driven cold pool edges. Journal of Advances in Modeling Earth Systems, 16, e2023MS003982.

Suselj, K., M. J. Kurowski, and J. Teixeira, 2019: A Unified Eddy-Diffusivity/Mass-Flux Approach for Modeling Atmospheric Convection. J. Atmos. Sci., 76, 2505–2537,

Park, S., Song, C., Kim, S., & Kim, J. (2024). Parameterization of the elevated convection with a unified convection scheme (UNICON) and its impacts on the diurnal cycle of precipitation. Journal of Advances in Modeling Earth Systems, 16, e2023MS003651.

Rooney, G.G., Stirling, A.J., Stratton, R.A. & Whitall, M.(2022) C-POOL: A scheme for modelling convective cold pools in the Met Office Unified Model. Q J R Meteorol Soc, 962–980.

Tan, Z., Kaul, C. M., Pressel, K. G., Cohen, Y., Schneider, T., & Teixeira, J. (2018). An extended eddy-diffusivity mass-flux scheme for unified representation of subgrid-scale turbulence and convection. Journal of Advances in Modeling Earth Systems, 10, 770–800. Citation: https://doi.org/10.5194/egusphere-2025-5329-RC2