

Answer to reviewer #2

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

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.

Thanks a lot for these very positive comments and for all your suggestions. We answered them all and tried to use them to improve the text.

Major points:

Please add a schematic explaining how your cold-pool parameterization works and what it changes in your convection parameterization.

As suggested in the reviewer's major points as well as by reviewer #1, we proposed a new schematic view of the parameterization and of its coupling with convective parameterizations (see new Fig. 1).

I suggest using either one LES model for both numerical experiments, or the same pair of models for both experiments, for consistency.

Yes, it would have been interesting to have a second LES for the AMMA case in order to perform the analyses with two LES for each case. However, for the AMMA case, we have only the Meso-NH LES. We however consider as essential to retain two LES for the RCE case, since this is an idealized case known to be highly sensitive to the microphysics scheme. We add sentences in the revised version to motivate this choice (L334-337):

▷ In the present study, we use the outputs of LES for two test cases, one over ocean and the other one over land. The test case over ocean was run with two different models, SAM and Meso-NH, with the exactly same setup in order to assess the uncertainty in LES, as it is known that RCE experiments are particularly sensitive to the microphysics scheme (Wing et al., 2018).

Please show a comparison of the results without the cold-pool parameterization to demonstrate its impact on the solution.

We consider that this question is a bit out of the scope of the present paper. Indeed, the introduction of cold pools in LMDZ was accompanied by a change in the closure condition of the convective scheme. Thus, the closure is not the same depending on whether cold pools are taken into account or not. How the introduction of cold pools together with thermal plumes and a change in closure is documented both by Rio and Hourdin (2008) in 1D and Hourdin et al. (2020) in the LMDZ 3D GCM.

Please comment on the changes in the behavior of convection due to cold pools.

In LMDZ, the effect of cold pools on convective behavior has already been well documented by Rio et al. (2009). Their results show that cold pools significantly improve the representation of the diurnal cycle of continental precipitation in LMDZ. This study is already cited in the manuscript. The relevant passage is as follows (L62-65):

▷ Indeed, the coupling of the Emanuel (1991) parameterization of deep convection with cold pool parameterization of Grandpeix and Lafore (2010) and with the thermal plume model of Rio and Hourdin (2008) in the LMDZ climate model significantly improved the simulation of the diurnal cycle of precipitation over land in the tropics (Rio et al., 2009), shifting its maximum from noon to mid afternoon.

Specific comments:

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

The title was changed according to the referee's suggestion:

▷ New title : Evaluation and improvement of the cold pool parameterization in the LMDZ climate model against Large Eddy Simulations

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

We added the information in the abstract :

▷ The evaluation is done both on a case of radiative–convective equilibrium that allows the study of convective processes in a well-controlled and quasi-stationary framework, without the influence of large-scale dynamics, and on the time-dependent continental case AMMA (African Monsoon Multidisciplinary Analysis), representative of typical Sahelian conditions with local initiation during the afternoon.

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 θ/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 .

Thanks a lot for pointing to this missing θ/T term in the equations of the paper. In fact, those terms were already missing in the original paper by Grandpeix and Lafore (2010). Hopefully, the error is not present in the parameterization itself, which is written in terms of static energy rather than potential temperature. This term has been corrected and all the other equations were double checked, very carefully. We did not find other mistakes in the equations.

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

Done

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

We agree that this sentence was somehow misleading. We've clarified things in the new version of the abstract :

▷ The introduction of a cold pool parameterization into the LMDZ climate model has significantly improved the representation of convection, in particular its diurnal cycle. Thanks to this indirect evaluation, the parameterization of cold pool was retained in the further versions of LMDZ. However, no detail evaluation of the representations of cold pools themselves was done so far. This work provides for the first time such an evaluation based on Large Eddy Simulation (LES).

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.

We agree that the wording was not well enough defined in the original manuscript. We tried to clarify as much

as possible those terms in the new manuscript. In particular, we make, and our model makes a clear distinction between the unsaturated downdrafts (which are part of the convective parameterization in the model) and cold pools. As for the later, we decided to conserve the wording "cold pools" all along the text. Cold pools are however a specific case of density currents created by the cooling provided by the unsaturated downdrafts. The modified text (beginning of Section 1, L19-25) is reproduced below :

▷ During thunderstorms, precipitation forms inside convective clouds. When it falls either to the side or below the cloud, into air that is not saturated with respect to water vapor, a portion of this precipitation evaporates. This evaporation cools the air, making it denser and creating so-called unsaturated downdrafts. When approaching the surface the descending air masses diverge horizontally. The cold air may then accumulate in pools that expand. These expanding masses of cold air are generally called "cold pools" or "wakes". The later term was retained when developing the code and will be used here in the equations for consistency. The former term will be used throughout the text of this paper. The cold pool, that spreads horizontally because its air is colder than its environment, can also be seen as a density current (Charba, 1974; Droegemeier and Wilhelmson, 1987). Cold pools are most often associated with a gust front, capable of lifting the surrounding warm air and thus promoting the development of new convective cells.

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).

We agree. We added the following to the text in the introduction (L38-40 and L46-61):

▷ Cold pools also play a role in triggering elevated convection (i.e., convection initiated above the boundary layer). Accounting for cold pools in a GCM has been shown for instance to improve the representation of nocturnal precipitation over the Southern Great Plains in central US (Park et al., 2024).

and ▷ Other explicit parameterizations of cold pools were proposed more recently by Park (2014) and also by Rooney et al. (2022). Implicit cold pool parameterizations have also been developed in order to represent the memory effect of cold pools onto convection. Piriou et al. (2007) and Suselj et al. (2019) represent this effect by formulating entrainment as a function of past precipitation evaporation. The parametrization of Del Genio et al. (2015) is somewhat intermediate between explicit and implicit parameterizations: cold pools are used as stores of cold air coming from precipitating downdrafts, thus providing a memory to deep convection which increases the phase lag relative to surface forcing in a manner very close to Piriou et al. (2007) and Suselj et al. (2019). More recently Freitas et al. (2024) designed a parameterization which focuses on the impact of gust fronts without considering the cold pools themselves. The parameterization was tested with the BRAMS model and was shown to improve the MCS representation. Among the cold pool parameterizations mentioned above, only those of Grandpeix and Lafore (2010) and Rooney et al. (2022) represent cold pool spreading as resulting from a temperature difference between the cold pool and its environment, that is, as a density current. In the other parameterizations, cold pool expansion is solely driven by the descending mass fluxes that feed the cold pool. The works of Rooney et al. (2022) and Freitas et al. (2024) introduced propagation from one model grid cell to another, a process which is not represented so far in LMDZ although it is known to be important for representing their impact on convective organization (Freitas et al., 2024). Several of the aforementioned studies show an improvement of the representation of the diurnal cycle of precipitation when parameterizing cold pools (Park, 2014; Rooney et al., 2022).

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.

We agree that the manuscript on this point was deserving to be clarified and nuanced. To our knowledge, LES have not yet been used to directly evaluate cold-pool parameterizations. They have been used to guide the development of such parameterizations, as we mentioned at line L48 of the submitted manuscript, with the reference to Kurowski et al. (2018). From what we understand from Suselj et al. (2019), the authors use LES to evaluate in detail their unified convection parameterization (shallow, moderate, and deep). They also use LES to study the effect of cold pools on convection, but not to directly evaluate the representation of the cold pools themselves in their model. We added a paragraph on this point in the revised manuscript (L77-83):

▷ Suselj et al. (2019) used LES to evaluate in detail the internal variables of their unified convection (dry,

shallow and deep) parameterization, for example by validating the surface fraction covered by moist updrafts. They also used LES to validate an approximation regarding the timing of when cold pools begin to invigorate convection in their unified convection scheme, and to study the effect of cold pools in the convection. Other studies have used LES to better understand cold pool processes (Feng et al., 2015; Meyer and Haerter, 2020; Lochbihler et al., 2021) and to guide the development of cold pool parameterizations (Kurowski et al., 2018). However, to our knowledge, no study has yet exploited LES to evaluate in detail the internal variables of a cold pool parameterization.

L76: Is it possible to specify which ones?

Not sure we got the question properly. Do you mean specifying "which other exercises". We rather simplified the sentence from (submitted manuscript):

▷ The latter is one of around twenty coupled models taking part in major international model intercomparison exercises, such as those of the CMIP (Coupled Model Intercomparison Project), the results of which are used in IPCC (Intergovernmental Panel on Climate Change) reports.

to (revised manuscript at L108-110):

▷ The latter is one of around twenty coupled models taking part regularly in the international coupled model intercomparison project (CMIP), the results of which are used in IPCC (Intergovernmental Panel on Climate Change) reports.

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

Yes. The reference was added to the text at the first mention of Q1 and Q2 (L127-128):

▷ The role of convective parameterizations is to provide sources of heating Q_1 and moistening Q_2 (following notations first introduced by Yanai et al., 1973) [...]

L99: Unclear. Which term is discussed here?

The wording was not clear indeed. We write in the new version (L127-134):

▷ The role of convective parameterizations is to provide sources of heating Q_1 and moistening Q_2 (following notations first introduced by Yanai et al., 1973) to the conservation equations of potential temperature θ and specific humidity q_v :

$$C_p \frac{D\theta}{Dt} = \frac{\theta}{T} [Q_R + (L_v + f_g L_f)(c - e)] - C_p \frac{1}{\rho} \frac{\partial \overline{\rho w' \theta'}}{\partial z} = \frac{\theta}{T} [Q_R + Q_1], \quad (1)$$

$$\frac{Dq_v}{Dt} = e - c - \frac{1}{\rho} \frac{\partial \overline{\rho w' q'_v}}{\partial z} = -Q_2/L_v, \quad (2)$$

where C_p is the heat capacity of dry air, Q_R is the radiative heating, c and e are condensation and evaporation rates, f_g is the condensate ice fraction, L_v is the latent heat of vaporization, L_f the latent heat of fusion and $-\partial_z \overline{\rho w' \phi'}/\rho$ (with $\phi = \theta$ or q_v) is the vertical convergence of the Reynold turbulent flux of ϕ accounting for the effect of the subgrid-scale turbulent or convective motions on the explicitly resolved large scale flow.

Subsections 2.2.1-2.2.3: It appears that the beginnings of paragraphs are missing.

This was a LaTeX issue: these are not sections but paragraphs. Corrected in the revised manuscript.

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

We agree that the text was not clear enough. It is indeed a bulk-plume model but we prefer to use the idea of

an effective plume. The new text is reproduced below (section 2.2.2) :

▷ Shallow convection (dry or cloudy) is handled in LMDZ with the so-called “thermal plume model”, a mass flux scheme which was developed to account for non local vertical transport by organised thermal plumes, cells or rolls in the convective boundary layer (Hourdin et al., 2002; Rio and Hourdin, 2008). The combination of an eddy diffusion with a mass flux scheme for the representation of turbulent transport in the convective boundary layer, first proposed by Chatfield and Brost (1987), has since be popularized as the EDMF (Eddy Diffusion Mass Flux) approach.

In the thermal plume model, more specifically, the population of convective structures within a grid cell are summarized into a unique effective ascending plume, [...]

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.

It is indeed a shallow convection scheme. We hope the text reproduced above, answering the previous question, makes it clear.

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

It may be the beginning of the sentence that makes the text a bit confusing in the submitted manuscript. The shallow convection scheme does not represent cumulonimbus clouds. Yes, the deep convection scheme is also a bulk-plume scheme. We rewrote this paragraph (L166-169):

▷ Deep convection is represented with the Emanuel (1991) mass flux scheme modified by Grandpeix et al. (2004). Its fundamental principles are retained, with a representation of a population of cumulonimbus clouds as a collection of saturated updrafts and downdrafts together with an unsaturated downdraft. This parameterization simultaneously represents transport, condensation, cloud formation, and precipitation.

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?

The idea was to give insight to the code but we agree that this sentence was somewhat confusing. The sentence was removed. The new paragraph in the revised manuscript (second paragraph of the section 2.2.4), following the one reproduced above, is:

▷ The core of the cumulonimbus is represented as an undiluted updraft that does not entrain air laterally above cloud base, but is gradually “eroded” while rising. This updraft is assumed to be fast enough to carry the liquid or solid water condensed within it. Following the Episodic Mixing and Buoyancy Sorting approach, a population of diluted ascending or descending air masses [...]

There is no need to use bold fonts here.

Bold font were removed.

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.

Done

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.

We agree that this paragraph was confusing. The first modification to the scheme, concerning the air mixture

between the updraft and environment is part of the convective scheme and should not be presented as a "modification". In the new version, we start the description of the deep convection scheme by (L166):

▷ Deep convection is represented with the Emanuel (1991) mass flux scheme modified by Grandpeix et al. (2004).

The other modification mentioned in this paragraph was anticipating the section **2.4 Triggering and closure of the deep convection scheme**. This paragraph (just before **2.3 Cold pools**) was thus simply removed.

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.

As suggested in the reviewer's major points as well as by reviewer #1, we proposed a new schematic view of the parameterization and of its coupling with convective parameterizations (see new Fig. 1).

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?

We hope that the detailed and most often relevant reviewer's remarks helped us improved the description of the cold pool model (section 2.3).

To start with, the mention of the grid-box is confusing and useless here. The idea behind infinite plane is made more explicit:

▷ The cold pool model represents a population of circular cold pools (or wakes) over an infinite plane, consistently with the way the Reynolds decomposition is used to separate, in GCMs, the 3D dynamical core from the 1D world of parameterizations of subgrid transport, assuming that the statistics of the turbulent or convective motions responsible for this transport are horizontally homogeneous on an infinite plane.

Why do all cold pools have the same radius, and does it change with time or remain fixed?

It is a simplification of the model. The text has been changed as follows (L195-198):

▷ An important simplification of the model consists in assuming that, within a given column of the model and at a given time step, all the wakes have the same height, radius, and vertical profiles of thermodynamic variables which is equivalent to say that we are representing the population of cold pools through a mean effective cold pool. All those variables however evolve in time according to the cold pool physics, as detailed below.

If their centers are statistically distributed with a uniform density D_{wk} , does this imply a constant distance between adjacent cold pools?

Not necessarily. Text modified as follows (L190-191):

▷ The centers of those cold pools are randomly distributed with a uniform number density D_{wk} (number of cold pools per unit area) assuming no overlap between two of them.

Does each cold pool have an associated downdraft, or is this only a conceptual model?

Difficult to answer this question, linked to the fact that both the convective and cold pools parameterizations are conceptual, or bulk, or effective models. We agree that the text was particularly confusing there. It was reorganized and we say more precisely on this question (L202-204):

▷ The main driving of the parameterization is the cooling associated with convective unsaturated downdrafts. All this cooling, provided by the deep convection scheme, is applied to the interior of the cold pools thus creating a contrast in temperature δT between the interior and exterior of the pool (see Fig 1.).

What is the relationship between cold-pool area and the environment?

The space is divided into two parts: the cold-pool region and its environment. Each has its own profiles of temperature, humidity, and vertical velocity. The definition of δX is grouped now with the introduction of the separation between cold pool and environment to make it clearer (L199-201):

▷ In practice, the model divides the space into two parts, the interior (wk) and exterior (or environment) of cold pools (ex) respectively. For temperature, humidity and vertical velocity, we introduce a wake anomaly $\delta X = X_{\text{wk}} - X_{\text{ex}}$ defined as the difference of its mean values in the two subdomains and \bar{X} the average over the horizontal domain.

When are cold pools initialized? What determines their life cycle, from initialization to decay? Is it linked to the amount of evaporating precipitation?

Cold pools are initialized as soon as convection is triggered and disappear when the evaporation of precipitation becomes weak, when WAPE falls below a given threshold. This shows that their life cycle is indeed linked to the amount of precipitation evaporation. We finally decided to add a full paragraph describing this life cycle before entering the detail of the parameterization and questions (L202-211)

▷ The main driver of the parameterization is the cooling associated with convective unsaturated downdrafts. All this cooling, provided by the deep convection scheme, is applied to the interior of the cold pools thus creating a contrast in temperature δT between the interior and exterior of the cold pool (see Fig. 1) Once the cold pools are initiated (which requires a prior activation of deep convection), they enter a positive feedback loop with deep convection. Cold pools reinforce deep convection both because the undiluted ascent, at the core of the parameterization of deep convection, is fed by air coming from the environment of the cold pool (warmer near the surface than the average grid cell), and because of a lifting energy provided by the cold pool spreading as detailed in the following section. The temperature difference between the cold pools and their environment is controlled by competing effects: it increases due to cooling by convective downdrafts and decreases due to cold pools spreading and gravity wave damping. When deep convection stops, this temperature contrasts decreases until switching off the cold pool parameterization based on a combination of thresholds.

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

Done

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

Yes. The sentence was removed.

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? Yes. We have revised the description of this aspect as well (L225:231):

▷ When cold pools appear in a grid cell where they were not present before, their fractional cover $\sigma_{\text{wk}} = \pi r^2 D_{\text{wk}}$ (where r is the cold pool radius) is set arbitrarily to 2%, corresponding to an initial radius of about 2.5 km over ocean and 30 km over land with the values chosen for the wake number density in LMDZ6A. The cold pool radius then grows with rate $\dot{r} = C_*$ so that the time evolution of the fractional surface reads:

$$\partial_t \sigma_{\text{wk}} = 2\pi r C_* D_{\text{wk}} = 2C_* \sqrt{\pi D_{\text{wk}} \sigma_{\text{wk}}}, \quad (3)$$

This surface fraction increases over time. It is limited to 40% of the domain. When this threshold is reached the growth of the cold pools is stopped and the radius remains constant.

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

We agree and rather removed the sentence which was not adding to the text

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

Yes. Lateral entrainment warms the cold pool and thus reduces the WAPE. The information was added as follows (239-242):

▷ The vertical subsidence which thus increases downward between p_{upper} and p_{wk} is fed by lateral entrainment e_{wk} without detrainment so that $e_{\text{wk}} = \sigma_{\text{wk}}(1 - \sigma_{\text{wk}})\partial_p\delta\omega + \partial_t\sigma_{\text{wk}}$. This lateral entrainment accounts for the horizontal component of the meso-scale circulation known to entrain dry and warm (in terms of potential temperature) air from low- or mid- tropospheric air into the cold pool (see Fig. 1), in turn reducing the WAPE.

L195: Why is this important?

It is not. The text was made more explicit on this point (L245-248):

▷ In the version used here, $\delta\omega^{cv} = 0$ above this level as we realized that the GL10 version was introducing a double counting of the vertical mass flux of unsaturated downdraft at p_{upper} . This modification has however little effect on the parameterization results since the mass flux associated with downdrafts at p_{upper} is small (result not shown).

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.

No, the cold pools are not part of downdrafts. We hope that Fig. 1 and all the modifications of the text will help remove ambiguities on that point. In the GL10 model, cold pools and downdrafts are represented as two distinct processes. The cooling by unsaturated downdraft is computed by the convection scheme. This cooling is assumed to take place within the fraction of the grid cell covered by cold pools. This cooling is driving the subsidence and horizontal spread of cold pools. In order to help clarify this point, we reorganized a little bit this section by moving the description of the cold pool tendencies (Q_1^{wk} and Q_2^{wk}) which was containing L199 to the end of the section. We also tried to insist on the fact that downdraft are part of the convection scheme and not of the cold pool scheme (L239-242 and L249-251):

▷ The vertical subsidence which thus increases downward between p_{upper} and p_{wk} is fed by lateral entrainment e_{wk} without detrainment so that $e_{\text{wk}} = \sigma_{\text{wk}}(1 - \sigma_{\text{wk}})\partial_p\delta\omega + \partial_t\sigma_{\text{wk}}$. This lateral entrainment accounts for the horizontal component of the meso-scale circulation known to entrain dry and warm (in terms of potential temperature) air from low- or mid- tropospheric air into the cold pool (see Fig. ??), in turn reducing the WAPE. and ▷ As already said, it is the cooling associated with the re-evaporation of rain in unsaturated downdrafts that is the primary driver of cold pool development. This process is reflected in the model by assigning the heating term Q_1^{unsat} computed by the convective scheme to the interior of cold pools, while Q_1^{sat} acts on their environment.

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

The risky statement "The cold pool model is now fully described. It includes:" :) was replaced by a less risky one (L278):

▷ Finally, the cold pool models includes:

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?

Cold pools are not part of the downdrafts.

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

We added (L323):

▷ This rather arbitrary choice is tested hereafter.

Eqs. 20-21: These equations appear unexplained.

This equations have been replaced by the following text and equations (L312-321):

▷ Each cold pool provides a power associated with its horizontal spreading. This power is calculated as the product of the kinetic energy supplied by the pool and the mass flux, evaluated over the entire contour of the pool ($L_g = 2\pi r$), over the full height of the pool h_{wk} , and using the spreading velocity C_* . The power ($ALP_{\text{wk},i}$) of an individual cool pool i is therefore:

$$ALP_{\text{wk},i} = \frac{1}{2}\rho C_*^3 h_{\text{wk}} L_g. \quad (4)$$

To obtain the average power (ALP_{wk}) over all cold pools in the domain, this individual power is multiplied by the cold pool number density D_{wk} :

$$ALP_{\text{wk}} = ALP_{\text{wk},i} D_{\text{wk}}. \quad (5)$$

Since $\sigma_{\text{wk}} = D_{\text{wk}}\pi r^2$, the lifting power ALP_{wk} reads:

$$ALP_{\text{wk}} = \epsilon\rho C_*^3 h_{\text{wk}} \sqrt{\sigma_{\text{wk}} D_{\text{wk}} \pi}, \quad (6)$$

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?

Done in the text reproduced just above.

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.

Thanks a lot. We hope that the reviewer's comments helped us to do a better job in that respect.

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?

This line corresponds simply to an intermediate calculation allowing ALP_{wk} to be expressed as a function of σ_{wk} . Indeed, ALP_{wk} is computed by integrating over the entire perimeter of the cold pool (referred to here as the gust front length L_g), which is $2\pi r$ for a circle. The radius r can then be expressed as a function of σ_{wk} using equation (21). We have tried to explain it more clearly in the revised version. Please refer once again to the new paragraph reproduced above.

L270: There is no need to explain this.

We decided to skip this sentence however, as an introduction to LES.

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.

We replaced the statement by (L326-328):

▷ They provide an explicit and detailed representation of turbulent and convective motions within the boundary layer and of the associated clouds (see e. g. Brown et al., 2002; Siebesma et al., 2003, among many others).

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.

We did not want to give too many references but we added (L328-329):

▷ LES were used as well for assessing parameterizations of deep convection, including cold pools that are represented explicitly in such simulations (see e. g. Suselj et al., 2019).

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."

We edited this section carefully (please refer to the pdf file with change track, section 2.5). As explained in answer to the major comment above, one of the reasons for choosing two LES in the RCE regime is that it is an idealized case known to be highly sensitive to the model microphysics. Here is the sentence added to motivate this choice (L334-337):

▷ In the present study, we use the outputs of LES for two test cases, one over ocean and the other one over land. The test case over ocean was run with two different models, SAM and Meso-NH, with the exactly same setup in order to assess the uncertainty in LES, as it is known that RCE experiments are particularly sensitive to the microphysics scheme Wing et al. (2018).

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.

We agree that LES provides simulations that include an explicit representation of cold pools, and also about the remark concerning the time dependency. We added to the text (328-329):

▷ LES were used as well for assessing parameterizations of deep convection, including cold pools that are represented explicitly in such simulations (see e. g. Suselj et al., 2019) and, at the end of section 2.5:

▷ One advantage of RCE for parameterization development is that it targets a steady state regime. By contrasts, the AMMA case corresponds to a diurnal cycle of deep convection over land, for which the time matters, and for which errors in the phasing of the triggering of deep convection can alter comparisons.

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.

The surface fluxes are fixed here, as stated in the text. The AMMA case is not stationary: our intermediate analyses showed that cold pools vary significantly over the diurnal cycle. However, to avoid overly long analyses, we chose to examine the average properties of the cold pools over the entire period rather than analyzing them at each individual time step.

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

Done

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.

Yes, this also affects cloudiness. Where this difference of humidity is pronounced, LMDZ produces almost no clouds, likely due to the overly dry atmosphere obtained at these altitude levels. We agree to include this de-

scription in the text at the end of section 5.1. Here is the added part:

▷ At the altitudes where these dry biases are found, notably between 800 and 400 hPa for the RCE case and between 700 and 600 hPa for the AMMA case, LMDZ produces almost no clouds, likely due to an overly dry atmosphere (not shown).

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

We agree with this remark, which aligns with one of the comments from Reviewer 1. We provide more physical interpretation of the tuning results and comment on the modifications that led to these results. Below is the description we added to the text (section 5.4):

▷ The `htexplo` tool retrieves automatically cold pool model parameter values close to those issued from direct analysis of LES. This highlights both the power of using such a tool for model development, avoiding a long phase of trial error in a multi-dimensional space, and the relevance of the model physics. Likewise, the range of values selected for σ_{int} leads to cold pool heights of about 600 m in the RCE case and 2 km in the AMMA case, in agreement with the expected orders of magnitude over ocean and over land, respectively. One goal of the tuning is to intensify cold pools, particularly in the RCE case. The increase of σ_{desc} is primarily responsible for strengthening cold pools in this case, through stronger evaporation. This increases confidence in the validity of the physics implemented in the cold pool model. In the AMMA case, the opposite effect (less colder cold pools) is explained by the rather large increase in C_* (from 3 to 5 m/s), induced by the adjustment of k . This increase in C_* accelerates the increase of the cold pools fractional area σ_{wk} , reducing the efficiency of evaporative cooling, which can no longer compensate for the dilution due to their rapid evolution. Although this mechanism is also present in the RCE case (with an increase in C_* from about 1 to 2 m/s), it remains much less pronounced there, allowing the evaporation obtained with these σ_{desc} values to effectively strengthen the cold pools. We also consider that the increase of $1-EP_{max}$ and σ_{desc} played an important role in the improvement of the specific humidity profiles for the RCE and AMMA cases, although other parameters may also play a role. The increase of the difference $1-EP_{max}$ increases the amount of condensed water released at the top of the convective columns, thereby moistening the upper layers of the atmosphere. The value of EP_{max} was set to 0.999 in the control configuration. The increase in σ_{desc} may both enhance humidity through enhanced evaporation and dry the boundary layer, for the AMMA case in particular, due to increased entrainment and downward advection of dry air from the free troposphere.

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.

To answer this question, we added sentences to the last paragraph of the section 5.4 (L671-676):

▷ We also briefly examined the behavior of updrafts, given their strong coupling with downdrafts and cold pools. We note a decrease in undiluted adiabatic updrafts (which represent the theoretical capacity of the column to transport air and moisture) and a decrease in saturated updrafts in the tuned simulations (not shown). These effects are more pronounced in the AMMA case. The decrease in undiluted adiabatic updrafts would be linked to the drying of the boundary layer, itself associated with the increase in σ_{desc} , as explained previously. This reduction in undiluted updrafts would also contribute to the decrease in saturated updrafts by limiting the moisture supply to the mixing with environmental air.

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?

We understand the idea, but we do not think that this approach can correct the triggering delay obtained in the AMMA case. In LMDZ, the cold pool scheme is only activated once deep convection triggers. Rather, we think that this delay is related to a miss representation of the transition from shallow to deep convection, which we are currently working on. We have added sentences in the new conclusion (L717-724) to discuss this delay:

▷ For the AMMA case, the analysis of the diurnal cycle also shows that the adjusted model reproduces well the intensity and updraft power of cold pools at the time of their maximum development, but with a lead of

about 3 hours in the diurnal timing of this maximum. While this single case is not statistically significant it is worth mentioning that early convective initiation is a known limitation of LMDZ, even though the representation of the diurnal cycle of continental precipitation has significantly improved thanks to the inclusion of cold pool effects (Rio et al., 2009). The remaining lead in convective onset cannot be modified by the cold pool model itself, since the cold pool model is by essence not active before convective initiation. It is most probably a question of transition between shallow and deep convection and specific work are ongoing in the LMDZ team to better represent this transition.

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.

Following this recommendation, we have reworked almost the entire conclusion as can be checked directly on the pdf of the difference between the original and revised versions

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