Final author reply to the editor of submitted manuscript egu2025-5798 titled "Prognostic modeling of total specific humidity variance induced by shallow convective clouds in a GCM"

Louis d'Alençon[1], Frédéric Hourdin[1], and Catherine Rio[2]

[1]LMD/IPSL/Sorbonne Université, CNRS, Paris 75005, France
[2]CNRM, Université de Toulouse, Météo-France, CNRS, Toulouse, France

Correspondence: Louis d'Alençon (louis.dalencon@lmd.ipsl.fr)

We thank the referees and the community for their constructive comments. Below, we provide a point-by-point response to all comments and indicate the corresponding changes made in the revised manuscript.

1 Referee #1

General comments: *This study introduces a theoretical variance prognostic model that can be used to determine sources of variance in specific humidity associated with shallow convective cloud processes, specifically within a global climate model. Previously, diagnostic variance models have been utilized to investigate humidity asymmetries associated with shallow convection, but such methods may not be accurate when applied to deep convection. By leveraging thermal plume representation in the model's mass flux scheme, as well as the bi-Gaussian distribution of specific humidity, the prognostic model of variance can be constructed. Such a model contains three terms that affect the variance: the difference of mean plume and environmental humidity, the difference of variance in the plume and environment humidity, and the vertical transport of humidity due to subsidence. To assess the skill of the prognostic model, the authors apply the model into the cloud scheme of a GCM single column model version and compare it to large eddy simulation output from specific case studies. In such cases, it was found that the new prognostic and old diagnostic models had similar skill in assessing general variance, but the prognostic model performed significantly better when solely applied to thermal plumes where detrainment becomes an important source of variance. Skewness, or asymmetry in the humidity distribution, was also improved by the prognostic model. The construction of a prognostic model for assessing variance of variables within a climate model may not only improve the accuracy of such representations but may also prove to be computationally efficient. Overall, the paper is clear and well-written, especially in the sections walking through the construction of the prognostic model. Background information and a literature review concerning how variance of specific humidity is handled in GCMs is supplied in the introduction, along with a clear statement highlighting the goal of implementing the Klein et al. (2005) prognostic variance equation into a GCM. Though the construction and application of the model are delicate, there are a few suggestions for how the study may be improved. Such concerns mainly apply to how subsidence is addressed in the model and additional methods of application. Specific recommendations and suggestions are shown in detail below. General Recommendation: Accept for publication following minor revisions.*

Author's response: We thank the referee for this thorough and insightful summary of our work. We are grateful for the positive assessment and for recognizing both the clarity of the manuscript and the potential interest of the proposed prognostic variance model. We also appreciate the constructive suggestions for improvement, which we address in detail below.

1.1 Main Points

Main Point i)a)

In the subsidence term, given that vertical velocity in the thermal should always be positive, is the sign of the contribution only dependent on the vertical gradients of the environmental humidity (or in the case of your model, variance of humidity)? I suggest that, in the discussion of the compensating subsidence term, you include a brief description of how that process is physically tied to variance. I understand the descent term is small relative to the mean term, but I think more of an explanation of how vertical gradients contribute to compensating subsidence would be beneficial.

Author's response: We thank the reviewer for this comment, as it helped us identify an indexing error in the calculation of the variance tendency terms, leading to an inconsistency in Figure 6, as may have been noticed. These terms are computed separately and are not used in the standard version of the code, since specific humidity and its square are advected within the thermals, as described by Eqs. (15) and (17) of the manuscript. Consequently, this issue has no impact on the other results presented in the manuscript. As correctly pointed out, the sign of the subsidence contribution depends solely on the sign of the gradient, both in the total specific humidity budget and in its variance budget; therefore, a positive gradient leads to a positive contribution to the variance, which was not observed in Figure 6. We have revised the manuscript accordingly to address this point.

Author's changes in manuscript: Figure 6 has been corrected by properly reindexing the terms. The item on line 260 has been updated to clarify the role of the vertical variance gradient: **"**Since the mass flux is positive, this term implies that a positive vertical gradient of variance contributes positively to the variance budget at the level considered. In physical terms, a larger variance at an upper level will tend to enhance the variance at the level below, and conversely, a smaller variance above will reduce it.**"** Similarly, in the response to Detailed Comment 2, physical explanations have been added on line 189: **"**Because the mass flux f is positive, a positive vertical gradient of the physical variable results in a positive tendency, as air with larger values of ψ is transported downward by subsidence.**"**

Main Point i)b)

Along the lines above, I believe that it would be worth it to at least investigate the role of subsiding shell features. Undoubtedly, the climate models are unable to resolve the shells, but does LMDZ have any sort of parameterization that accounts for them? Though this is more so the case with deep convection (as found in Savre (2021)), subsiding shells may contribute to upwards of 10 percent of the downward mass flux at any given level, and this subsiding shell air would have different properties than that of both the plume and the ambient environment. I know that the Rio et al. (2010) paper you cited discusses how the shell features *may impact detrainment, which would then impact your convective variance tendency. Ultimately, it is up to you whether you would like to account for such features in your model, but would you consider investigating them?*

Author's response: Subsiding shells have first been indentified around shallow convective clouds Heus and Jonker (2008) and then have been shown to contribute to the mass-flux Glenn and Krueger (2013) and to around 10% of turbulent fluxes within deep convection Savre (2021). However, whithin shallow convection, Brient et al. (2023) in their figure 5 show that their vertically-integrated contribution to turbulent fluxes is of the order of 5 to 7%. Their contribution then appears less important than the one of sub-cloud layer downdrafts, also called dry tongues Couvreux et al. (2007). Then, in the LMDZ model, the representation of coherent dry tongues is of higher priority than the representation of subsiding shells. A dedicated parameterization and tuning effort has been tested, but it has not yet yielded published results. Therefore, paralleling this parameterization with that of the variance model is not yet planned, although both developments are progressing and will eventually be integrated — potentially by adding a third Gaussian term to the cloud scheme to represent the downdraft fluxes, which could impact the asymmetry of the water distribution. Also, in deep convective regimes, we will first focus on the representation of the impact of the precipitating downdrafts of the Emanuel scheme on the specific humidity variance rather than subsiding shells. In the thermal plume model used here, the existence of subsiding shells is rather taken into account via the detrainment term, by assuming that the evaporation of condensate at the edge of clouds favors detrainement, decreasing the updraft mass-flux. In the same spirit, the calculation of buoyancy within thermals has been adjusted by accounting for the virtual potential temperature of air originating from the layer above (Hourdin et al., 2019).

Author's changes in manuscript: A discussion sentence has been added in the conclusion of the article regarding subsiding shells (Line 457). "Furthermore, subsiding shells, whose convective transport contribution can reach from 5 to 10% (Glenn and Krueger, 2013; Savre, 2021; Brient et al., 2023), are not explicitly represented in our mass-flux model. However, as previously said, the detrainment parameterization includes certain aspects that indirectly account for their influence. The parameterization of sub-cloud layer downdrafts, also referred to as dry tongues Couvreux et al. (2007), is currently under development and could further contribute to the variance budget."

Furthermore, in response to detailed comment 3, the assumption Line 192 has been expanded by adding references on subsiding shells and the properties of air in the vicinity of cumulus clouds. "This hypothesis, which may be questioned, notably by investigating the impact of subsiding shells on the thermodynamic properties of the air in the vicinity of cumulus clouds (Heus and Jonker, 2008; Wang and Geerts, 2010; Christian Mallaun and Rotach, 2010), simplifies both mean and variance equation."

Main Point ii)

How are your entrainment and, more importantly, detrainment terms (e and d) determined in your cases? Are they directly output from LMDZ? I would imagine that in the tropics this is not too much of an issue, but it is possible that cases of moderate to strong vertical wind shear in the midlatitudes would impact these values and the variance tendencies.

Author's response: Entrainment and detrainment depends to first order on buoyancy. Strong winds are only indirectly modifying the thermal plume model through large scale environment modification and a drag term between the plume and its environment. To answer this questions, we added a full description of the entrainment and detrainment, which in fact helps also understand the role of the free parameters involved in tuning. It allowed us to suppress in turn vague sentences which were used in the submitted manuscript to compensate for the absence of a full description of the parameterization, when introducing those free parameters.

Author's changes in manuscript: The section from line 159 to line 183 has been added:

“The entrainment rate $\epsilon = e/f$ depends positively upon the plume buoyancy B, according to the following equation:

$$\varepsilon = \max\left[0, \frac{B1}{1+B1}\left(\frac{A1B}{\overline{w_{\mathrm{th}}^2}} - A2\right)\right] \tag{1}$$

where $A1$, $B1$, and $A2$ are tunable parameters.

The presence of parameters $A1$ and $A2$ in the computation of entrainment comes from the source term of the conservation equation for the vertical momentum (Eq. 6, discussed in detailed comment 1, with $\psi = w$). This source term reads $S = A1B - A2\overline{w_{\mathrm{th}}^2}$ and is the sum of a buoyancy and drag terms. Note that the plume fraction α is computed at each vertical level as $f/(\rho\overline{w_{\mathrm{th}}})$, f being computed from the mass conservation equation and $\overline{w_{\mathrm{th}}}$ from this vertical momentum conservation equation. For a given value of S, the source term in the vertical momentum conservation equation, parameter B1 (taking values in [0,1]) controls whether the plume velocity increases rapidly with no lateral entrainment (B1=0) or increases less rapidly with strong lateral entrainment of air from the environment, assumed at rest (large values of B1).

The detrainment rate, $\delta = d/f$, is mainly positive in the regions where the buoyancy is negative and reads

$$\delta = \max\left[0, -\frac{A1\times B1}{1+B1}\frac{B^*}{\overline{w_{\mathrm{th}}^2}} + CQ\left(\frac{\Delta q_t/q_t}{(\overline{w_{\mathrm{th}}}/w_0)^2}\right)^{0.5}\right] \tag{2}$$

with $w_0 = 1\ \mathrm{m\,s^{-1}}$. Δq_t is the contrast in humidity between the plume and its environment, and CQ is a tunable parameter. The CQ term was introduced to represent in part the enhancement of detrainment by the negative buoyancy subsequent to the evaporation of cloud water. The term is however active below the cloud as well. Note that a more physical parameterization of this CQ term is currently under development.

The computation of the buoyancy in the detrainment formulation reads:

$$B^* = g\frac{\theta_{v,th}(z) - \theta_v(z+\delta z)}{\theta_v(z+\delta z)} \tag{3}$$

where θ_v is the virtual potential temperature. In this formula, the buoyancy is computed by comparing the plume internal properties at altitude z with the environment at altitude $z+\delta z$, with $\delta z = DZ \times z$ (DZ being an adjustable parameter as well). This modification enabled the thermal plume model to simulate correctly the stratocumulus and transition cases, by making the plume "aware" of the top boundary layer inversion before reaching it, thus increasing artificially detrainment just below. As explained in Hourdin et al. (2019), it may in part account for the fact that the air detrained at a given level has followed an overshoot and then a rapid downward motion, as occuring in fountain structures above stratocumulus or in so-called subsiding shells, created around cumulus clouds by reevaporation of the detrained cloud water.”

Main Point iii)

This point is more so out of curiosity, but how would you expect the model to perform in cases of deep convection? In these cases, I would expect the contribution of subsidence to be greater both near and away from cloud, as well as in regions of precipitation. I look forward to looking at your future work that addresses this with Emanuel's scheme!

Author's response: We thank the reviewer for their interest in this work. The implementation of a prognostic variance model within the Emanuel scheme is currently under investigation. The sources of variance are more complex to describe in this framework due to the large number of updrafts, precipitating downdrafts, and the non-Lagrangian conservation of total specific humidity. Nevertheless, building on previous work in shallow convection and by including a specific model to represent the impact of precipitating downdrafts on the variance, it has already been possible to implement a preliminary version. First, provisional results indicate that the downdraft terms—particularly the precipitating ones in the vicinity of the cloud—have indeed a non-negligible impact.

Author's changes in manuscript: Some perspectives on extending this work to deep convection have been added in the conclusion (Line 465). **"**As a natural extension of this study, we are working on integrating the formalism of the Emanuel's scheme for deep convection into the variance model and further source terms associated with convective transport and evaporation of precipitation, with a particular focus on representing the impact of precipitating downdrafts from the Emanuel scheme on specific humidity variance. In contrast to shallow convection, these downdrafts are expected to have a significant effect on variance. Two competing processes may be involved: a drying effect associated with the descent of dry air, and a moistening effect linked to rain re-evaporation.**"**

1.2 Detailed Comments

Detailed Comment 1

Line 142: How is the value of a thermal plume variable determined in the model? Do certain thresholds of other variable have to be met for a thermal plume to be present in any grid cell? It may be beneficial to explain that here. The calculation of plume fraction α should also be addressed in line 145.

Author's response: The vertical profile of a physical variable ψ_{th} within the thermal plume is determined by vertically integrating upward from the surface a conservation equation (Eq. 4 in the new manuscript). The presence of an unstable layer near the surface is sufficient to initiate a thermal in the overlying 1D column. The plume fraction α is computed as $f/(\rho w_{th})$, f being computed from the integration of the mass continuity equation for the plume (Eq. 3 in the new manuscript) and w_{th} from the equation of conservation of vertical momentum (Eq. 4 in the new manuscript with a non zero source term).

Author's changes in manuscript: The paragraph starting at line 148 has been revised to include explanations on how the different quantities are computed within the thermals.

"The mass flux transport relies on the so-called thermal plume model (Hourdin et al., 2002; Rio and Hourdin, 2008; Rio et al., 2010). It is activated only if the surface layer is unstable and relies on an original closure in Convective Availabale Potential Energy (CAPE) described by Hourdin et al. (2002). It represents a population of thermal plumes through a single equivalent thermal plume with vertical velocity $\overline{w_{\rm th}}$ and surface fraction α. The vertical variation of the convective mass flux $f = \rho\alpha\overline{w_{\rm th}}$, assumed stationary during one physics time step, is related to a lateral entrainment e and detrainment d through a mass conservation equation:

$$\frac{\partial f}{\partial z} = e - d \tag{4}$$

The value of variable ψ within the thermal plume, noted $\overline{\psi_{\rm th}}$, is given by the conservation equation:

$$\frac{\partial f\psi}{\partial z} = e\bar{\psi} - d\overline{\psi_{\rm th}} + \alpha\rho S \tag{5}$$

where S is a source term for variable ψ ($S \equiv 0$ for a conserved variable) and α is the fraction of the surface covered by plumes.” and later on in the same section (Line 162)

“[...] the source term of the conservation equation for the vertical momentum (Eq. 4, with $\psi = w$). This source term reads $S = A1B - A2\overline{w_{th}}^2$ and is the sum of a buoyancy and drag terms. Note that the plume fraction α is computed at each vertical layer as $f/(\rho\overline{w_{th}})$, f being computed from the mass conservation equation and $\overline{w_{th}}$ from this vertical momentum conservation equation. ”

Detailed Comment 2

Line 153: It may be beneficial to explain physically why the compensating subsidence only depends on the vertical gradient of the variable, especially under the assumption that plume velocity is always positive.

Author’s response: We have followed this suggestion.

Author’s changes in manuscript: The sentence “Because the mass flux f is positive, a positive vertical gradient of the physical variable results in a positive tendency, as air with larger values of ψ is transported downward by subsidence.” has been added in the revised manuscript Line 189.

Detailed Comment 3

Line 155: This assumption would need to change if the implementation of a subsiding shell framework was added, given that entrained air would be moister. If you choose not to investigate this problem with shells included, it will not hurt to mention that the addition of shell would likely affect this assumption, just as was done in Rio et al. (2010)

Author’s response: We have followed this suggestion. More generally, the issue of organized subsiding structures has been discussed in the response to the main points.

Author’s changes in manuscript: The assumption Line 192 has been expanded by adding references on subsiding shells and the properties of air in the vicinity of cumulus clouds. “This hypothesis, which may be questioned, notably by investigating the impact of subsiding shells on the thermodynamic properties of the air in the vicinity of cumulus clouds (Heus and Jonker, 2008; Wang and Geerts, 2010; Christian Mallaun and Rotach, 2010), simplifies both mean and variance equation.”

Detailed Comment 4

Line 186: I suggest a brief sentence explaining (or hypothesizing) why the diagnostic approach is inaccurate for deep convective cases to further motivate why the prognostic approach is better. This could also be added into the brief discussion in line 50.

Author's response: In LMDZ, the bi-Gaussian scheme is used in the so-called large-scale condensation scheme, used for all kinds of clouds except those associated with deep convection. For deep convection, another PDF is used (log-normal with positive asymmetry) with also a width parameter computed from the value of the in-cloud water estimated by Emanuel's scheme (Bony and Emanuel (2001)). However, this PDF is not taken into account in the large-scale condensation scheme. Additional explanations have been provided to justify the implementation of a prognostic model that accounts for sources of variance associated with the intensity of lateral and vertical air exchanges. Although this paper is limited to shallow convection situations, these motivations were largely influenced by certain exaggerated cloud behaviors observed in deep convection, which led us to suspect an underestimation of variance by the diagnostic model.

Author's changes in manuscript: The following sentence has been added (Line 52) rather than at Line 227, to better emphasize these motivational aspects related to deep convection in the introduction:**"**In this scheme however, when the thermal plumes are absent or weak, the subgrid distribution reduces to a single gaussian, with a width imposed as a constant times the total water. In some cases with coexistence of shallow and deep convection, the bad representation of clouds in single columns simulations with LMDZ led us to hypothesize that the Gaussian width may be underestimated due to the omission of other sources of subgrid variance than thermal plumes as arising for instance from the detrainment of deep convection or from precipitating downdrafts.**"** We preferred not to repeat these considerations at line 227, but we can include them there if deemed necessary.

Detailed Comment 5

Line 271: With all of your cases occurring in the summer months at low altitudes, I wonder what impact ice would have on saturation deficit in the model. It may not hurt to discuss what potential implications this may have, especially in winter or when applied to deep convection.

Author's response: The LMDZ model is developed using an approach in which the distribution of water among its different phases is not retained from one time step to the next. At the beginning of each physics time step, condensates (both liquid and solid) are evaporated, and turbulence, as well as shallow convection, are parameterized based on total water and the liquid water temperature as a thermodynamic state variable. The new variance parameterization is likewise based on this total water content prior to recondensation. The presence of ice therefore has no direct impact on the computation of the variance or on that of the saturation deficit, since the partitioning of condensed water into ice and liquid occurs after these parameterizations. However, the saturation vapor mixing ratio q_{sat} is computed based on liquid–vapor or ice–vapor equilibrium depending on temperature, which may affect the resulting condensed water contents in cold regions. q_{sat} is lower under ice–vapor equilibrium than under liquid–vapor equilibrium, which is expected to increase the saturation deficit and the associated condensed water content.

In contrast, in the deep convection model, ice thermodynamics are explicitly taken into account throughout the convective processes in LMDZ. However, the variance and large-scale cloud schemes, as developed in this article, operates outside of these processes.

A priori, the presence of ice is expected to primarily modify the buoyancy of convective plumes through the release of latent heat, thereby altering the properties of the thermals and the associated transport of variance. In deep convection, ice could lead to enhanced precipitation, which in turn impacts the variance, through the melting and subsequent evaporation of hydrometeors.

Author's changes in manuscript: A sentence has been added in the conclusion to mention the importance of accounting for the presence of ice in clouds (Line 462). **"**This study focuses on liquid boundary-layer clouds. In the presence of ice, beyond thermodynamic effects associated with latent heat release (which enhances thermal updrafts), additional microphysical processes (e.g., condensation nuclei, supersaturation) should be considered and are indeed an active area of research within LMDZ.**"**

Detailed Comment 6

Line 290: This ties back to the main point concerning entrainment and detrainment. They both depend on the parameters you list, but how specifically? Do you use an equation to solve for e and d directly?

Author's response: For this comment, please refer to the response to the corresponding main point. Additional information on the calculations of entrainment and detrainment is provided there, in particular in Equations 1 and 2. The parameters influencing the calculation of entrainment and detrainment are A_1, A_2, B_1 and CQ. In particular A_1 and B_1 directly enhance the exchange rates in proportion to the buoyancy. It is worth noting that the tuning tool provides scatter plots for each metric–parameter pair across all simulations within a given wave. Although these plots are too extensive to be included in the manuscript, they allow for a more detailed assessment of the impact of each parameter on the selected metrics. Some parameters influencing the exchange rates, in particular B_1 are among the most critical in this tuning process.

Author's changes in manuscript: The expressions for entrainment and detrainment have been included in the manuscript from line 159 to line 183, as they are detailed in the response to Main Point ii.

Detailed Comment 7

Line 295: Is there precipitating convection in any of your cases? I am just curious to see how large of an impact the choice for CLC and EVAP parameters would have on results. I could imagine this would be much more significant in deep convection cases.

Author's response: Some of the cases analyzed in this study include precipitation, although it remains weak compared to convective precipitation in deep convection. As mentioned in the previous comment, the tuning tool allows us to assess the impact of each parameter across all metrics. In this framework, the CLC and EVAP parameters do not appear to be critical in the present shallow-convection study. The tuning for deep-convection cases has not yet been implemented, but parameters controlling precipitation and its evaporation (which are other parameters in LMDZ than CLC and EVAP acting for shallow convection) are indeed likely to play a more critical role in such regimes.

Detailed Comment 8

Line 324: I am assuming you do not show results for the IHOP simulation because it is a nearly cloudless environment. I suggest you add a sentence here confirming that is why no results are shown

Author's response: The IHOP case, which is almost cloud-free, is indeed used in our tuning process only to control the potential temperature profile. Since this potential temperature profile is only weakly affected by the new parameterization, we have not presented specific results for this case. We followed this suggestion and added an explanatory sentence.

Author's changes in manuscript: Line 377 a sentence has been added to clarify this point. "The results for the almost cloud-free IHOP case—used to control the potential temperature profile, which remains largely unchanged by the new parameterization—are not discussed here."

Detailed Comment 9

Line 340: Great job highlighting the potential negative impact of not having organized subsidence structures (like shells). I am curious if more areas of negative skewness would arise in deep convective regimes when subsidence near cloud is stronger.

Author's response: Elements addressing this comment have been provided in the responses to the main comments. In deep convection, a variance scheme specifically designed for precipitating downdrafts is currently under evaluation and may significantly contribute to the variance budget. However, within our bi-Gaussian framework, this does not necessarily result in a negative skewness; such an effect would require the introduction of a third component in the water distribution to extend it toward lower values. However, it could act as a sink term for the variance.

Author's changes in manuscript: Some perspectives on extending this work to deep convection have been added in the conclusion (Line 465). "As a natural extension of this study, we are working on integrating the formalism of the Emanuel's scheme for deep convection into the variance model and further source terms associated with convective transport and evaporation of precipitation, with a particular focus on representing the impact of precipitating downdrafts from the Emanuel scheme on specific humidity variance. In contrast to shallow convection, these downdrafts are expected to have a significant effect on variance. Two competing processes may be involved: a drying effect associated with the descent of dry air, and a moistening effect linked to rain re-evaporation."

Detailed Comment 10

Line 346: It may be beneficial to include a hypothesis as to how you would expect the variances to change in deep convection cases. My initial thoughts are that you would see greater contributions from the subsidence term (main point iii), so neglecting the variance and subsidence terms may not prove sufficient.

Author's response: We have followed this suggestion. Indeed, the first tests show a sometimes significant impact of precipitating downdrafts which seems to confirm this thoughts.

Author's changes in manuscript: Line 412 a sentence has been added to clarify this aspect: **"**In that context, however, particular attention will also need to be paid to precipitating downdrafts, which can have a significant, and often negative, impact on the variance and skewness.**"**

Detailed Comment 11

Table 1: How are the tuning metrics for each case chosen?

Author's response: In this work, we have stayed as close as possible to the metrics used for the standard model calibration ((Hourdin et al., 2021)). Here is a summary of the reasoning presented in that study: Various metrics were tested during preliminary experiments. The selected metrics were chosen to reflect key aspects of the simulations, including the thermodynamic state of the mixed layer and cloud characteristics. For cloudy cases, either total cloud cover or cloud height is used, with integral diagnostics preferred because they are less sensitive to vertical resolution than maximum cloud height. Metrics are averaged over time to smooth numerical oscillations, and specifically target features that are sensitive to parameter choices, such as the maximum cloud fraction in the RICO case and the boundary-layer development in transition cases ((Hourdin et al., 2019)). The final selection is somewhat arbitrary and relies on the modeler's expertise and objectives.

Author's changes in manuscript: Section 4.1 (Tuning setup, Line 326) has been rewritten to provide more details on our tuning procedure. **"**A long standing issue in parameterization development and improvement was the difficulty of retuning simultaneously the free parameters after a parameterization change, without which a physics improvement could be overled by a bad parameter tuning. This retuning is now made automatic thanks to the `htexplo` tool. The same tuning was applied here to the old model version and the new one with prognostic scheme. After a number of 'waves', we identify a number of configurations for both models, for which a series of metrics computed on SCM simulations of the IHOP, ARMCU, RICO and SANDU cases (Couvreux et al., 2021; Hourdin et al., 2021) match the metrics values computed on LES to less than a given tolerance to error σ. We use the same metrics as in paragraph 5.1 of Hourdin et al. (2021). These metrics are based on three model variables: potential temperature, humidity and cloud fraction. They are defined as temporal and spatial integrations over time and altitudes. For cloud fraction, three specific metrics are used: the first one is linked to the maximum cloud fraction on the vertical $\alpha_{cld,max}$, the second one represents an average altitude of clouds $z_{cld,ave}$, and the last one corresponds to the altitude of the maximum cloud fraction $z_{cld,max}$, computed as an averaged altitude weighted by the fourth power of the cloud fraction. Table 1 details all the different metrics used in this tuning (the three cloud metrics, and those concerning potential temperature θ and humidity q_v).

The free parameters are also kept the same as in Hourdin et al. (2021) except for the diagnostic model parameter of the diagnostic variance scheme $BG1$ and $BG2$ (Eqs. 15 and 14) which are replaced by parameters τ and τ_{th} in the prognostic scheme, allowed to vary between 100 s and 2000 s. The other free parameters, common to both tuning exercises, are: 1) those involved in the modeling of the entrainment and detrainment rates of the thermal plume model (Eqs 1 and 2): $A1$, $B1$, $B2$, CQ and DZ and 2) the CLC and EVAP parameters are involved in the precipitation and rain re-evaporation model, CLC being associated with the critical incloud water from which precipitation is activated and EVAP being a free parameter of the precipitation flux equation which controls the fraction of precipitation that re-evaporates at a given altitude.”

Detailed Comment 12

Figure 1: This figure is a bit difficult to interpret. I suggest spending more time explaining the figure and/or removing the wave simulations. For example, is it just in the max metric that you see the 0.75 score for the 10 simulations? Could it be worth it to remove the other metrics aside from this and do a one figure comparison between diagnostic and prognostic? Ultimately the decision is yours, but greater explanation of the figure may prove helpful.

Author’s response: We attempted to improve the readability of Figure 1 by removing the scores of the simulations for the different waves, as suggested. Only the scores of the 10 best simulations were retained. Regarding the second part of the comment, the maximum score is indeed reached for only one metric. However, for each of the best simulations, this metric can differ, which makes a direct comparison between the best simulations of the prognostic and diagnostic cases challenging. Additional explanations have been added to the text to facilitate interpretation of the figure.

Author’s changes in manuscript: Figure 1 has been revised for improved readability. In particular, the scores of the simulations for the different waves were removed, keeping only those of the 10 best simulations. This is expected to enhance the clarity of Figure 1. In the paragraph starting at line 360, the following explanations have been included: “From bottom to top, the figure displays the scores of the 10 best simulations across the 11 studied metrics. The second row shows the average score for each simulation, and the top row shows the maximum score of each simulation, corresponding to the worst score obtained among all metrics. As mentioned previously, the simulations are ranked according to the order of their maximum scores.”

Detailed Comment 13

Figure 2: I suggest adding plots of q here to show differences between environmental q and cloud q. This would help visualize regions of variance as well.

Author’s response: We followed this suggestion by adding a column to the previous Figure 4 and swapping Figures 3 and 4. As a result, the plot of q (thermal minus environment) now appears immediately after Figure 2 on Figure 3.

Author’s changes in manuscript: Figure 3 and 4 has been revised. Figures 3 and 4 have been exchanged, with Figure 3 now presenting a plot of the specific humidity difference between the thermals and their environment.

Detailed Comment 14

Figure 3: How was the choice for the specific times made? Do you see similar differences between the 3 models at different times?

Author’s response: The choice of the specific times for these diagnostics is closely linked to the carefully selected time intervals used for the metrics (see Detailed Comment 11). Of course, beyond these specific dates and selected metrics, we have extensively examined the temporal evolution of various quantities to ensure that no key aspects were overlooked. Not all of these analyses can be shown here, but some figures (Figures 4 and 5) include temporal evolutions over the full duration of the case considered. In general, the analyses at the selected dates are fairly representative of the entire simulation and highlight the most challenging aspects of the modeling, without hiding sometimes significant differences with the LES (see Figure 2 for the SANDU case).

Detailed Comment 15

Figure 6: If you could possibly find a way to adjust the legend so the plot is more visible, that would be beneficial.

Author's response: We have followed this suggestion.

Author's changes in manuscript: Figure 6 has been revised for improved readability.

1.3 Technical Comments

Technical Comment 1

Line 62: Move definition of LES to introduction (line 23). The same could also be done for LMDZ.

Author's response: We have followed this suggestion. In the same spirit, definitions of PDF and TKE have also been added.

Author's changes in manuscript: The definitions of LES and LMDZ have been moved (Lines 23 and 46), and the redundant definition has been removed. Additionally, a definition of PDF has been added in the Introduction (Line 25) and TKE, the turbulent kinetic energy, at Line 85.

Technical Comment 2

Line 77: Use of "is" instead of "are" for parameterization tested in the paper.

Author's response: The wording has been corrected.

Author's changes in manuscript: The sentence now reads "is used" instead of "are used" (Line 82).

Technical Comment 3

Line 93: Change spelling of "modified".

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 98 now correctly reads "modified" instead of "modifief".

Technical Comment 4

Line 94: Reserve use of "et" for citations only, otherwise use "and" throughout.

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 99 now correctly reads "and" instead of "et".

Technical Comment 5

Line 96: Put "great plains" in uppercase.

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 101 now correctly reads "Great Plains" instead of "great plains".

Technical Comment 6

Line 163: Correct spelling to "powerful".

Author's response: The sentence has been removed in the revised version of the manuscript.

Author's changes in manuscript: The sentence has been removed in the revised version of the manuscript.

Technical Comment 7

Line 166: Correct spelling to "asymmetry".

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 204 now correctly reads "asymmetry" instead of "asymetry".

Technical Comment 8

Line 310: Correct spelling to "Here".

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 357 now correctly reads "Here" instead of "Her".

2 Referee #2

General comments: *This study introduces and evaluates a prognostic description of total specific humidity variance. As nicely introduced by the study, the idea of prognostic equations for the higher moments of total humidity and their use in cloud schemes based on certain PDF families, is around for more than 20 years by now. Nevertheless, new available datasets and parameterizations give enough reason to continue the developments and test new implementations. In the presented study previous work is recaptured and the implementation in the LMDZ model is described. The implementation is mainly oriented on work by Klein et al (2005) to implement the prognostic terms of the variance by taking the different between environmental and detrained air.*

The evaluation is done in SCM mode based on 4-6 LES case studies covering different situations of shallow convection. The evaluation shows similar results for the cloud cover and variance between the new prognostic implementation and an old diagnostic implementation - but improvements for higher order moments as the skewness.

The study is tackling the important topic of cloud schemes, which didn't see too much progress over the last years to decades and it is following on the interesting development of prognostic PDF schemes. The text is nicely written and the evaluation results are very interesting.

Author's response: We thank the referee for this clear and informative overview of our work and for placing it in the context of previous developments. We are grateful for the positive assessment and for recognizing the relevance of revisiting prognostic approaches with new datasets and parameterizations. We also appreciate the encouraging evaluation of the results, and we address the referee's comments and suggestions in detail below.

2.1 Main Points

Main Point 1

The source terms for the prognostic variance are based on differences between the mean and variance of environmental vs. detrained air. Especially for the variance this was one of the main issues in implementing those equations as the variance might not be available from convection schemes. This study shows (Fig. 6) that mainly the difference of the mean value is acting as a source and the other terms could be neglected. This is contradicting the original publication (Klein et al. 2005, as also mentioned in l348-351) and sounds a bit like wishful thinking. The authors mention quickly, that this might be due to different sampling strategies. As it seems to be so sensitive and the question if 2 out of 3 source terms (the complicated ones) could be neglected is very important for the implementation, it would be a lot more convincing, if the authors could elaborate on this point and either test different sampling strategies or find a more reasonable argument for this difference between their own and the original publication.

Author's response: We thank the reviewer for this comment, which highlights a point that we addressed too quickly in the manuscript. The explanations as they were presented were not sufficiently detailed to justify the difference between our study and that of Klein, and we have taken this opportunity to revisit this point in more depth.

Specifically, of the three relevant source terms of variance, one is related to compensating downdrafts, and both studies are consistent in showing that it is of a smaller order of magnitude. The comparison of the other two terms is solely a comparison between differences in specific humidity (squared) and differences in variance between thermals and the environment. We mentioned the difference in convective sampling strategy, but this is not the most relevant point in the discussion. The value of specific humidity in the environment and its variance is relatively insensitive to sampling; the only changes due to sampling can come from the thermal terms, but these are modified only marginally. In particular, as shown in Figure 3 of the manuscript, and confirmed by observations (A. G. Williams (1993) for example) the standard deviations of humidity in both thermals and the environment are small compared to the difference between the mean values (in particular, the σ/qt ratio is on the order of 1–5 percent). So it is impossible, even with changes in sampling, to significantly increase the variance difference term. Unless one were to sample excessively wide thermals whose humidity is close to that of the environment, sampling cannot resolve a discrepancy of more than an order of magnitude.

The main difference arises from the fact that Klein's study concerns a case of deep convection, whereas our work—at least in this article—focuses on shallow convection cases. In these shallow convection cases, the study of differences in specific humidity and their variance in LES confirms the predominance of the first term, and sampling has only a minor effect.

However, in parallel with the writing of this article, our modeling work predicting variance in deep convection cases has been actively pursued, raising the question of accounting for the variance difference term. Indeed, preliminary LES analyses we conducted show that humidity distributions are much broader both in updrafts and in the environment relative to the mean humidity (the σ/qt ratio in particular is much higher than in shallow convection cases, on the order of 10–30 percent). It is therefore entirely plausible that the variance difference term could become one to two orders of magnitude larger in this case, which would then be consistent with Klein's study.

The LES analysis and tuning are not yet finalized, but we have implemented a first model for deep convection that explicitly accounts for the variance difference term. By summing the individual contributions to the variance of each mixed flux in the Emanuel scheme, we hope to accurately reflect the effect of humidity variance in updrafts. Preliminary tests have shown that this effect is indeed significant, although this work still needs to be confirmed.

Author's changes in manuscript: The sentences beginning at Line 407 has been restructured as follows: "This result is specific to shallow convective cases, where the standard deviations of humidity, both in thermals and in the environment, are small compared to the mean values and their difference (see Fig. 3 in the new manuscript). In contrast, in a deep convective case, such as that studied in K05, the ratio $r = \frac{\sigma}{q_t}$ is much higher, and one can therefore expect a significant impact from the variance difference term. K05 indeed shows that, in this case, the variance difference term is of the same order of magnitude as the squared humidity difference term. This point should not be overlooked when extending this study to deep convection cases."

Main Point 2

Figure 1 looks a bit overloaded. And the impression is even increased due to the overlay of legend and plot. I am not having a good idea or brilliant suggestion, but would like to motivate the authors to rethink if there could be an easier way to transport their message for the tuning experiments.

Author's response: We have followed this suggestion.

Author's changes in manuscript: Figure 1 has been revised for improved readability. In particular, the scores of the simulations for the different waves were removed, keeping only those of the 10 best simulations. This is expected to enhance the clarity of Figure 1. In the paragraph starting at line 360, the following explanations have been included: "From bottom to top, the figure displays the scores of the 10 best simulations across the 11 studied metrics. The second row shows the average score for each simulation, and the top row shows the maximum score of each simulation, corresponding to the worst score obtained among all metrics. As mentioned previously, the simulations are ranked according to the order of their maximum scores."

Main Point 3

Figure 2 is showing the representation of the cloud cover. The unit of the left column color bar is mentioned in the text, but it would be nice to also add the unit for the different plots. I assume it is still percent. In that case the differences are between 50 and -50 percent, which sounds quite a lot for me. Would it be possible to elaborate a bit in the text why this difference is so large? And why you don't worry about it? Also considering the very different amount of cloud fraction between the different cases, a relative difference might be more meaningful than an absolute difference.

Author's response: The unit (percent) has been indicated on the figure.

Regarding the significant differences in cloud cover between the LES and the SCM in the SANDU case. Large cloud-related errors represent a critical challenge. Although our model has significantly improved over the past two decade and performs well overall compared to other GCMs, particularly in shallow convection, significant errors still remain in some cloud fields. In this particular case, this is due to the fact that in the LES, the cloud cover is vertically shallow but locally very high, as can be seen particularly in Figure 4, left column. The difficulty of our GCM in reproducing this fine vertical structure is a problem we are well aware of and on which previous studies have been conducted ((Hourdin et al., 2019)). This difficulty leads to very large differences in this particular figure, although the new prognostic model partially reduces them without completely resolving the issue. More generally, it is important to emphasize that these cloud profiles are obtained from SCM simulations using the full model, i.e., including all components of the model.

Regarding the suggestion to show relative differences rather than absolute ones, we encounter difficulties when cloud cover vanishes in the LES or SCM. This results in extremely large relative differences that are difficult to represent graphically. For now, we have therefore made no changes in this regard, but we remain open to other suggestions.

Author's changes in manuscript: Figure 2 has been revised for improved readability. Several sentences in the paragraph have been revised starting from Line 414: "Accurately representing the vertical profile of cloud fraction during the stratocumulus-to-cumulus transition remains particularly challenging (SANDU case). Hourdin et al. (2019) showed that a carefully tuned modification of the detrainment parameterization represents an important first step toward simulating this transition." and later on, Line 417 "Moreover this cloud fraction is too thick at night in LMDZ especially from the second day of the simulation where it becomes significantly finer in the LES, leading to sometimes large differences in Fig. 2. Figures 3 and 4 show that both aspects are attenuated with the variance prognostic model, even though some notable differences still persist."

Main Point 4

There are several versions of bigaussian, bi-Gaussian, and Bigaussian in the text. It would be nice to have that consistent.

Author's response: The spelling has been corrected.

Author's changes in manuscript: We made sure to correct the spelling of the word "bi-Gaussian" throughout the text.

2.2 Minor/Technical Comments

Minor/Technical Comment 1

Line 6 : spelling of "asymmetry".

Author's response: The spelling has been corrected.

Author's changes in manuscript: We made sure to correct the spelling of the word "asymmetry" throughout the text.

Minor/Technical Comment 2

Line 93 : spelling of "modified".

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 98 now correctly reads "modified" instead of "modifief".

Minor/Technical Comment 3

Line 94/95 : blanks missing between Ref, Fast and (?) Slow.

Author's response: This correction has been implemented.

Author's changes in manuscript: Line 99 now correctly reads "They will be refered as IHOP/REF, ARMCU/REF, RICO/REF and SANDU/REF, FAST and SLOW".

Minor/Technical Comment 4

Line 114 : spelling of "facilitate".

Author's response: This sentence has been removed from the revised version of the manuscript.

Author's changes in manuscript: This sentence has been removed from the revised version of the manuscript.

Minor/Technical Comment 5

Line 158 : closing bracket missing.

Author's response: This correction has been implemented.

Author's changes in manuscript: Line 197 now correctly reads "$s = a_l(q_t - q_{sat}(T_l))$" instead of "$s = a_l(q_t - q_{sat}(T_l)$".

Minor/Technical Comment 6

Line 166 : spelling of "asymmetry".

Author's response: The spelling has been corrected.

Author's changes in manuscript: We made sure to correct the spelling of the word "asymmetry" throughout the text.

Minor/Technical Comment 7

Line 245 : very slower (?) - very slow ? or slower than?

Author's response: This correction has been implemented.

Author's changes in manuscript: Line 295 now correctly reads "with slower" instead of "with very slower".

Minor/Technical Comment 8

Line 310 : spelling of "Here".

Author's response: The spelling has been corrected.

Author's changes in manuscript: Line 357 now correctly reads "Here" instead of "Her".

Minor/Technical Comment 9

Figure 6 - it would be nice to not have the legend overlaying half of one plot.

Author's response: We have followed this suggestion.

Author's changes in manuscript: Figure 6 has been revised for improved readability.

References

A. G. Williams, J. M. H.: Interactions between coherent eddies in the lower convective boundary layer, Bound.-Lay. Meteorol., 64, 55–74, https://doi.org/https://doi.org/10.1007/BF00705662, 1993.

Bony, S. and Emanuel, K. A.: A Parameterization of the Cloudiness Associated with Cumulus Convection; Evaluation Using TOGA COARE Data, J. Atmos. Sci., 58, 3158–3183, https://doi.org/10.1175/1520-0469(2001)058<3158:APOTCA>2.0.CO;2, 2001.

Brient, F., Couvreux, F., Rio, C., and Honnert, R.: Coherent subsiding structures in large-eddy simulations of atmospheric boundary layers, QJR Meteorol Soc, 150, 834–856, https://doi.org/https://doi.org/10.1002/qj.4625, 2023.

Christian Mallaun, Andreas Giez, G. J. M. and Rotach, M. W.: Subsiding shells and the distribution of up- and downdraughts in warm cumulus clouds over land, Atmos. Chem. Phys., 19, 9769–9786, https://doi.org/https://doi.org/10.5194/acp-19-9769-2019, 2010.

Couvreux, F., Guichard, F., Masson, V., and Redelsperger, J.-L.: Negative water vapour skewness and dry tongues in the convective boundary layer: observations and large-eddy simulation budget analysis, Bound.-Lay. Meteorol., 123, 269–294, https://doi.org/https://doi.org/10.1007/s10546-006-9140-y, 2007.

Couvreux, F., Hourdin, F., Williamson, D., Roehrig, R., Volodina, V., Villefranque, N., Rio, C., Audouin, O., Salter, J., Bazile, E., Brient, F., Favot, F., Honnert, R., Lefebvre, M.-P., Madeleine, J.-B., Rodier, Q., and Xu, W.: Process-Based Climate Model Development Harnessing Machine Learning: I. A Calibration Tool for Parameterization Improvement, J. Adv. Model. Earth Syst., 13, e2020MS002 217, https://doi.org/10.1029/2020MS002217, 2021.

Glenn, I. B. and Krueger, S. K.: Downdrafts in the near cloud environment of deep convective updrafts, J. Adv. Model. Earth Syst., 6, 1–8, https://doi.org/https://doi.org/10.1002/2013MS000261, 2013.

Heus, T. and Jonker, H. J. J.: Subsiding Shells around Shallow Cumulus Clouds, J. Atmos. Sci., 65, 1003–1018, https://doi.org/https://doi.org/10.1175/2007JAS2322.1, 2008.

Hourdin, F., Couvreux, F., and Menut, L.: Parameterization of the Dry Convective Boundary Layer Based on a Mass Flux Representation of Thermals, J. Atmos. Sci., 59, 1105–1123, https://doi.org/10.1175/1520-0469(2002)059<1105:POTDCB>2.0.CO;2, 2002.

Hourdin, F., Jam, A., Rio, C., Couvreux, F., Sandu, I., Lefebvre, M.-P., Brient, F., and Idelkadi, A.: Unified Parameterization of Convective Boundary Layer Transport and Clouds With the Thermal Plume Model, J. Adv. Model. Earth Syst., 11, 2910–2933, https://doi.org/10.1029/2019MS001666, 2019.

Hourdin, F., Williamson, D., Rio, C., Couvreux, F., Roehrig, R., Villefranque, N., Musat, I., Fairhead, L., Diallo, F. B., and Volodina, V.: Process-Based Climate Model Development Harnessing Machine Learning: II. Model Calibration From Single Column to Global, J. Adv. Model. Earth Syst., 13, e2020MS002 225, https://doi.org/10.1029/2020MS002423, 2021.

Rio, C. and Hourdin, F.: A Thermal Plume Model for the Convective Boundary Layer: Representation of Cumulus Clouds, J. Atmos. Sci., 65, 407–425, https://doi.org/10.1175/2007JAS2256.1, 2008.

Rio, C., Hourdin, F., Couvreux, F., and Jam, A.: Resolved Versus Parametrized Boundary-Layer Plumes. Part II: Continuous Formulations of Mixing Rates for Mass-Flux Schemes, Bound.-Lay. Meteorol., 135, 469–483, https://doi.org/10.1007/s10546-010-9478-z, 2010.

Savre, J.: Formation and maintenance of subsiding shells around non-precipitating and precipitating cumulus clouds, QJR Meteorol Soc, 147, 728–745, https://doi.org/https://doi.org/10.1002/qj.3942, 2021.

Wang, Y. and Geerts, B.: Humidity variations across the edge of trade wind cumuli: Observations and dynamical implications, Atmos. Res., 97, 144–156, https://doi.org/https://doi.org/10.1016/j.atmosres.2010.03.017, 2010.