Response to Anonymous Reviewer 1

We thank the reviewer for their careful reading of the manuscript, their kind words, and their constructive suggestions. We are pleased that the reviewer finds our results valuable and our manuscript well written and worthy of publication. Below we provide detailed responses to each comment, with manuscript changes highlighted in blue.

Minor comments

- **Intro line 50:** *"perhaps expand a little on the main findings of Pauluis and Held?"*

  We have expanded the discussion of Pauluis and Held at line 52 by adding the following sentence:

  > They showed how, for moist convection, most of the irreversibility is attributed to processes involving water (diffusion of water vapor, irreversible phase-changes and drag on hydrometeors) rather than turbulent dissipation.

- **Section 2 line 70:** *"Shouldn't you consider cloud top radiative cooling as an internal source of entropy since the process (cooling) happens within the system? Please comment."*

  In this paper we follow the material system definition for analyzing the entropy production of the stratocumulus-topped boundary layer. Following the approach of Goody (2000), this means to consider all radiation as part of the surroundings, acting as an external heat source/sink throughout the system's domain. This effectively corresponds to replacing the irreversible interaction between matter and radiation with a reversible heat source/sink, which doesn't have any effect on the dynamics of the system. In this view, radiative cooling is treated as a heat source, entering the entropy budget of Eq. 3 on the left side. We added a clarifying sentence at line 77:

  > In particular, this translates into considering radiative cooling simply as an external heat sink for the system.

- **Section 2:** *"Please explicitly differentiate entropy S from s early on."*

  We have added at the start of Section 2, line 68, the following sentence:

  > Here and in the following, S denotes the entropy (J K^{-1}), while s represents the specific entropy ($\mathrm{J\,K^{-1}\,kg^{-1}}$).

- **Line 255:** *"Repeated "LES formulation"."*

  We have removed the second "LES formulation", with the new sentence now reading:

  > Consistent with the LES formulation, diffusive fluxes are derived using the subgrid-scale eddy diffusivity and the model's 3D diffusion scheme.

- **Line 149:** *"Subtitle 3.1 is probably not necessary since there is no 3.2 and further."*

  We removed the subtitle 3.1 and kept the subsection title "Entropy budget computation for LES".

- **Table 1 and section 4:** *"It would be informative to estimate the uncertainty associated with each contribution. I am wondering in particular how much of the discrepancies between your stratocumulus and the RCE cases could be explained by differences in the parameterization of* *moist processes. I am also wondering how much we can trust the heat dissipation contribution in RCE experiments given the relative coarse vertical grid and idealized surface fluxes. Please comment."*

Uncertainties for each contribution are estimated as the standard deviation (given the short time series) over the 1 h analysis period and are shown as error bars in Fig. 2. Given that these values are very small (roughly 1% for moist processes, much less for the other percentage contribution, and approximately 0.05 $\mathrm{mWK^{-1}m^{-2}}$ for the total entropy production), we chose not to include them in Table 1 to avoid overstating the precision of our estimates. We agree that the true uncertainty in the comparison between stratocumulus and RCE is better reflected by the physical differences between the two setups rather than by this statistical uncertainty.

Regarding the potential influence of differences in moist process parameterizations, we agree that these could contribute to the observed discrepancies. However, we believe that the dominant signal reflects the very different physics of the two systems, including the differences in vertical extent, moisture content, and the processes responsible for the reported entropy production. While it would be very interesting to investigate this further, this is outside the scope of the present manuscript.

Finally, on the heat diffusion contribution in RCE, we agree with the reviewer that a direct comparison with our stratocumulus simulations may not be straightforward. As already noted in the manuscript, the negative sign of this term in RCE is a result of the coarser vertical resolution and the fact that heat diffusion in the model is turbulent rather than molecular. We have added a sentence at line 201 of the manuscript to make this caution more explicit:

> However, given the strong differences in vertical resolution and model details between the two setups, we remark that a direct quantitative comparison of this term should be treated with caution.

- **Section 5, Line 212:** *"the sentence is unclear and should be rephrased."*

  We agree and have replaced the sentence with:

  > We now focus on moist processes, which represent the dominant contribution to the internal entropy production in both regimes.

- **Section 6:** *"A relevant reference to cite would be "Natural Convection as a Heat Engine: A Theory for CAPE", Renno and Ingersoll, `https://doi.org/10.1175/1520-0469(1996)053<0572:NCAAHE>2.0.CO;2`."*

  We thank the reviewer for the reference, we added it to line 258.

- **Section 6, lines 254-255:** *"Shouldn't surface friction be also considered to evaluate the total work produced? Also, figure A8 clearly shows the balance between TKE production and dissipation in the steady state atmosphere."*

  We thank the reviewer for this comment. Surface friction is indeed already included in the turbulent dissipation rate used, as it occurs in the surface layer just above the surface where turbulent dissipation acts. At the surface itself the velocity is zero, so there is no work done. As pointed out by the reviewer, Fig. A8 confirms that TKE production and dissipation are in balance in steady state, supporting our approximation of $\langle \dot{W}_K \rangle$ using the volumetric turbulent dissipation rate. We have included a clarification in the manuscript at Line 261:

  > In steady state, $\dot{W}_K$ can be approximated with the turbulent dissipation rate (Pauluis and Held, 2002a), which includes the contribution from surface friction.

- **Section 6, eq 14 and text:** *"I think that it is worth illustrating the energy balance with a figure or table to quantify and visualize the contribution from the 3 main energy sources and sinks mentioned (surface fluxes, radiative cooling and subsidence). It would also be useful to show that the boundary layer is energetically in equilibrium in both selected cases."*

  We have added a bar chart figure in the appendix (Fig. A6) to illustrate the energy balance for the open- and closed-cell cases. We have added the reference for the figure in Section 6, just before Eq. 14 (line 268).

- **Section 8, line 358-359:** *"Please rephrase the sentence. I am not sure the current formulation is at all correct."*

  We agree that the sentence as written is not fully correct. We rephrased it in the following way:

  > By utilizing a large ensemble, we are able to extract robust statistics. While the median total entropy production differs between the two morphologies, the considerable overlap between the two distributions suggests that entropy production alone is not sufficient to cleanly distinguish between open- and closed-cell convection.

- **Line 369-371:** *"This is grammatically awkward, please rephrase."*

  We agree that the sentence is indeed awkward, we rephrased it as:

  > One hypothesis is the maximum entropy production principle [references], which suggests that systems select states that maximize dissipation, though its validity remains debated.

- **Line 376 onward:** *"I would refrain from using such an argument, or at least would not emphasize it, simply because the system considered is not closed and is to a large extent driven by external fluxes and exchanges of energy (radiation and subsidence are treated as external sources)."*

  We thank the reviewer for the comment. We have toned down the language in this paragraph by removing the last two sentences, to avoid overreaching conclusions and risking a misinterpretation of our results. We note that the usual version of the Maximum Entropy Production (MEP) principle is formulated for non-equilibrium, open systems with strong energy fluxes at the boundaries, which is indeed the case here. With this paragraph, we intended to make a purely empirical observation: not only is the total amount of dissipation not a good metric to distinguish between the two states, but the most probable state appears to be the one with the lower entropy production, as opposed to what expected is from the MEP. We believe this is worth reporting and agree to not draw stronger conclusions given the complicated non-equilibrium nature of our system. We have revised the paragraph to make the limited claim clearer.

- **Appendix:** *"Appendix B is referenced before Appendix A. You should swap them to follow this order."*

  We have swapped them to follow the reference order.

Response to Anonymous Reviewer 2

We thank the reviewer for their careful reading of the manuscript, their kind words, and their constructive suggestions. We are pleased that the reviewer finds our results valuable and worthy of publication. Below we provide detailed responses to each comment, with manuscript changes highlighted in blue.

General comments: Case Selection

- *"Why are all cases nocturnal? A brief justification in the text would be helpful."*

  The dataset used in this study consists exclusively of nocturnal cases, as it was designed to study the evolution of stratocumulus convection in a simpler setup without the diurnal cycle forcing. This is particularly useful for our analysis, since it allows the system to reach a quasi-steady-state configuration relatively quickly, avoiding the strongly forced limit-cycle behavior associated with the diurnal cycle, which would complicate the analysis presented here and require much longer simulations to obtain steady statistics. Extending this framework to cases with a diurnal cycle would be a very interesting direction for future work. We have added a justification in the text at the end of the first paragraph of Section 3 (line 131):

  > The nocturnal setup was chosen to avoid the complexity of diurnal cycle forcing, allowing the system to equilibrate into a simpler quasi-steady state.

- *"Additionally, you reference Glassmeier et al. (2019) as a justification for excluding cases that don't produce clouds or precipitate quickly, but isn't this essentially excluding a third stable mode? Even though is a relatively small proportion of the total ((191-159)/191=0.16 = 16%), it doesn't seem small enough to justify excluding them even if the overall science question concerns stratocumulus clouds. Are these excluded cases examples of increased efficiency? How much different are the open cell cases from these clear cases given that there isn't as much cloud water in the open cell cases? Are they essentially the same as the clear cases that were excluded?"*

  These cases fall into two categories. Cases that do not produce clouds were not simulated at all, as the initial conditions would place the lifting condensation level above the inversion height, preventing cloud formation. For the strongly precipitating cases, these were excluded as non-physical. The model includes a 2-hour spin-up during which the cloud microphysics module is switched off. Once the spin-up ends and microphysics is activated, these cases precipitate out immediately, leading to a non-physical dissipation of the cloud layer. They are therefore considered artifacts of the model rather than a physical third stable mode. We have added the following sentence in the third paragraph of Section 3 (line 144):

  > The former correspond to lifting condensation levels above the inversion, while early precipitation cases exhibit rapid, non-physical dissipation of the cloud layer immediately after the 2 h spin-up.

Specific Comments

- **Lines 75-80:** *"Should turbulence dissipation be in (3)? I assume these are the "diffusive" terms?"*

  Turbulence dissipation indeed enters Eq. 3 as an implicit contribution to the specific internal production of entropy on the right-hand side. It is explicitly accounted for in the decomposition of the specific internal production of entropy in Eq. 5.

- **Lines 75-80:** *"Additionally, should the term be referred to as "specific production of entropy"? The term "specific" is typically used when mass or density are normalized out.."*

  The variable indicated by the letter "s" (as opposed to the capital S) indeed is normalized by density, and it has units of entropy (J/K) divided by density (kg/m3). We have added a sentence at line 68 for clarification:

  > Here and in the following, S denotes the entropy (J K^{-1}), while s represents the specific entropy ($\mathrm{J\,K^{-1}\,kg^{-1}}$).

- **Line 81:** *"Please clarify what it meant by "sum over repeated indices"."*

  By "sum over repeated indices" we mean the convection where if an index is repeated, a sum over them is implied (usually referred to as Einstein notation, or Einstein summation convention). This refers to the "x" index, which represents the species in the atmospheric fluid (dry air, water vapor and liquid water). We have updated the manuscript and made the summation explicit in Eq. 3.

- **Lines 105-110:** *"How is it possible to have "frictional dissipation due to turbulent motion"? Molecules do not "rub" together. Turbulent dissipation increases the kinetic energy of the gas, which is NOT friction."*

  We apologize for the confusion, and we agree that the terminology could be misleading. By "frictional dissipation due to turbulent motion" we refer to the irreversible process by which kinetic energy is transferred from the large scales to smaller scales through the turbulent cascade, until it reaches scales small enough for viscosity to act, converting it irreversibly into heat. We used the term to be consistent with previous literature (e.g. Pauluis and Held, 2002a; Singh and O'Neill, 2022). We have rephrased the sentence to make it clearer, by explicitly mentioning viscous dissipation (line 110):

  > [..] frictional dissipation due to viscosity at the end of the turbulent cascade, [..].

  We would like to keep, if possible, the label of "frictional dissipation" for consistency with the nomenclature used in previous literature.

- **Lines 178-180:** *"I am not sure it is "surprising" that it occupies only 1% of the mass but is responsible for 90% of the entropy production. The remaining constituents cannot undergo a phase change and, other than turbulent dissipation, have no means to increase entropy because they are likely dominated by adiabatic processes."*

  We agree with this observation and have rephrased the sentence to better reflect the physical motivation behind it, and removed the word "striking" (line 185):

  > This reflects the unique role of water vapor which, despite accounting for less than 1 % of the domain mass, is the only constituent able to undergo phase changes.

- **Line 200:** *"It is difficult to conclude from Fig. A3 that open cells show a stronger contribution from horizontal gradients, since the values are both very small and y-axis ranges differ between panels. Mentioning the actual values in the figure or text would strengthen this statement."*

  We have added the following values to the figure caption:

  > Horizontal contributions account for approximately 2 % and 1 % of the total for water vapor mixing in the open- and closed-cell cases respectively, and less than 1 % for sensible heat in both cases.

  We have also clarified the statement in the text to specify that the larger horizontal contribution in open cells is specific to water vapor mixing (line 207):

  > While open cells show a stronger contribution from horizontal gradients in water vapor mixing, diffusion remains dominated by vertical gradients.

- **Lines 208-210:** *"The word "roughly" appeared twice in the same sentence. Consider revising for better readability."*

  We apologize for the oversight and have updated the sentence by removing the repeated use of "roughly" (line 216):

  > An order of magnitude smaller vertical extent and temperature difference between stratocumulus and RCE translates to approximately an order of magnitude smaller total entropy production.

- **Line 265:** *""First, the shallow vertical extent of the system severely limits the available temperature gradient. This in turn constrains the maximum theoretical work that could be generated from a given set of boundary fluxes and effective temperatures (Pauluis and Held, 2002a)." This is an important statement and maybe the most important in the paper."*

  We thank the reviewer for this comment. We agree that this is a central result of our analysis and one of the key contributions of this paper. We have slightly rephrased the sentence to make our point stronger, also expanding on it following reviewer 3's comments (line 273):

  > First, the maximum theoretical work available to the system is strongly constrained. The shallow vertical extent of the system severely limits the available temperature gradient, which in turn constrains the maximum work that could be generated from a given set of boundary fluxes and effective temperatures (Pauluis and Held, 2002a).

- **Line 319:** *""Large number (> 100) of closed-cell cases" – It would be helpful to explicitly state the number of open- vs. closed- cell cases, either here or earlier in section 3."*

  We added the number of open- vs. closed-cell cases in section 3 (line 147):

  > The final dataset thus comprises 155 simulations, with 131 closed-cell and 24 open-cell cases.

- **Line 369:** *"Capital "M" should be made lowercase or capitalize the entire "Maximum Entropy Production"."*

  We have made the capital "M" lowercase.

- **Lines 374-377:** *""A stochastic analysis of our LES ensemble shows that the probabilistic landscape is not symmetric, and the open-cell state is the globally stable one (Hernandez and Glassmeier, personal communication, 2026). Therefore, the total irreversible entropy production does not appear to select the observed state, and the open-cell configuration, contrary to a maximum entropy production expectation, instead has a lower median dissipation." The term "globally stable one" needs a bit more explanation. Are you referring to "global" in terms of the ensemble of LES simulations or "global" in terms of the global coverage being greater? I wonder if the Entropy Maximization Hypothesis is relevant only if the holistic global cloud system is considered. In other words, maybe entropy maximization includes an integral that includes all such systems operating globally at any point in time? In other words, maybe inefficient closed cellular structure exists over a larger area and/or last for a longer period, thereby increasing its overall global entropy increase."*

  We thank the reviewer for this comment, and for this interesting perspective on the global cloud system. We apologize for the confusion: the term "globally stable" in this paragraph refers to the stability landscape of the LES ensemble, not to global geographical coverage. Specifically, for this argument we consider the evolution of the ensemble over a bistable landscape, which can be approximated by two potential wells, one for each morphology into which the cloud fields relax during their evolution. For multistable systems such as this, it has been argued (e.g., Endres, 2017) that the more stable state (the "deeper" of the two wells) is the one that dissipates the most, which represents a version of the maximum entropy production argument for multistable systems. Interestingly, in another study (Hernandez and Glassmeier, 2026, preprint) we find that the open-cell state is the more stable one (or as it is often referred to in the dynamical systems literature, the "globally stable" state). This appears to be in contrast with the maximum entropy production argument, since in this manuscript we find that the open-cell state exhibits a lower median dissipation than the closed-cell state. We agree that, for a geographically global argument, things might be more complicated: the lifetime of individual cloud systems, their spatial coverage, interactions with other components of the Earth system, and the potential "export" of dissipation beyond the local domain would all need to be taken into account. To avoid confusion, we have replaced the term "globally" in the main text, and rephrased the sentence as (line 403):

> A stochastic analysis of our LES ensemble shows that the probabilistic landscape is not symmetric, with the open-cell state being the more stable of the two.

- **Figure A2 and A3** *"Fig. A3 is referenced before Fig. A2 in the text. Consider switching the order of these two figures for consistency."*

  We have swapped the order of these two figures.

- **Appendix B** *""TKE" is capitalized in Fig. A8 caption but lowercased in the text. I suggest capitalizing "TKE" throughout."*

  We have capitalized "TKE" throughout.

Response to Olivier Pauluis

We thank the reviewer for their careful reading of the manuscript and for their positive assessment of our work. We are pleased that the reviewer finds our results important and worthy of publication. We would like to apologize for missing the very relevant pieces of literature mentioned in the review, and thank the reviewer for pointing them out. We believe these additions have strengthened the manuscript. We are also grateful for the insightful comment on the work done to lift water, which we believe is another important addition to the manuscript. Below we provide detailed responses to each comment, with manuscript changes highlighted in blue.

1 Discussion of the existing literature

1.1 Water vapor and mechanical efficiency

Reviewer's comment

Pauluis (2011) introduces the steam cycle as an analog of the Carnot cycle, in which the energy source is replaced by the injection of water vapor and provides an explicit expression of its mechanical output (see eq. 15 and 25 as well as Figure 3). A central finding in Pauluis (2010) is that the efficiency of atmospheric circulation depends on the relative humidity at which it operates. In particular, shallow convection in a dry environment would be particularly inefficient (see the discussion of Figure 3...).

In Sections 6, 7, and 8 of the manuscripts, the authors present some arguments for why the stratocumulus is inefficient. These arguments, in many ways, repeat parts of the discussion in Pauluis (2010).

Authors' response

We thank the reviewer for pointing out this very relevant reference, and for helping us better understand our results. We have added a discussion of the role of relative humidity in limiting the mechanical efficiency, as described in Pauluis (2011). We refrained from attributing the lower mechanical efficiency of stratocumulus (compared to RCE) to differences in relative humidity as, at least in our simulations, the relative humidity in the stratocumulus mixed layer is actually quite high, with slab-averaged values of $70 - 80\%$ for closed cells, and reaching 95% in some open-cell cases, comparable to those observed in RCE. We have added the following sentences in section 6 (L. 278):

> Furthermore, as argued by Pauluis (2011), the relative humidity at which the system operates also has an impact on the maximum amount of work, with drier systems being less efficient than moister ones. A thermodynamic cycle operating in a partially saturated environment has a maximum efficiency that increases with the degree of saturation, approaching the Carnot limit only in the fully saturated case. The departure from full saturation of the stratocumulus mixed layer therefore provides an additional constraint on the maximum extractable work.

1.2 Moist processes have a much greater impact on shallow convection than deep convection

Reviewer's comment

In Sections 6 and 7, the authors argue that the efficiency of shallow convection is quite small, with a much larger share of the entropy production attributable to moist processes. This is very similar to some of the results presented in Pauluis (2016). This paper introduces a methodology to reconstruct the thermodynamic cycles that underpin an atmospheric flow. It also argues that shallow thermodynamic cycles will be more strongly affected by moist processes (see the discussion after eq. 21):

- "This implies that the relative importance of the Gibbs penalty is inversely proportional to the depth of the cycle, measured here by the temperature difference between the heat source and the heat sink." (p.4417)
- "For shallow convection, the scaling implies that the Gibbs penalty should be on the same order of magnitude as the maximum work." (p.4418)

Authors' response

We agree that our results, particularly from the ensemble analysis in Section 7, are consistent with and support the reviewer's findings on the relative importance of moist processes in shallower cycles. We have added the following sentences at the end of Section 7 to acknowledge this connection (L.353):

> Furthermore, both morphologies show, on average, a higher share of entropy production attributable to moist processes compared to previous RCE results (Pauluis and Held, 2002a, Singh and O'Neill, 2022). This is consistent with previous work of Pauluis (2016), who extracted thermodynamic cycles of different depths from RCE convection and found that shallower cycles show a higher share of moist processes compared to deeper ones. Together with the results presented in this paper, this supports the relative importance of moist processes scaling inversely with the depth of the thermodynamic cycle.

1.3 Mechanical efficiency of atmospheric flows

Reviewer's comment

The fact that moist processes significantly affect the mechanical output of atmospheric flows has been established in numerous studies. Many of these have been ignored by the authors, who mention only a limited number. In addition to the two papers mentioned above, I would point to:

- Several papers have investigated tropical cyclones (Pauluis and Zhang (JAS, 2017), Fang, Pauluis and Zhang (JAS 2017), Regibeau-Rocket, Pauluis and O'Neill (J Climate, 2023).
- Laliberte et al. (Science, 2015) show the impacts of moist processes on the global circulation.

Authors' response

We apologize for the oversight, and we thank the reviewer for the relevant list of references. We have added the references pertaining to the tropical cyclones at L. 55, and the reference to Laliberte et al. (Science, 2015) as L. 49.

2 Water lifting vs. precipitation-induced dissipation

Reviewer's comment

Dissipation of kinetic energy by falling precipitation is a direct result of the fact that the atmosphere continuously lifts water. In RCE, the work done to lift the water exactly balances the precipitation-induced dissipation. This is not the case in the simulations presented here, as a large-scale subsidence continuously "removes" water from the domain. In this context, the work done to lift water exceeds the dissipation by precipitation. This is something that authors should document. In particular, they show that the open-cell convection exhibits a fair amount of dissipation by precipitation in contrast to the closed-cell case. However, it is likely that the work done to lift the water against the large-scale subsidence is quite significant (meaning larger than the precipitation-induced dissipation). While the argument could be made that the lifting does no directly correspond to an entropy source (albeit the water will fall at some point and thus dissipation will occur somewhere else in the atmosphere), it would be useful to document it and discuss it in the context of the efficiency (it is likely that it is larger than the generation of kinetic energy...).

Authors' response

Following the reviewer's suggestion, we have computed the work done to lift water in the domain as $\dot{W}_{\mathrm{q}} = \int \rho g w q_{\mathrm{t}} \mathrm{d}V$, where q_{t} is the total water mixing ratio and w is the resolved vertical velocity. We find time-averaged values of 258 mWm^{-2} for the open-cell case and 486 mWm^{-2} for the closed-cell case. As the reviewer predicted, these values largely exceed both the kinetic energy generation (20 and 70 mWm^{-2} for open and closed cells respectively) and the dissipation by falling hydrometeors (183 mWm^{-2} for open cells and negligible for closed cells). We have added the following paragraph at the end of Section 6 to document and discuss this point (L. 287):

> Beyond the inefficient generation of kinetic energy, stratocumulus perform substantial work in lifting water within the domain against the large-scale subsidence, which we estimate as
>
> $$\langle \dot{W}_q \rangle = \frac{1}{A} \int_{\Omega} \langle \rho g w q_t \rangle dV, \qquad (1)$$
>
> where q_t is the total water mixing ratio and w the resolved vertical velocity. Time-averaged values for $\langle \dot{W}_q \rangle$ are 258 and 486 mWm^{-2} for the open- and closed-cell cases respectively, largely exceeding $\langle \dot{W}_K \rangle$ (24 and 68 mWm^{-2}). In RCE, at steady-state this work is balanced by the dissipation of kinetic energy by falling hydrometeors within the domain (Pauluis and Held, 2002a). This balance does not hold in our simulations: open cells dissipate only approximately 183 mWm^{-2} through precipitation, while the non-drizzling closed-cell case has negligible precipitation dissipation. In the real atmosphere, the water removed by subsidence would eventually precipitate in other regions, where the associated dissipation would occur. However, since this process does not happen inside our simulation domain, the associated dissipation is not accounted for in our entropy budget.

Minor comments

- **L. 114:** *"'the frictional effect on falling hydrometeors': please attribute to Pauluis, Balaji and Held (JAS,2000) for the first discussion of rain as a dissipative process and a significant source of entropy."*

  We thank the reviewer for the reference and have added it at L. 121 as suggested. We have also corrected the term from *friction* to *frictional* for consistency.

- **Figure 1:** *"It would be useful to document the water content as well, and possibly where the cloud layer is located in the vertical cross-section. (panels a and d)."*

  We have added the total water mixing ratio q_t profile to panels (a) and (d) of Figure 1 as suggested. Regarding the cloud layer location, we have highlighted it only for the closed-cell case, where it is sufficiently well-defined. For the open-cell case, the cloud layer is unfortunately difficult to identify unambiguously due to the highly heterogeneous nature of the cloud field and the presence of drizzle in the domain and at the surface. This is illustrated in the figure below, showing the vertical profile of cloud water and a cross-section. While the cloud layer starts appearing just below the inversion (above $\sim$ 800 m), the presence of drizzle makes the cloud base hard to cleanly identify. The increased cloud water in the lower part of the domain is due to rain evaporation, which can shift rain back into the cloud-droplet category of the microphysics scheme. This is a known feature related to how the microphysics module classifies rain and cloud water based on their geometric mean radius at the beginning of the bulk scheme.

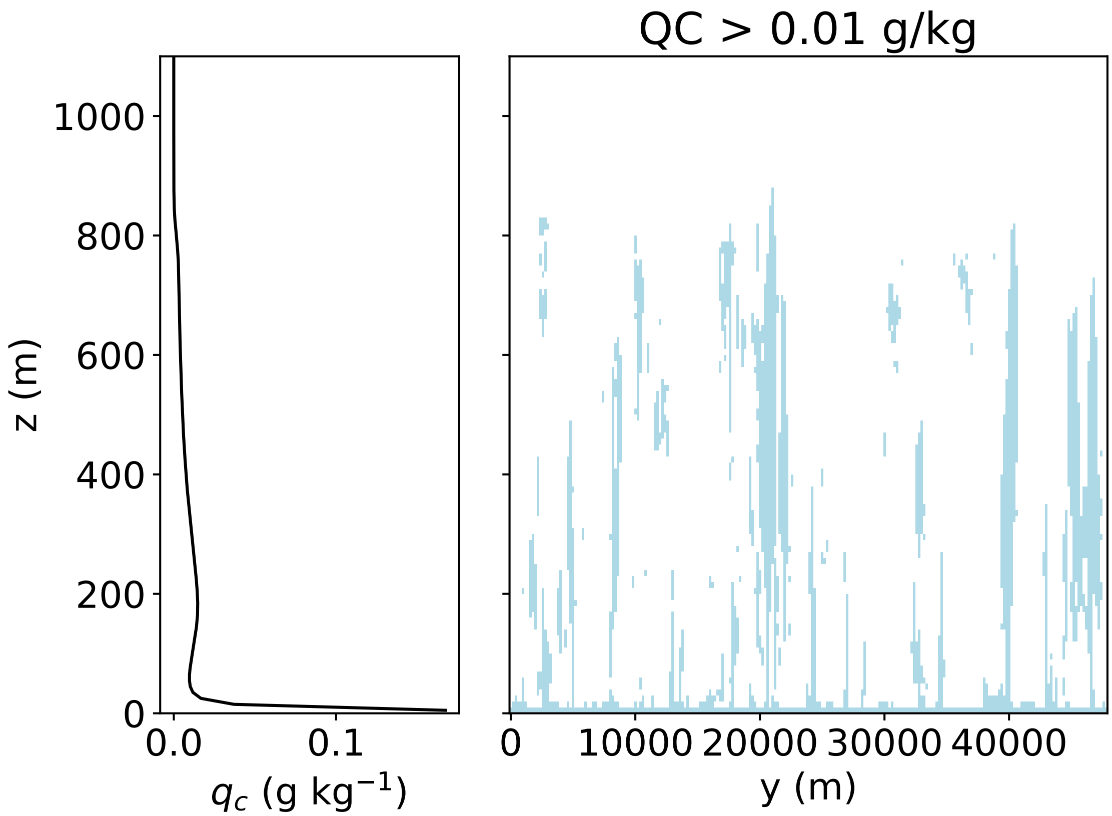

Figure 1: **Open-cell cloud water**. Profile of domain-mean cloud water mixing ratio q_c (averaged over the last hour) on the left, and cross-section of q_c (showing regions where $q_c > 0.01\ \mathrm{g\,kg^{-1}}$) on the right.