

## 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/m<sup>3</sup>). We have added a sentence at line 68 for clarification:

Here and in the following,  $S$  denotes the entropy (J K<sup>-1</sup>), while  $s$  represents the specific entropy (J K<sup>-1</sup> kg<sup>-1</sup>).

- **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.