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.