We thank the reviewers for their supportive comments.

In the rest of this file, we show the comments from Reviewers #1 and #2 in red and blue, respectively, and our response in black.

After addressing reviewers' comments, we list other minor edits we have made since the first revision.

Reviewer #1

Review of "Diurnal evolution of non-precipitating marine stratocumuli in an LES ensemble" by Chen, Zhang, Hoffman, Yamaguchi, Glassmeier, Zhou and Feingold, Manuscript egusphere-2024-1033, First Revision

I thank the authors for their careful consideration of and responses to the many reviewer comments. The new set of SST+0.5K simulations was an unexpected bonus and strengthens the paper, I think. I would recommend that the paper be accepted after the authors have a chance to consider the minor comments below.

Recommendation: Minor Revisions

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Specific/minor comments (10/277 means p. 10, line 277, ALL LINE NUMBERS FROM TRACK CHANGES VERSION):

7/204-212: While the other reviewer and I had criticisms of the way the forcings and initial conditions for the ensemble were chosen, there is no need to be overly apologetic in describing the ensemble and what one might learn from it. The ambition of these ensembles is impressive, and there is a lot to be learned from them. I believe the sentences about the limitations of the ensemble design be phrased in a more positive manner, something like "While design of the ensemble did not account for the observed co-variation environmental conditions in simulation configurations, ..., the ensemble captures features of the observed diurnal cycles of marine stratocumulus, suggesting that analysis of the statical behavior of the ensemble could be valuable."

Thanks to the reviewer for understanding. This is exactly the point that we tried to convey: this LES ensemble does capture a range of very realistic diurnal cycles in observed clouds and thus could be valuable.

Please see the updated text in L216–L218 in the tracking-changes file.

19/sec. 6.2: The residuals in the LWPc cloud volume budget for the SST+0.5K hiD cases are stronger during the afternoon than in the fSST ensemble. Is this mainly related to the smaller afternoon cloud fractions seen in some simulations in the SST+0.5K ensemble? Or is there some other cause?

We believe that it is because hiD clouds are more decoupled from the surface in the SST+0.5K set, comparing Figure S8 and Figure 2 (same as the plots in the files uploaded for the first revision). This is consistent with the weaker surface fluxes with narrower differences between SST and surface air temperature (comparing Figures S9 and S2).

Very decoupled conditions are challenging to handle because the assumption of a well-mixed cloud volume is less likely to hold. Also, rapid ascending/descending of the cloud base may introduce larger uncertainly in the BM term.

Under more decoupled conditions, we agree that cloud fractions would be smaller in SST0.5K+ set.

Please see updated text in L583–584 in the tracking-changes file.

20/591: Regarding, "... we explore the impacts of diurnal cycles and free tropospheric humidity on the cloud system evolution of non-precipitating marine stratocumuli ..." This manuscript explores much more than just the impacts of the diurnal cycle and FT humidity, so I would suggest that the list be made more inclusive (and longer) or rephrased as something like "... impacts of diurnal cycles and varying atmospheric states on ...". I'm not sure "atmospheric states" is exactly right, but some catchall phrase like that might be more accurate than just "FT humidity".

We agree that the current scope of the manuscript is broader than just the impacts of diurnal cycles and FT humidity, which was the starting point of this study. We now reword this sentence to make it more consistent with the current scope. Please see updated text in L591–593 in the tracking-changes file.

The texts in Abstract and Introduction have also been updated accordingly. Please see L2–L3 and L71–73 in the tracking-changes file.

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Typographical/rephrasing suggestions (OPTIONAL):

5/129: "anisotropic"

Done. Please see L129 in the tracking-changes file.

7/206: comma before "while"

Done. Please see L211 in the tracking-changes file.

Reviewer #2

The authors addressed most of my major concerns satisfactorily. I only have two minor comments:

- the reason for disregarding the states with cloud tops reaching the nudging layer is more profound than that stated in the paper: it is to avoid interfering with the Entrainment Interfacial Layer (Haman et al. 2007, Kurowski et al. 2009) that is the cloud-free region where the cloud-top dynamics still occurs. If we modify EIL, we modify the cloud layer as well.

Thanks for being specific about the mechanism. We now update the text to elaborate the reason for excluding these cases. Please see L145–L147 in the tracking-changes file.

- the argument for choosing 200m grid spacing needs more clarifications. Matheou and Teixeira (2019) showed that grid convergence for Sc is typically achieved for grid spacings of several meters. The problem of grid convergence from the DNS and LES perspectives, and the benefits of hi-res simulations, was also elaborated in Mellado et al. (2018). For the 200-m grid spacing, a significant fraction of small-scale turbulence is not resolved. SGS transport can compensate for that to some degree, but it is unclear from the description provided in the text how the model does it for such coarse resolutions. Stevens et al. (2005) showed that SGS schemes tend to overestimate entrainment even for the resolutions much finer than the one applied in this paper.

We have incorporated these works into our discussions following the description of the resolution. Interestingly, Mellado et al. (2018) suggests that, if one has to use a coarse resolution, it is better to also use a large aspect ratio. Please see updated text in the paragraph from L123–136 in the tracking-changes file.

References:

Haman, K.E., Malinowski, S.P., Kurowski, M.J., Gerber, H. and Brenguier, J.-L. (2007), Small scale mixing processes at the top of a marine stratocumulus—a case study. Q.J.R. Meteorol. Soc., 133: 213-226.

Matheou, G., and J. Teixeira, 2019: Sensitivity to Physical and Numerical Aspects of Large-Eddy Simulation of Stratocumulus. Mon. Wea. Rev., 147, 2621–2639.

Mellado, J. P., Bretherton, C. S., Stevens, B., & Wyant, M. C. (2018). DNS and LES for simulating stratocumulus: Better together. Journal of Advances in Modeling Earth Systems, 10, 1421–1438.

Kurowski, M. J., P. Malinowski, S. and W. Grabowski, W. (2009), A numerical investigation of entrainment and transport within a stratocumulus-topped boundary layer. Q.J.R. Meteorol. Soc., 135: 77-92.

Other than the changes required to address the reviewers' comments, we now make the following edits.

- Change "amongst" to "among" in L17–18 in the tracking-changes file.
- Define "SGS" to be later used in the discussion of the grid spacing. See L83–84 in the tracking-changes file.
- Adding acknowledgments. See L663–672 in the tracking-changes file.