Response to referee #2

Comments to the Author

This manuscript (1) reports on hole-punch cloud observations that occurred during CLOUDLAB experiments, and (2) conducts an LES sensitivity study to see how the timing and extent of simulated hole punch clouds change with two different model parameters: the sub-grid turbulence parameterization, and the initial LWP of the cloud at the location where seeding is performed.

In all simulation runs, a 100% reduction in LWC is achieved in over 5% of the grid cells (unclear whether this is throughout the entire simulation domain or just at the seeding height), providing a great platform to study the limit of complete LWC depletion. When looking at the largest decrease in LWC with time, the investigators observe that a minimum is reached after a longer period of time in runs with larger initial LWP. The investigators also note that the area of the hole is larger with increasing initial LWP. The authors explain that higher LWP provides ample consumable cloud droplets for the hole to continue growing, compared to low LWP where the available water droplets are consumed, shutting off the continued growth of ice particles. The authors also note that varying the Smagorinsky constant produced no noticeable effect on the cloud microphysics.

The study offers a conceptual picture of the processes governing the hole punch clouds in the simulation. It does not use the observational data in the analysis. The results are thought provoking and will be of interest. Generally, the paper struck me as being very short, and there are many questions that arise as I read through this brief article. I believe that offering more complete details to the reader will fill out the paper, increase readability, and offer a more thorough analysis of the physical concepts under study. Beyond minor clarifications of methodology, there are two important scientific clarifications which I feel must be addressed, which are the first two comments below:

We thank the referee for the constructive and thorough comments and we will address the concerns below.

Relationship between LWC and LWP

The first clarification that should not be overlooked is the relationship between LWP and LWC, and how the differences between LWC and LWP impact the data interpretation. They are used nearly interchangeably in the paper, yet they are not the same, and are not necessarily required to scale with each other. (deep cloud with low LWC can have the same LWP as shallow cloud with high LWC)

According to Table 1, it seems that all simulations are conducted on the same cloud system just selected at different times of day during its evolution. Does the cloud depth change with time in your simulation, or does the LWC change with time? Or is it some combination of both? How would that impact how these results are to be interpreted? For example, if LWC remains constant, but cloud depth is changing, then it could be more appropriate to interpret results as being a function of cloud depth.

Ln 133: Data is grouped and interpreted as "Low background LWC", but simulations are classified by LWP, not LWC. With LWC and cloud depth being the most important variables for LWP, it would be helpful and interesting context for the reader to see initial LWC and cloud depth values for each of these simulation cases. So much information about the cloud is hidden by not presenting these values. It could be included in the table, or as a time-series similar to Figure 3. It could optionally go into supplementary materials if the authors feel it is not critical to the main text.

Thank you for raising this valid point, which we want to address in more detail. It is absolutely true that the cloud depth plays an important role as well for understanding LWP and LWC. It is indeed the case that over time, the selected cloud changes in depth as well as LWC. In a more idealized set-up the proposed approach of keeping LWC constant while cloud depth changes could provide additional insights. In our case, we decided to select cases based on different LWPs with the consequence, that the LWC at seeding level is not comparable to the observed one. The reasoning was that with the model simulations, we have the full 3D field of changes inside the seeding plume and can examine the WBF process across the vertical. We adapted the "Low background LWC" (line 1789) as this was indeed misleading given the classification by LWP. We also added the vertical distribution of LWC over time as an appendix figure and the following in the main text (line 118): "*Figure A1 shows a height-time profile of LWC for all seeding simulations highlighting that over time LWC and the cloud depth change, both of which impact LWP. While it is a limitation to have varying LWCs for each seeding simulation, we conduct the analysis on the entire seeding plume, which disperses vertically. Hence, we evaluate the efficiency of the WBF process across a range of LWCs.*".

Physical interpretation of the sub-grid mixing length scale

   I'll start with the caveat that I am not an expert in LES. This paper review, however, did send me down a rabbithole of sub-grid scale modeling. Please give this reviewer grace upon emerging with the following comments - I apologize in advanced for being long-winded.

   I'll first mention that the rabbit-role journey began because the manuscript does not sufficiently describe the Smagorisky constant, and does not provide sufficient references. This reader feels that in order to motivate why the authors chose to vary the smagorinsky constant, a clearer explanation of its physical significance to the simulation is required. Especially so, since this turbulence parameter is central to the abstract and conclusions.

   The phrasing of Line 46 "more or less mixing", and Ln 88 "Low values indicate more turbulent flows, while higher values yield more mixing", is unclear to me and I can't discern the intended meaning. Two references are provided: Lilly 1962, wherein there is no mention of the "smagorinsky constant/coefficient"; and Dipankar 2015, which only mentions the smagorinksy constant once, without defining it.

   Before going further, I'll mention that Dipankar as well as other references below use C_s (capital C) instead of lowercase c_s, so the authors may consider adjusting their notation to be consistent with the more predominant usage. The lower value of 0.17 is attributed to Lilly 1962, but I was unable to find this minimum value in the paper. During my rabbit hole adventure it seems that most other literature references Lilly 1967 for this 0.16/0.17 value rather than 1962, but I was unfortunately not able to locate a version of the 1967 report to read it myself. It also may be possible that Lilly 1967 used lowercase c_s, but it seems that C_s has become more standard throughout literature.

   Other literature provided me with some introductory understanding of the smagorinsky constant. Very helpful was Mason and Brown 1999. I suggest that this paper (and perhaps others as well) be referenced for the readers benefit, but more importantly the authors may find the discussion in this paper relevant towards interpreting why changing the smagorinsky constant did not impact their results: Mason, P. J., and A. R. Brown, 1999: On Subgrid Models and Filter Operations in Large Eddy Simulations. J. Atmos. Sci., 56, 2101–2114, https://doi-org.cuucar.idm.oclc.org/10.1175/1520-0469(1999)056<2101:OSMAFO>2.0.CO;2.

   My very entry-level understanding upon emerging from the SGS rabbithole is that there exists the filter length scale which is related to the smallest resolved length scale in the LES. Then in the subgrid, there is kinetic energy removed from the system by friction through the eddy-cascade process. The sub-grid model is needed to quantify this energy removal, with the smagorinsky constant playing a role in parameterizing this energy transfer.

   The smagorinsky constant comes across in literature as a loosely-constrained empirical constant, and I have a difficult time relating it directly to a physical property of the simulated cloud. Sometimes it seems like the constant is treated as something that needs to be set appropriately in order to avoid non-physical results and not "break" the simulation. In the range of coefficients explored in this study, it's possible the authors remain within a realm of validity where large impacts on the resolved scale are not to be expected.

   However, to the extent that energy dissipation on the subgrid scale is expected to change the resolved-scale dynamics, I wonder if the investigators can confirm/quantify any changes to the turbulence in the resolved scale due to their modifications of the smagorinsky constant? Did or did not the changes made to the constant affect the resolved-scale turbulence in the cloud? If not, then it would indicate that changing this constant did not truly allow you to compare different cloud turbulence conditions.

   Ln 117-119: By referencing other literature about sub-grid scale turbulence, I think these statements can likely move outside the realm of hypothesis and speculation. Depending on how one interprets the prior literature on this topic, one could go so far as to say that the results seen here are an expected outcome, and that the resolved turbulence is most definitely of higher importance.

   I acknowledge that the authors will have greater expertise in this area than I, and do not wish to speak with absolute authority, but I do encourage the authors to investigate this a bit more thoroughly.

Thank you for your input. We adapted the methods section to have a more detailed discussion on the Smagorisnky constant. We also adapted the correct labeling of the Smagorisnky constant. Line 123: " *Furthermore, we perturb within the 3D turbulence parameterization (Samgorisnky, 1963; Dipankar et al., 2015) the mixing length, expressed through the Smagorinsky constant Cs_s. Low values of C_s indicate more turbulent flows, while higher values have a more laminar flow, i.e., less small-scale eddies. C_s also acts to suppress numerical instabilities by damping the waves through enhanced mixing. In numerical weather simulations often higher values of C_s are used because of coarse grid resolutions (Deardorff, 1970; Mason and Brown, 1999). In the case of ICON in large-eddy mode the default value is 0.23. In contrast, in engineering applications often $C_s = 0.1$ is used. We chose to perform*

additional simulations with a value of 0.17 and 0.3. The lower value is based on Lilly (1962) and Lilly (1967), who theorized it to be a minimum for turbulent flows to represent isotropic homogeneous turbulence. We chose the higher value as an upper limit to C_s. The possible impact of subgrid-scale mixing on clouds was examined for variety of cloud types. Bryan et al., 2003, for example, found that a high C_s suppresses entrainment into deep convective clouds. Takemi and Rotunno, 2003 and Matheou et al., 2011 both identified a delayed/suppressed rain formation upon increasing C_s in cumulus clouds. The enhanced mixing inhibits the effective cloud droplet growth as well as subsequent collision-coalescence processes to form rain. To our knowledge, no study has yet investigated the impact of C_s on liquid-ice-phase interactions. By varying C_s across a widely accepted range of values, we want to investigate if we see a change in the efficiency of the WBF process.". For us, it was surprising to not see any effect in terms of ICNC and LWC, because the mixing of the hydrometeors is crucial for their growth. We emphasized and offered an explanation here (line 154): *"Hence, the subgrid-scale mixing seems not to be a crucial parameter in changing cloud microphysical processes for this particular cloud type. This is in contrast to other studies (Bryan et al., 2003; Takemi and Rotunno, 2003; Matheou et al., 2011), which found a dependence of C_s, but also employed coarser horizontal grid resolutions. We hypothesize that in our case the explicitly resolved turbulence (grid-scale at 65 m) is dominant enough, such that the sub-grid scale effect plays a minor role. A more thorough analysis of horizontal grid spacing is the subject of a future study building on the work in CLOUDLAB."*

> **Questions about the microphysics parameterization** The results of this study are based solely on the simulation, so I think more emphasis can be placed on providing relevant background information about the model, particularly on the microphysics/ice nucleation parameterization. I understand that references are provided, but relevant info can be summarized, similar to the overview which was provided for CLOUDLAB, so that the reader is not required to retrieve and read each reference.
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> Ln 85: Can the authors provide a brief summary of the immersion freezing parameterization and its key dependencies?
> Ln 88: Is ICNC in the simulation given the same >25um requirement as the observed ICNC?
> Ln 107: What size exactly?
> WBF mechanism: I am curious about the ratio of droplets converted to ice via immersion freezing compared to those lost to evaporation via WBF. Or do only the seeding particles experience immersion freezing, such that no pre-existing LWC is lost to immersion freezing?

Thank you, we added more details to the freezing parameterization we use as well as the model's heterogeneous ice nucleation parameterization. In short, the immersion freezing parameterization has a strong temperature dependence (line 94): *"The cloud microphysics scheme was extended (Omanovic, Ferrachat, Fuchs, Henneberger, et al., 2024) by an interface for reading in an external file with the location of the seeding emissions, a prognostic seeding particle tracer, and a freezing parameterization assuming immersion freezing of the seeding particles (Marcolli et al., 2016; Miller et al., 2025). The parameterization is based on fitting a sigmoid curve to laboratory measurements. With decreasing temperature the frozen fraction of seeding particles strongly increases. The total number of nucleated ice crystals is limited by the available cloud droplets. The two-moment microphysics scheme itself also has a heterogeneous ice nucleation parameterization for dust particles (Ullrich et al., 2017). However, for the temperature ($-5.5\,°C$) of interest here, there is no background ice nucleation activity."*. We do not apply a size minimum for the simulated ice crystals because we can clearly identify them using the model's prognostic ice tracer. The argument is less about the specific size but that we need some time that the ice crystal growth becomes effective at reducing LWC. We extended the statement as follows (line 141): *"From Fig. 2a−c it is evident that the reductions in LWC are slightly delayed compared to the nucleation of ice crystals. The ice crystals first need to grow to a certain size to be effective in reducing the LWC as we showed in a previous study, where the ice crystal size was underestimated by almost a factor 2 compared to observations (Omanovic, Ferrachat, Fuchs, Henneberger, et al., 2024)."*. The WBF mechanism specifically encompasses two processes: evaporation of cloud droplets and depositional growth of ice crystals, the connecting factor being the water vapor. At the time of the immersion freezing of the seeding particles, we have a direct transfer from the liquid phase to the ice phase, which does not contribute to the WBF mechanism. Given that the cloud droplet number concentration is higher by 1-2 orders of magnitude, the loss in cloud droplets upon immersion freezing is small.

> **Clarifications of methodology and interpretation** Abstract: "delay in the strongest reductions of liquid water content". Ln 135: "Hence, a delay in the appearance of strong LWC reductions is notable".
> I wonder if this "delay" can be interpreted as a longer duration/lifetime of the hole-punch cloud, as the

Thank you for your input. It is interesting to think about the "delay" effect as a lifetime indicator. However, the minimum point does not point towards a "refilling" of the hole, as the relative changes in LWC remain at -100%. Hence no refilling can be noted. We have to be aware that we identify the seeding plume based on ICNC, which can sediment. If they start to leave the cloud as fallstreaks, as in traditional hole-punch clouds, the end of life of the cloud is initiated. Thus, we could have a masking effect of sedimentation. In our opinion, the extent of the hole-punch cloud should be independent of the ice nucleation parameterization. The seeding particles initiate the ice phase, but after that we only investigate the interactions between ice crystals and cloud droplets. This also is valid for traditional hole-punch clouds, which form upon the penetration of an aircraft through a cloud layer and the associated pressure and temperature drop. Here, no aerosols are involved, but as soon as the ice phase is formed, the WBF process can take place. We also want to note that we do not follow the freezing parameterization from Miller et al., 2025 because we did not provide a parameterization in there but we follow the one from Marcolli et al., 2016, who conducted ice nucleation experiments with seeding particles in the laboratory. For all seeding simulations, we have the same emission pattern, hence all seven grid boxes experience the same emission rate (for 6 min) and overall the same nucleation rate. Moreover, our model applies a saturation adjustment before and after calling the cloud microphysical processes, hence no water supersaturation is possible. The area of the hole depends on the wind speed as well as the horizontal mixing length, which acts in all directions, but is dominant for the cross-wind expansion. We do not see a difference in ice nucleation activity given that the temperature across all simulations is comparable. The immersion freezing is limited by the available cloud droplet numbers and not LWC. We believe there might be a misunderstanding of how these particles are represented in the model. We inject seeding particles and based on the frozen fraction (limited by cloud droplets), we form ice crystals. We do not inject solution droplets that may hygroscopically grow and freeze. This is something our model cannot represent. We also do not allow for any aerosol recycling, i.e., aerosols from melted/sublimated ice crystals are lost. This assumption is justified because during our timescales of 10 min to 30 min, the effect of recycled aerosols should be negligible. We greatly appreciate this comment and extended following to clarify our seeding and freezing procedure (line 103): "*The emitted seeding particles freeze upon meeting the right conditions with respect to temperature and availability of cloud droplets. Hence, we have a direct transfer from seeding particles to the ice phase based on the computed frozen fraction following the immersion freezing parameterization. We do not include a budgeting of the seeding particles, i.e., aerosols from melted/sublimated ice crystals are lost and are not available for subsequent ice nucleation.*".

For the entire analysis we only focus on the seeding plume defined by the masked ICNC ($>0.001\,\mathrm{cm}^{-3}$). We added a statement both to captions of Figure 2 and 3 to clarify this. We also adapted the contour labels.

Yes, the cloud optical thickness is calculated across the vertical column, where the liquid cloud is present. We corrected the labeling of r_e to r because it is the spherical radius of cloud droplets calculated with the prognostic mass and number tracers in the model.

The area is calculated based on the grid points which show these strong reductions in LWC and taking their cell area (line 170): "*We also computed the maximum hole area at these minima by calculating the cell area of the associated grid points. We find a similar pattern:*". The wind speed and direction remain fairly comparable given that these clouds are forced by steady north-easterly winds.

Yes, indeed, this was a mistake in our formulation. We double-checked this and adapted the sentence to be clearer. See also response to referee #1, comment to L127-138.

Thank you for this suggestion. We added the following to make it clearer (line 165): "*The 5th percentile represents the lower tail of the distribution of LWC, i.e., the 5 % of grid points which experience the strongest changes.*".

The reason for showing the 95th percentile of ICNC is that the strongest changes in LWC are associated with grid points having large ICNC. We refrain from showing the total ICNC because it is then difficult to put it into

context with the changes in LWC. It is true that the ICNC is a combination of ice nucleation, aggregation, complete melting and sublimation, and sedimentation. It would be interesting to compute the individual process rates to have a better understanding of the processes governing the seeding plume, which could be done in a future study.

> Figure 3(b): Once the relative reduction in LWC reaches -100%, it seems to remain fixed there, but in panel (a) the absolute reduction in LWC starts to go back up. For example, looking at L60, the absolute LWC reduction in panel (a) increases to zero at 19 minutes (implying zero reduction relative to the initial LWC), but the relative LWC reduction remains flat at -100%. Can the authors explain how this happens?

If the background LWC is small, i.e., close to 0, the absolute change may be small while in relative terms it is still a -100% change. This pattern underlies our explanation that the seeding plume encounters differently "cloudy" environments.

> Ln 138-139. The results on cloud optical thickness: Is this a true statement? In some cases it appears that LWC reductions start going back up to zero in panel (a), while the relative changes in optical thickness don't change accordingly.

This is possible as explained in the above answer. The absolute changes may be very small, as for many simulations after 20 min but in relative terms, these changes are large.

> I am curious about the response of the cloud after seeding ceases. What changes in the cloud evolution are expected once seeding ceases after 6 minutes? In the figures, no noticeable changes happen at the 6-minute mark, does this align with what the authors expect?

We start tracking the seeding plume with the start of the seeding and this means we have a continuous growth of the plume while seeding particles are being emitted. However, once the emissions are stopped, only the evolution of the existing ice crystals defines the seeding plume. These ice crystals can grow through vapor deposition, aggregation, and then start to sediment. As soon as they hit the ground, they are removed from the ice tracer in the model, hence the constant reduction in ICNC over time (next to the dilution effect upon mixing). Moreover, once the seeding perturbation is concluded, the cloud is again in an unperturbed state, i.e., no ice crystals and similar conditions as before the seeding started.

> **Questions about the observations**
> I understand that the purpose of this paper is not to analyze the observational data, nor to perform a direct model-obs comparison. However, I do have a few general questions about the observations which could be clarified, as they are relevant to the study of LWP dependence: For the observations, I'm curious if the LWP corresponds to a remote sensing measurement or is it derived from the in-situ LWC? Ln 70-72: How are the estimates of observed LWP and LWC obtained?
> The magnitude of ICNC seems to be comparable between the observation and the simulated L25 case shown in Figure 2. However, the observed cloud shows decreases in LWC on the order of $0.1\,\mathrm{g\,m^{-3}}$, while the simulation, despite starting with initial LWP of $25\,\mathrm{g\,m^{-2}}$ or higher, never exceeds more than $0.05\,\mathrm{g\,m^{-3}}$ of LWC reduction. Can the authors comment on this? The simulated L25 cloud has comparable LWP to the observed case, but does it have smaller LWC? How, then, does the cloud depth compare between the simulation and observed case?

We added the instruments to the reported LWC and LWP (line 76): "*The interplay between the ice and liquid phase is further demonstrated in Fig. 1c, which shows the by HOLIMO measured liquid water content (LWC, $\mathrm{g\,m^{-3}}$) and ice crystal number concentrations (ICNC, $\mathrm{cm^{-3}}$) for one of the three hole-punch cloud seeding experiment. The liquid water path (LWP) of the background state was $\approx 25\,\mathrm{g\,m^{-2}}$ (based on microwave radiometer observations).*". It is true, that the cloud depth differs between the observations and simulations. It is a challenge to exactly reproduce these conditions in model simulations, hence we decided to achieve comparable LWP, given that the seeding plume spreads out vertically, and report relative changes in LWC. The simulated cloud depth is roughly twice as high as the observed one, explaining the difference in LWC while LWPs agree well. We added the following statement to account for this limitation (line 161): "*While we selected the seeding simulations based on the background LWP ($\approx 25\,\mathrm{g\,m^{-3}}$), we did not achieve an agreement between observed and simulated LWCs. In this case, the simulated clouds have a higher vertical extent, which may impact the emergence of a hole-punch cloud.*".

> Ln 53: "This type of clouds" → "This type of cloud"

Thank you, it is adapted.

> Ln 64-65: Did all three happen on the same day?

Yes, they did. We added the following (line 72): "*All three seeding experiments were conducted on the same day (12 January) in the morning hours (9:00 UTC − 10:30 UTC).*".

> This is a very nit-picky language thing, but the use of negative values with the term "reduction" is not exactly precise, though the reader can still understand what you mean. The word "reduction" has a negative sign implied within it, thus adding another negative sign negates it, resulting in a positive. For example, "-60% addition" = "60% reduction", and likewise "-60% reduction" = "-60% addition". Some examples where this comes up (non-exhaustive):
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> 1. Ln 111-112: "minimum of mean reduction" → The words being said are "minimum reduction", however you're actually referring to the point of maximum reduction in LWC. The trouble arises from the fact that your "reduction" values are negative.
>
> 2. Ln 134: "minima of LWC reductions"
>
> 3. Ln 155: "-60% reduction"
>
> Possible solutions: Instead of labeling the figures as "LWC reduction", the authors can use the label "LWC change". The term "change" does not have an implied sign as the term "reduction" does. Then, the authors are free to say "60% reduction" and "maximum of LWC reductions" in the text, which makes more sense when reading.
> The authors could also simply leave it as it is, just knowing that a bit of confusion can occur. It's not difficult for the reader to understand from context what the authors mean to say. However, without looking at the figure axes, the phrase "minimum reductions" in the text could definitely be misunderstood, and it would be challenging for others to quote this out of context.

Thank you for proposing your solutions. Referee #1 also raised this issue and we adapted everywhere necessary reduction to change. We agree this way it is clearer to the reader and avoids the confusion of minimum and maximum reduction. We do not provide here the adapted lines but refer to the diff document.

> Ln 127: Capitalize after the colon.

Thank you, it is adapted.

> Ln 128: I suggest "there are two processes" instead of "we have two processes"

Thank you, it is adapted.

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