Response to Referees

Glaciogenic seeding-induced hole-punch clouds and their sensitivity to the clouds' background state

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Dear editor and referees,

We would like to thank the editor for handling our manuscript and the referees for their careful evaluation of the revised manuscript. Below we address our detailed responses to all the comments. In this response-to-review document we try to clarify and address each of the suggestions, comments, and questions made during the review process. Therefore we have copied the comments in lavender boxes and have addressed them one by one. In the response we use italic fonts to quote text from the revised manuscript. Additional to the revised manuscript, we have uploaded a version of the manuscript with highlighted tracked changes.

Best regards, Nadja Omanovic et al.

Response to referee #1

Comments to the Author

> The manuscript by Omanovic et al. explores hole-punch clouds measured during the CLOUDLAB field campaign. Simulations are performed with the NWP model ICON in a large-eddy simulation setup with varying liquid water path and Smagorinsky constants (c_s) to explore the sensitivity of the clouds to these variables. While large impacts are found from a varied LWP, the impact of a varied c_s is negligible.
>
> This manuscript is nicely written and follows a logical structure. Some parts could benefit from some more clarifications to make the manuscript more approachable for non-experts, please see Specific Comments below. The study is a short but interesting one, providing thus far, unexplored topics, and deserves publication after some revisions. I have no major concerns. Some discussion points are provided below

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

General comments

> **GC1** There is no mention of cloud droplet size distribution. Has this been explored if it has any impact on the rate of glaciation?

Thank you for this input. This is indeed interesting and we have not yet explored it. We chose to achieve a similar cloud droplet number concentration as observed ($\approx 500\,\mathrm{cm}^{-3}$). Quantifying the effect of cloud droplet size distribution could be challenging given the inherent limitations of a two-moment microphysics scheme as well as the saturation adjustment governing the growth and evaporation of cloud droplets, which treats all cloud droplets equally. We nonetheless believe that this investigation could indeed be insightful and will pursue this in the follow-up project of CLOUDLAB.

> **GC2** In regard to L99-L101, have the authors tried to vary the Smagorinsky constant by, say a factor of 10? The values chosen span a small range and it is surprising to see no impacts from varying this constant.

The chosen values for C_s are based on theoretical derivations of C_s and commonly applied to the sugrid-scale closures. C_s cannot be larger than one. Therefore, we did not perform tests with factor 10. We did test the effect of setting it to zero. However, then our simulation turned unstable because no damping was applied to the numerical diffusion. Please see for more details the response to referee #2, their second comment "Physical interpretation of the sub-grid mixing length scale".

> **GC3** In L119 the authors state “We hypothesize that the explicitly resolved turbulence (grid-scale) is of higher importance.” To this effect, have the authors explored other horizontal grid spacings? It would be interesting to see how large impact this has on the rate of glaciation.

In previous studies we have increased the horizontal resolution from 130 m (Omanovic, Ferrachat, Fuchs, Henneberger, et al., 2024) to 65 m (Omanovic, Ferrachat, Fuchs, F. Ramelli, et al., 2025). However, we did not conduct a systematic analysis of the changes. We are currently working on a study with a focus on this. We added the following to the results (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.*"

Specific Comments

> **L22** “It takes place because of different saturation water vapor pressures over water and ice” While not wrong I would like a longer explanation to make it more accessible to non-experts. Also include the standard statements on the constraint on vapour pressure ($e_{liq} > e > e_{ice}$).

Thank you, we extended now the description by the following (line 22): "*It takes place because of different saturation water vapor pressures over water* ($e_{s,w}$) *and ice* ($e_{s,i}$)*. At water-subsaturated conditions cloud droplets will evaporate, while ice crystals continue to grow as long as they experience ice supersaturated conditions* ($e_{s,i} < e < e_{s,w}$*, where* e *is the ambient water vapor pressure). Moreover, the evaporating cloud droplets serve as a reservoir of water vapor, which can be taken up by the ice crystals. In the case of* $e > e_{s,w}$*, both hydrometeors grow while in case of* $e < e_{s,i}$*, cloud droplet evaporate and ice crystals sublimate. During the WBF process, ice crystals grow large enough to start to sediment and form fallstreaks below the cloud.*"

> **L53** How is the ‘void of ice crystals’ requirement upstream from the field site ascertained in the experiments?

We added the following explanation in line 55: "*They are often void of any ice crystals because of low INP concentrations as well as cloud top temperatures being* $< -10\,^{\circ}\mathrm{C}$*. Hence, they are the ideal candidate for cloud seeding.*" as well as in line 79: "*It is clearly notable that the reductions in LWC are co-located with the presence of ice crystals. Furthermore, ice crystals were measured only during the seeding signal, further highlighting the absence of any naturally formed ice crystals in the cloud. After the passage of the seeding plume, the cloud reverts to its background state ...*"

> **L61-62** This sentence is a bit unclear to me. Is the size threshold of 25 mum the limit of detection?

Thank you, we adapted the following (line 64): “*All particles with diameters smaller than* $25\,\mu\mathrm{m}$ *are classified as cloud droplets. For particles larger than* $25\,\mu\mathrm{m}$ *we distinguish between cloud droplets and ice crystals.*". The reason is that we need a certain size to distinguish fully round (cloud droplets) from non-round (ice crystals) particles.

> **L63** Please add the reasoning behind the change in analyzing frequency for seeding and non-seeding experiments

We added following clarification (line 66): “*The difference in analysis frequency is due to the rather low ICNC* ($<0.5\,\mathrm{cm}^{-3}$) *as well as the measurement time of the seeding signal* ($\approx 6\,\mathrm{min}$) *compared to the one of the background. The goal was to achieve a robust data population during the passage of the seeding plume, while during non-seeding conditions, we can average over a longer time period, since the cloud is expected to remain stable and no significant changes are anticipated on that timescale.*"

> **Fig. 1 (d)** Does the y-label indicate that the ceilometer is placed just below 20m? If so, this could perhaps be mentioned in the field site setup to make it clearer.

We added in the caption of Figure 1 the following to make it clearer: "*The vertical axis indicates the height above the ceilometer, which can be interpreted as the height above ground.*"

> **L73** Perhaps I'm not aware of the correct terminology but "the attenuated backscatter signal moves higher up" sounds a bit awkward?

We reformulated that part to make it clearer (line 82): "*Moreover, a ceilometer, located at the main field site, also captured the change in cloud base. Before and after the seeding the maximum of attenuated backscatter is close to the ceilometer (see Fig. 1d). During seeding, the attenuated backscatter is decreasing in the levels close to the ceilometer, while we see an increase in the backscatter at heights between* 50 m *to* 70 m *above the ceilometer.*"

> **L78** The "we already demonstrated' sounds like it has been done in this manuscript. Perhaps "In previous studies" or similar would be more appropriate.

Thank you, we adapted it as follows (line 89): "*In previous studies we demonstrated ...*".

> **L89-95** From a modelling perspective the method makes a lot of sense, but do the authors have an indication whether a hole-punch cloud did occur in the observations during the seeding during this day? And if not, is there an explanation to why that does not occur?

The observations of hole-punch clouds only occurred on 12 January 2024, because the cloud layer was shallow and the perturbation from the cloud seeding was enough to dissipate the cloud. For other experiment days, we did not observe complete dissipation of the cloud across its vertical extent. We added this as follows (line 108): "*To evaluate the impact of the cloud's LWP on the emergence, size, and lifetime of the hole-punch cloud, we chose a model simulation day, 24 January 2023, that was one of the field experiment days within CLOUDLAB. During this day, we conducted 9 seeding experiments in the field, none of which showed any signs of hole-punch clouds. This is mostly because the cloud was much thicker (extent of up to* 500 m*, see Figure 2a in Fabiola Ramelli et al. (2024)) preventing its complete dissipation after the given observation time. In the model simulations for this day, we have varying LWPs while temperatures agree with the one from the field experiments of a hole-punch cloud (*$-5.5 \pm 0.2\,^{\circ}\mathrm{C}$*).*".

> **Fig 2 d-f** Should 'minimum' not rather be 'maximum' as it refers to the largest reduction in LWC? Furthermore, at heights above the seeding level the reductions are very abruptly cut off, why is this? Should it not return to 0 rather than no values? Is it constrained by plotting or physics? In 2 d-f minor xticks could be added to improve readability.

In accordance with referee # 2, we adapted the term "reduction" to "change" throughout the manuscript to avoid this kind of confusion. Only in places, where its meaning is unambiguous we kept "reduction". The LWC is abruptly cut off because we identify the seeding plume by masking for grid points where ICNC $> 0.001\,\mathrm{cm}^{-3}$. This can lead to this flat top because above the cloud top there is a strong inversion with low humidity, leading to the sublimation and melting of the ice crystals. We added a clarification in the figure caption: "*The abrupt cutoff at height* 1300 m *is caused by the masking of the seeding plume with ICNC* $> 0.001\,\mathrm{cm}^{-3}$ *(see Sect. 2).*"

> **L118** Perhaps this insensitivity is due to the characteristics of the clouds? This statement seems a bit too generic for this reason, could the authors add "for these clouds" or similar?

Thank you, we added the following (line 155): "*...for this particular cloud type*".

> **L129-130** 1) and 2) could be added to better distinguish the processes.

Thank you, that was added (line 172 and 173).

> **L132** This would also require a quite homogeneous temperature at seeding height as this is done at quite warm temperatures if I understand the setup correctly. Have the authors looked into the activation rate of the seeding particles in terms of spatial heterogeneity? And how this may impact the obtained ICNC.

For finding the best matching seeding level in the model setup, we averaged the temperature along the drone flight legs with only slight variations across the grid points (<0.1 °C). The reported temperature by the field experiments is calculated in a similar fashion, i.e., averaging the temperature along the drone seeding legs during the seeding. While there is definitely a strong dependence on temperature for the ice nucleation of the seeding particles, within the small range of temperature, the nucleation rates are rather similar. In the specific case discussed in this manuscript, we found a frozen fraction of 0.025 ± 0.002. Moreover, the horizontal temperature distribution is rather homogeneous given that we have stable conditions with steady north-easterly winds. We extended our hypothesis regarding the larger area of seeding (line 175): "*To achieve faster and more wide-spread reductions, a stronger seeding perturbation across a larger area (i.e., longer seeding legs with higher seeding concentrations and/or during a longer seeding time period to achieve higher ICNC) would be required.*"

L137-138 As the authors have model output that spans the region of the seeding plume to the field site, could this not be confirmed by evaluating the upstream path?

Thank you, this is true and we changed the statement to be clearer on that. We rechecked the conditions and it is correct, that the cloud layer changes in space and time, thus the relative changes in LWC can be explained (line 180): "*It also notable that for seven out of nine seeding simulations a second local minimum is achieved (between* 20 *and* 25 min*), which is also associated with* −100 % *LWC reductions. As the seeding plume is advected in space, it encounters background states with first lower and then again higher LWCs, such that a renewed reduction in LWC is possible at later times.*"

Conclusions I am missing some references and comparisons to other work. As this topic is quite unique I can see that it's hard to find good references, but some references and discussion to whether the reduction in LWP has been previously been seen to this effect should ideally be included here. One example could be: "Large-Eddy Simulations of the Impact of Ground-Based Glaciogenic Seeding on Shallow Orographic Convection: A Case Study" Chu et al. 2017. Also a discussion on the varied Smagorinsky constant in comparison to other LES papers would be appropriate, while other papers may not discuss glaciation, a discussion on whether changes in the clouds are seen could be done.

Thank you, we extended the discussion on the Smagorinsky constant. For more detail, please see the response to referee #2, their second comment "Physical interpretation of the sub-grid mixing length scale". Thank you for the reference to Chu et al., 2017. We extended our conclusion part by the following (line 200): " *This raises the question if the explicitly resolved turbulence is dominant enough, such that no additional subgrid-scale turbulence is required. Xue et al., 2016 and Chu, Xue, et al., 2014; Chu, Geerts, et al., 2017 both found a strong dependence of simulating glaciogenic cloud seeding on horizontal resolution. With increasing resolution, the efficiency through the WBF process to form precipitation increased. Hence, if one wants to successfully simulate glaciogenic seeding at coarser horizontal resolutions, the subgrid-scale turbulence may be of higher importance.*"

Technical Corrections

L1 "is usually" instead of "usually is"

Thank you, it is adapted.

L74 add "the" for "the cloud top"

Thank you, it is adapted.

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.
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> 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.

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 (*$-5.5\,^{\circ}\mathrm{C}$*) 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 "minimum" corresponds to a transition point when the LWC is no longer decreasing and starts to fill-in with liquid water again (i.e. the closing of the hole)? Ln 130-131: "As soon as maximum reductions of $-100\,\%$ LWC are achieved, no more liquid water is present to be consumed, such that the extent of the hole is limited." This statement lacks nuance. Can the authors expand more on why this occurs, and frame it within the context of their ice nucleation parameterization? It's not immediately clear to me why this would limit the extent of the hole. If seeded particles are dispersed and advected across an identical area in all cases, then why does achieving $-100\,\%$ LWC in one region of the cloud stop ice from growing in other regions where seeding particles are present? Based on the immersion freezing parameterization detailed in Miller et al 2025, I am curious about the interplay between the ambient saturation ratio, the size evolution of the seeded solution droplets and their water activity, and their subsequent ability to nucleate into ice crystals. Is it possible that ice nucleation itself is limited to a smaller lat/lon area in the lower LWP cases? Is the area of the hole limited in the along-wind direction? Or perpendicular to the wind (cross-wind) direction?

One interpretation is that, since seeding is ongoing for 6 minutes, and is dispersed into an existing cloud, then every "wave" of seeding particles is met with an initial LWC from which to produce a concentration of ice crystals and a "hole" that is advected downstream. In this sense, it seems that the along-wind length of the hole will be the same in all cases (would depend primarily on the wind speed). In terms of the cross-wind width of the hole, this seems like it will depend on the horizontal mixing timescale relative to how long the seeded particles are "active" in terms of their ice nucleating abilities. Perhaps in low LWC conditions, by the time the seeded solution particles mix horizontally, they have already evaporated to a point where they can no longer nucleate new ice. Maybe in the higher LWC conditions, seeded particles can remain efficient at ice nucleation for longer, and this longer timescale relative to the horizontal mixing timescale allows ice particles to nucleate along a greater cross-wind width of cloud, before advecting downwind. Anyways, a lot is left open to speculation on the part of the reader, so it would be great if the authors can detail some of these nuances in their analysis.

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.*".

> Figure 2(a-c):
>
> 1. In the caption, mention that a mask (ICNC $>0.001\,\mathrm{cm}^{-3}$) is used.
> 2. The contour labels are not legible.
>
> Figures 2(g-i) and Figure 3:
>
> 1. At each timestep, are values of LWC in all grid cells of the simulation used to compute the 5th percentile? Or is it only calculated using the grid cells at the seeding level height? Or are LWC values calculated only for the masked (ICNC $>0.001\,\mathrm{cm}^{-3}$) grid cells?

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.

> Ln 123: For the optical thickness, at each timestep, am I correct in understanding that the optical thickness is calculated across the full vertical column of the simulation? Is r_e the effective cloud droplet radius?

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.

> Ln 127: How is the hole "area" calculated? Would the lat/lon extent of the hole be influenced by the wind speed and direction? Are the windspeed and direction identical for all simulated cases?

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.

> Ln 136-139: Can your hypothesis not be confirmed by looking at the simulation? Did indeed the plume encounter first lower and then higher LWC?

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.

> The use of percentiles can be a little tricky to interpret. The authors could improve readability by stating a physical interpretation of the percentiles: "5 % of the grid cells had LWC reductions greater than the value shown", "In Figure 3(b), once the value reaches −100 %, it indicates that at over 5 % of the grid cells had a complete depletion of LWC."

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

> Figure 2(g) 95th percentile ICNC → At first glance it seems like ice crystal number concentrations are constantly decreasing, despite active seeding occurring... but the interpretation of this 95th percentile metric requires some thought. "Of the grid cells containing ice, 95 % of them have ice concentrations larger than this value": If the value is decreasing, it means that more grid cells with lower concentrations are occurring. This can be seen in the pink contours, where the dark 0.2 line at first encompasses a high ratio of the masked cells, later on the 0.2 contour area shrinks and the others grow, such that 0.2 only is a small ratio of the masked cells. This could indicate dispersal of ICNC... where there are fewer regions of extremely high concentrations. This is overall tricky to interpret without a fair amount of pondering on the part of the reader. Did the authors have a specific intent for using the 95th percentile here?
>
> Rather than 95th percentile, the total ICNC could be nice to see instead. If the total ICNC increases with time it means continued nucleation/production of ice, whereas if it decreases it would indicate precipitation or aggregation.

The reason for showing the 95^{th} 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):

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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