# Reply to Anonymous Referee #1

Referee comment on "Modeling and verifying ice supersaturated regions in the ARPEGE model for persistent contrail forecast" by S. Arriolabengoa et al. (egusphere-2025-1499, https://egusphere.copernicus.org/#RC1, 2025).

We thank the Referee for the constructive feedback provided on our study. The insightful comments have helped us to improve the quality of the manuscript and have also suggested interesting directions for future research. The answers to the various remarks are given as follows. For better legibility, the Referee's comments are highlighted with a gray background and changes in the manuscript are in italic.

## General comments

Overall I find the manuscript to be acceptable, although a few minor suggested revisions are mentioned below. It is clearly written and well organized with supporting evidence and logic and easy-to-follow outcomes. The main criticism (further discussed below) pertains to the persistent problem in some models of achieving the right outcome for the right reason.

## Specific points

1. The discussion in Sections 2.2 and 2.3 could be made part of the appendix directly instead of including in the main body. The cloud scheme is already discussed in detail in the appendix so isn't it simple to keep all those details in one place? The fact that multiple closure methods were attempted could be omitted and only the one picked could be described. The rejected method seems impertinent to readers. During the research, the authors discovered a closure idea that was inferior but that happens frequently in model parameterization development. Which dead-end pathways to describe to readers is subjective, but it doesn't seem to add any insight directly to a physical problem being solved. As one manuscript reviewer's opinion only, I would not require this to be addressed in a revision, so the editor can decide if there is mutual agreement among reviewers.

Section 2.2 and Section 2.3 present the re-working of the cloud schemes concepts used in ARPEGE (Sommeria and Deardorff, 1977; Smith, 1990) when supersaturation is allowed. We included these sections in the main body because:
1- the paper is primarily focused on adapting the current ARPEGE model,
2- we see opportunities to apply the proposed methodology to other cloud schemes with similar statistical concepts,
so we would prefer that the structure of section 2 remain unchanged within the main body. In order to achieve a more straightforward flow in the main body, we suggest to simplify L144-153 to go directly to the cloud fraction and condensate mean content expressions, in sections 2.2 and leave more detailed comments in the appendix:

*The mean cloud fraction $C^+$ and mean cloud condensate content $\overline{q_c^+}$ after adjustment can be expressed in relation to the centered and reduced probability distribution $G_{[0,1]}(t)$ (see calculations in Appendix), such as*

$$C^+ = \int_{-Q_{c,k}/\sigma_{s,k}}^{+\infty} G_{[0,1]}(t)\, dt,$$

$$\overline{q_c^+} = \sigma_{s,1} \int_{-Q_{c,k}/\sigma_{s,k}}^{+\infty} G_{[0,1]}(t) \left( t + \frac{Q_{c,1}}{\sigma_{s,1}} \right) dt.$$

About the remark on closures, we believe that including different closure methods could be useful when applying the proposed methodology for including supersaturation in the cloud scheme to a different atmospheric model. In that case, the calibration process will not yield the same results and the chosen closure may differ. However, it is true that this frequently happens when developing a model parametrization, so if the editor feels that omitting this part from the main body would make the text easier to read, we will modify it during the revision process.

2. While I agree that the scale of model data versus observations is extremely different, I believe it is insightful to see a distribution of the fundamental raw model data error. A good example is found in Fig. 5 of Thompson et al (2024). The frequency histograms of RHice in this manuscript's Fig. 6 provides a good indication of the changes in ARP-new vs. IFS and Obs, but a distribution plot of direct model error for every single IAGOS unfiltered observation is desired as well.

Figure 6 in Section 5.1 has been enhanced by adding a panel with the distribution plots required by the referee (Figure 6c) and also IAGOS unfiltered frequency histogram (Figure 6a) as required by referee 2. The legend has been modified accordingly.

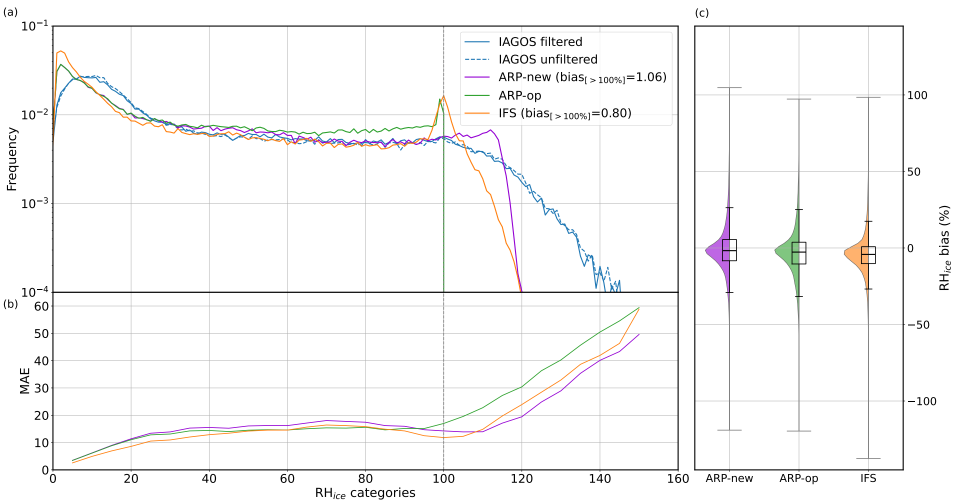


*Figure 6: (a) Frequency histogram of $RH_{ice}$ (1 % bins) with the associated frequency bias on $RH_{ice}$ > 100 % and (b) Mean Absolute Error (MAE) computed for different categories of observed humid-*

*ity (5 % bins). Results are shown for IAGOS observational dataset (blue), filtered and unfiltered, ARP-new (purple), ARP-op (green) and IFS (orange). (c) Distribution plot of model bias in $RH_{ice}$, computed against unfiltered IAGOS observations, for the three ARP-new, ARP-op and IFS models. Verification dataset from the 1st July 2022 to the 30th June 2023 within the aerial boundary of 80° W–40° E and 30–75° N, covering North Atlantic and Europe.*

Further comments have also been added to discuss the new figure:
L259: *Model results are compared with filtered and unfiltered IAGOS observations, the latter being used to compute the distribution plots of direct model errors (Fig. 6c).*
L267: *We note that there is no significant difference between filtered and unfiltered observation histograms, showing that applying a 100s mean-filter does not alter the properties represented in the original IAGOS dataset.*
L279: *In the analysis of the direct model error, Fig. 6c shows that ARP-new has a median bias centered on 0, slightly better than the median bias of ARP-op and IFS. However, IFS shows a narrower interquartile range (11.06%), compared to (13.87%) for ARP-new. This indicates that, after bias correction, IFS has a slightly better overall accuracy in representing humidity.*

3. Why are various models still not using a better physical representation of ice depositional growth from a physical means rather than using variants of saturation adjustment? Efforts to create and use tuning knobs to handle ice supersaturation rather than updating inherent physical growth equations seems endless. Eq. 1 is just another tuning knob component of three elements described in this paper: (1) a calibration coefficient; (2) a simplistic temperature function; and (3) a closure method that doesn't properly represent the physics as shown clearly in Fig. 5. The "cliff" in the histogram is related to Eq. 1 and the sentence in Line 112: "Once the supersaturation threshold is locally exceeded, local adjustment is instantly obtained back to saturation." In other words, as humidity grows progressively larger, it will cross the threshold and then suddenly the RHice is instantly dropped (let's say for example 145%) back to 100% while adding the excess vapor directly into solid phase.

There is no need to invoke a need for 2-moment cloud ice treatment to result in proper RHice forecasts. This appears to be a common misconception. A mass mixing ratio single moment scheme suffices with additional assumptions of ice spectral distribution. A basic inverse exponential distribution with a Y-intercept parameter that can increase as ice mass increases while holding a slope constant is one such assumption. This follows the most basic observations that more ice number comes with more ice mass. From whatever assumptions are made for number distribution, the total ice number (or number within bins of specific size ranges) can be diagnostically calculated, which effectively turns a 1-moment scheme into a 2-moment treatment. There is no solid evidence to say that 1-moment schemes are incapable of predicting the correct outcome compared to 2-moment schemes.

The essential problem of the microphysics is the lack of accounting for slow physical vapor depositional growth of ice. Creating a new threshold for when to convert instantly the excess vapor over ice saturation into cloud ice isn't solving the problem yet (as Fig. 5 clearly shows). In fact, the method to create initial ice where none previously existed could be fine with the new technique, but once ice does exist in a grid volume, do not permit more ice to nucleate and use a "electrical capacitance" analogy to grow the existing ice by vapor deposition. That way some of the excess (over saturation) water vapor can remain in gas phase and continue to permit $RH_i > 100\%$.

We thank the referee for sharing these comments and suggestions of improvement.
Our main objective was to improve the ISSR forecast ability of the operational global model ARPEGE. Modifying the microphysics parameterization of ARPEGE would have had large impacts on both forecast and assimilation systems. Our approach provides positive outputs that are much easier to propose for short-term operational use.

In the future, we plan to upgrade ARPEGE by using the physical parameterizations from our regional model AROME. Among these, the microphysics scheme would be ICE3 or LIMA. Explicit deposition on ice crystals is already available in LIMA (both with the 1-moment or 2-moment description of ice), and could be in ICE3 as well. Thus, it was not necessary at this time to work on the microphysics scheme of ARPEGE, but these ice-growing processes will be investigated and improved.

To be more specific about how to improve the system, we propose to rephrase the second paragraph from L442 in Sect. 6.1:
*For example, in the modified scheme, the adjustment in the cloud to ice saturation is assumed to be instantaneous, which is probably a major limitation of the physical description of ice representation in our context and an explanation of the cut-off on the highest supersaturation observed in the $RH_{ice}$ distribution histogram (Fig. 6a). Indeed, it has been shown that $RH_{ice}$ decreases with a relaxation time that can exceed several time steps, to finally reach a few percent above saturation, thus allowing local in-cloud supersaturation w.r.t ice. Taking into account pre-existing ice and a better description of the physics of the vapor deposition should then improve the representation of the highest values of ISSR. Sperber and Gierens (2023) proposed [...]*

With regard to the mention of 2-moment scheme, we suggest to modify L455-459 to add more precise comments:
*Regarding the use of microphysical 2-moment schemes, we acknowledge that cloud formation processes can be better represented in NWP models involving more detailed physical processes, (Vié et al., 2016; Thompson et al., 2024; Seifert, 2024). For GCM climate models where contrail clouds are parametrized (e.g. Bock and Burkhardt, 2016; Chen et al., 2012), this type of scheme has proven useful for estimating the physical and optical properties of contrails, such as optical length (see e.g. Zhang et al., 2024). However, there is currently no consensus that obtaining a satisfactory repre-*

*sentation of $RH_{ice}$ in the UTLS necessarily requires the introduction of a second moment into the microphysical scheme, and the balance between computational costs and benefits in terms of improved forecasts must be carefully evaluated before implementing such a complex scheme in an operational NWP system.*

## Technical corrections

I did not exhaustively list many technical corrections because the manuscript was relatively good overall and I am late submitting the review so I am optimistic that other reviewers made more suggestions. Here are just a couple items.

L59: "verification methods deserve to be completed to accurately..." is awkward. It is simpler to state that verifying RHice in general is needed as well as threshold-based (ISSR) conditions?

The phrase has been rephrased as follows: *Verification of $RH_{ice}$ and threshold-based conditions is needed to accurately describe NWP model capabilities.*

L60: "known to be a rather rare phenomenon in the atmosphere." It is not rare. It occurs 11% of the time in the entire atmosphere if you believe radiosonde data per Thompson et al (2024) or the manuscript's quote of 10% of the time from the IAGOS dataset. That does not seem especially rare. The phrase is basically repeated in L246.

It is true that, statistically, this is not a rare phenomenon and the wording could be misleading. We have changed the word and rephrased the two sentences as follows:
*... known to be an occasional phenomenon in the atmosphere ... / ISSR occurrences are acknowledged to be an occasional phenomena in the atmosphere ...*

# Bibliography


Lisa Bock and Ulrike Burkhardt. The temporal evolution of a long-lived contrail cirrus cluster: Simulations with a global climate model. *Journal of Geophysical Research: Atmospheres*, 121(7): 3548–3565, 2016.

Chih-Chieh Chen, Andrew Gettelman, Cheryl Craig, Patrick Minnis, and David P Duda. Global contrail coverage simulated by cam5 with the inventory of 2006 global aircraft emissions. *Journal of Advances in Modeling Earth Systems*, 4(2), 2012.

A. Seifert. Two-moment cloud ice scheme for predicting ice supersaturation in the global icon. Presented at the Contrail Analysis Workshop – Google Research, London UK, 2024.

RNB Smith. A scheme for predicting layer clouds and their water content in a general circulation model. *Quarterly Journal of the Royal Meteorological Society*, 116(492):435–460, 1990.

G Sommeria and James W Deardorff. Subgrid-scale condensation in models of nonprecipitating clouds. *Journal of the Atmospheric Sciences*, 34(2):344–355, 1977.

Dario Sperber and Klaus Gierens. Towards a more reliable forecast of ice supersaturation: concept of a one-moment ice-cloud scheme that avoids saturation adjustment. *Atmospheric Chemistry and Physics*, 23(24):15609–15627, 2023.

Gregory Thompson, Chloé Scholzen, Scott O'Donoghue, Max Haughton, Roderic L Jones, Adam Durant, and Conor Farrington. On the fidelity of high-resolution numerical weather forecasts of contrail-favorable conditions. *Atmospheric Research*, 311:107663, 2024.

Benoit Vié, J-P Pinty, Sarah Berthet, and Maud Leriche. Lima (v1. 0): A quasi two-moment microphysical scheme driven by a multimodal population of cloud condensation and ice freezing nuclei. *Geoscientific Model Development*, 9(2):567–586, 2016.

Weiyu Zhang, Kwinten Van Weverberg, Cyril J Morcrette, Wuhu Feng, Kalli Furtado, Paul R Field, Chih-Chieh Chen, Andrew Gettelman, Piers M Forster, Daniel R Marsh, et al. Impact of host climate model on contrail cirrus effective radiative forcing estimates. *EGUsphere*, 2024:1–25, 2024.