

Response to Referee #1

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This statement concerns our revision of the Paper #egusphere-2026-1463 submitted to the Atmospheric Measurement Techniques, entitled “*Revisiting error models for the assimilation of all-sky infrared satellite radiances*”, based on the referees’ report.

We would like to thank all reviewers for taking the time to read and review our manuscript. In response to the comments we have added another 1000 bootstrapping runs based on 3 months of data from another modelling system (DWD ICON) data in another season (winter). As we incorporated an additional data set from DWD, we also added Annika Schomburg as additional coauthor from DWD. A description of the ICON data is added in Sect. 2.3. To show the bootstrapping runs more clearly, we split Sect. 5 into two subsections. Results from AROME and ICON are presented in Sect. 5.1 and Sect. 5.2, respectively. Additionally, we incorporated the findings into the ‘Discussion and summary’ section. The overall conclusion, however, remain largely unchanged as the new dataset confirmed the main conclusions.

Other than the large amount of text tweaks due to adding the additional dataset, the biggest change was the inclusion of two additional figures (Figs. R1 and R2), which are Figs. 9 and 14 in the revised manuscript. Figure R1 illustrates that the maximum cloud effect is physically better justified, and Fig. R2 gives a more detailed illustration of the bootstrapping results for every run.

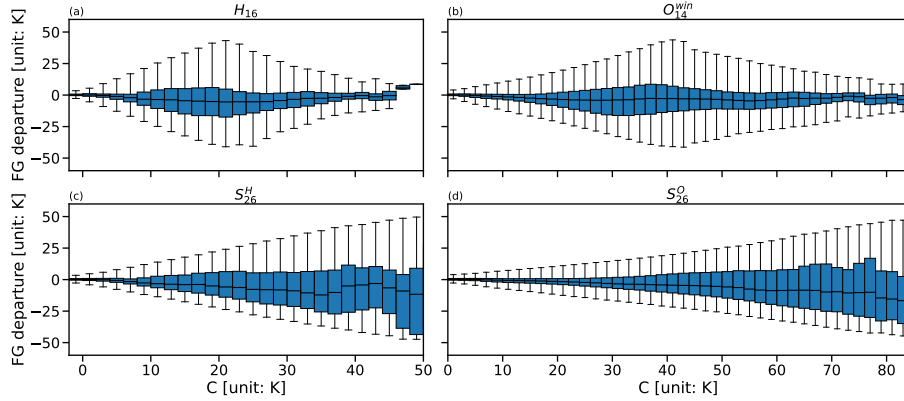


Figure R1: Box plots of Ch7.3 FG departures (verification period) binned for every 2 K of C . C computed with (a) H_{16} method, (b) O_{14}^{win} method, (c) S_{26}^H method and (d) S_{26}^O method. The lower and upper boundaries of the box are the first quartile and third quartile, respectively; the lower and upper whiskers show the 0.5 percentile and 99.5 percentile, respectively.

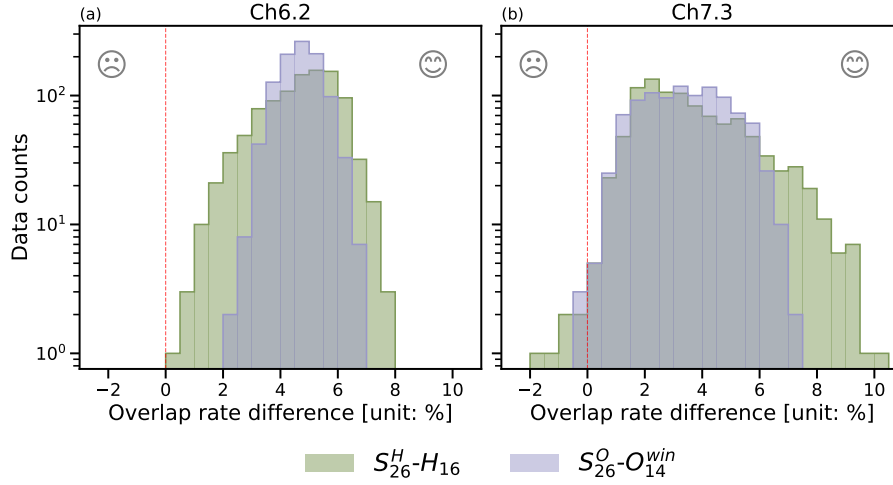


Figure R2: Histogram of overlap rate difference between S_{26}^O and O_{14}^{win} methods (deep indigo) and between S_{26}^H and H_{16} methods (olive gray) for (a) Ch6.2 and (b) Ch7.3. Results are computed based on 1000 bootstrapped runs.

In addition, we corrected one error in L247-250, where we explain which data is removed by QC. We can not know whether the FG departure is twice the cloud effect or minus twice the cloud effect based on the conditions we provide. Thus, we changed the text into: *"We can understand which data is removed by looking at when the absolute FG departure is about twice the cloud effect. This can occur when no clouds are present in the model ($BT_{\text{cld}}^M = BT_{\text{clr}}^M$), resulting in an $|FG \text{ departure}|$ exactly twice the cloud effect, or when $BT^O \approx BT_{\text{clr}}^M$, resulting in an $|FG \text{ departure}|$ approximately twice the cloud effect."*

We also corrected some typos throughout the text, and replaced some full names (such as brightness temperature) with their abbreviations for consistency.

Below we respond to the comments of referee #1. Text in *italics* is the new text in the paper. We have not included here all the modified text from the manuscript as it would make this long document even much longer.

Comments by Referee #1

Major comment 1. The authors proposed using the maximum cloud effect instead of the mean cloud effect, but the theoretical justification for this claim is insufficient. It is recommended that the authors provide additional statistical theory or sensitivity experiments (e.g., using different quantiles such as 90% or 95%) to demonstrate that the maximum is the optimal choice.

Thank you for the advise. We've added two additional figures Figs. R1 and R2 (Figs. 9 and 14 in the revised manuscript) to better illustrate that the maximum cloud effect is physically better justified and can lead to more Gaussian distributions than the original mean cloud effect.

The first new figure (Fig. R1) displays the FG departure distributions binned by different C . It shows that the statistics of the FG departures have a clear monotone relationship to the max

C s (new modified approaches) which only holds for lower values of C in the original formulations. Thereby, the revised C s are a better predictor of FG departure statistics than the original versions.

The second new figure (Fig. R2) shows the histograms of overlap rate differences between the revised and original approaches from the 1000 bootstrapped experiments, providing complementary information to the mean rates shown in Table 1. This figure shows that the max approaches outperform the original approaches in all 1000 experiments for Ch6.2, and in over 99% of experiments for Ch7.3.

Major comment 2. The study only used a one-month dataset (August), and the generalizability of the methods and findings requires further verification. It is recommended to conduct additional validation across different seasons (e.g., the convectively active summer period and the stably stratified winter season).

As noted in the beginning of the response, we've added three winter months (Jan, Feb, Dec) of data from the DWD operational model ICON as an additional dataset in response to this comment and similar comments from the other reviewers. As the additional data covers a longer period, a different season and data from a different modelling system, it serves as an independent and complementary test for the AROME results and supports the generalizability of the results. We conducted another 1000 bootstrapping runs with the new dataset, confirming that the max approach outperforms the mean approach.

A description of the ICON data set and results of the error models using the additional data have been added in Sect. 2.3 and Sect. 5.2. Furthermore, we've incorporated the findings into the 'Discussion and summary' section and made some minor revisions throughout the manuscript to make it consistent.

Major comment 3. This study only examined the Gaussianity of the departure distribution and did not demonstrate the impact of the proposed error model on analysis quality or forecast skill when applied in an actual data assimilation cycle. The authors could appropriately conduct idealized or real data assimilation experiments to illustrate its practical value.

We fully agree that it would be interesting to also test the error models in data assimilation experiments in addition to the evaluation of the Gaussianity of the normalized departures. This endeavor is certainly planned for the future, but conducting such experiments for an extended period to achieve significant results is currently not feasible with the GeoSphere AROME system and our current computing resources. Furthermore, describing and evaluating such experiments would lead to a significant expansion of the currently already rather long manuscript with 14 figures. Thus, we have decided to leave such an evaluation to future studies.

Minor comment 1. The terms "Cloud-height effect" and "cloud effect" are used interchangeably in the text, but the two are not entirely equivalent.

Thank you for the suggestion. We've changed all the "cloud-height effect" to "cloud effect" for consistency. In addition, we changed "the nonlinear cloud height effect" in L284 and L301 to "the

nonlinear effect caused by cloud height". "cloud height effect" in L402 was changed to "effect of cloud height".

Minor comment 2. Line 107: "Austria in 2023, ." Change to "Austria in 2023."

Revised accordingly.

Minor comment 3. Line 121: "for the BTclr calculation" maybe "for the BTcld calculation"?

Thanks for noting this, it should be " BT_{cld}^M ". Revised accordingly.

Minor comment 4. Figure 1: "BTclr" in caption change to "BTcld". Please check the correct use of these physical quantities throughout. E.g., Lines 160-165, caption of all the Figs.

Thanks for the careful review. We've thoroughly checked the manuscript and corrected errors and inconsistencies.

Minor comment 5. Line 161: Delete a "between".

Revised accordingly.

Minor comment 6. Line 277: What is "CA"?

Thank you for pointing this out. This should be " C ". Revised accordingly.