Author's Response

We would like to thank the reviewers for reviewing the revised manuscript and for their constructive comments. In the following, we reply to the individual comments from the reviewers. We use colour to organize this as follows:

- The questions and comments from the reviewers are marked in blue.
- The replies from the authors are written in black.
- Any changes to the manuscript are written in green.

Reply to report #1

Reviewer: L115f: In the tropics, ISSRs are rare below 10000 m, and have been show to occur at altitudes of more than 15000 m <— shown instead of show

Authors: Thank you for pointing this out. We have corrected show to shown.

Reviewer: L197ff: In the extratropics, we find that ERA5 has a cold bias of 0.5 K on average in the upper troposphere, but it cannot be ruled out that this is not due to sensor error as the mean difference is within accuracy range of the IAGOS temperature sensor. <— There is a double negative here.

Authors: The double negative in this sentence has been corrected. It now reads as follows:

"In the extratropics, we find that ERA5 has a cold bias of 0.5 K on average in the upper troposphere, but it cannot be ruled out that this is due to sensor error as the mean difference is within accuracy range of the IAGOS temperature sensor."

Reviewer: L340: upper tropopshere. Typo

Authors: We have fixed this to "upper troposphere".

Reviewer: L340: It is also interesting to note than in South Asia in JJA <— that instead of than

Authors: Thank you for noting this mistake. This has been corrected to "It is also interesting to note that in South Asia in JJA…"

Reviewer: L406: However,the ISSR fraction is sensitive <— missing space

Authors: The missing space has been fixed.

Reviewer: L500: conditions compared to clear-sky for a a dataset <— double „a“

Authors: This has been fixed: “… conditions compared to clear-sky for a dataset …”

Reply to report #2

Reviewer: Studying the equitable threat score (ETS) for different RHi threshold of ERA5 is a major improvement. It clearly shows that lowering the RHi threshold can increase the ability of ERA5 to predict ISSRs. However, I get the impression that the results (in which the ETS is usually quite low), are being over-interpreted to some extent.
As an example, I refer to line 337: “In North America and Europe, the best ETS at 30 hPa above tropopause was found at an RHi threshold of 75%; perhaps lowering the threshold even more could allow for more improvements.” I doubt that there is a physical explanation for the best ETS at a threshold of 75% (or even lower). The dry bias in ERA5 is not large enough to justify this threshold. I rather believe that this result (with also quite low values) has no physical meaning but is only found because of the very low number of ISSR occurrences in the LS. I would recommend not using thresholds below 85% unless you can explain why using lower thresholds makes physical sense.

Authors: Thank you for this feedback. The justification for the lower threshold is that in some instance, such as in the upper troposphere in Europe, some minor deviations are already observable at 75%, but they are not very significant yet. Therefore, we have adjusted the analysis to not use thresholds below 85% and subsequently edited the text both in Section 3.4, Section 3.5 and conclusion. The paragraph related to line 337 now reads:

“In the extratropic lower stratosphere, lowering the RHi threshold in ERA5 shows a minor improvement in the ETS, but the ETS score still indicates a weak correlation between IAGOS and ERA5. There is a low number of ISSR occurrences in this layer, which combined with the underestimation of RHi in ISS conditions, might make it difficult for ERA5 to predict ISSRs in these instances, resulting in a low ETS.”

Reviewer: I like the detailed investigation of the “sensitivity to definition of cloudy and clear-sky conditions”. This is very helpful to get an understanding of the differences between the different possibilities to distinguish between cloud free and cloudy conditions.

However, the explanation why the use of IAGOS ice crystal number concentration and not ERA5 CIWC should be preferred doesn't convince me. The ice crystal number concentration from IAGOS BCP is not designed to measure subvisible clouds and, hence, known to miss the very thin clouds; ERA5 CIWC on the other hand is a very sensitive tool to even detect few and small cloud particles.
Starting at line 44 in the supplement it is stated: "[...] the CIWC thresholds established by Wang et al. (2025) and Petzold et al. (2025) tends to classify a measurement as in-cloud if there is any sign of the presence of ice clouds. Hence, if we want to observe ice supersaturation under clear-sky and cloudy conditions for contrail avoidance purposes, we recommend to make use of the ice crystal number concentration in IAGOS and the cloud cover in ERA5."
In my opinion it is good to classify a measurement as in-cloud, if there is any sign of the presence of ice clouds. Do you mean that for contrail avoidance purposes it is not necessary to distinguish between clear-sky and subvisible clouds because in both cases aircraft flying in these regions could cause contrail cirrus with a warming effect? And that therefore the ice crystal number concentration from IAGOS BCP is sufficient? Then, please phrase it like this (line 429 and in the supplement). Incidentally, including regions with sub-visible cirrus would also be possible with CIWC by using a higher threshold.

Authors: We thank the reviewer for this comment. We agree that the original explanation was unclear. Our recommendation concerns the choice between the CC and the CIWC, rather than between the CIWC and the IAGOS BCP. We maintain the IAGOS BCP as the observational reference, acknowledging that it may miss very thin cirrus due to design limitations. We recommend the CC as the more appropriate ERA5 counterpart to the BCP. This is because, as the reviewer also pointed out, distinguishing between clear-sky and subvisible cirrus is not critical for contrail avoidance as contrail cirrus would result in a warming impact in both cases. However, we also acknowledge the CIWC thresholds could be adjusted in order to better match the CC, but this requires further investigation. We have revised the text accordingly in the paper and in the supplement. If anything remains unclear, we are happy to clarify further. In the supplement, the paragraph including Line 44 now reads: "The main difference between the ice crystal number concentration, the CC and the CIWC definition is how the data points are classified. With the ice crystal number concentration and the CC definition, more points are classified as clear-sky compared to the CIWC definition, as the CIWC tends to classify measurements as in-cloud if there is any sign of the presence of ice clouds. This results in a lower probability of observable ice supersaturation conditions in clear-sky conditions when using the CIWC definition, also confirmed by Wang et al. (2025) and Petzold et al. (2025). As an example, the IAGOS ISSR fraction in the North Atlantic under clear-sky conditions, found with the ice crystal number

concentration, shows a maximum of approximately 25% at 30 hPa below the tropopause. However, with the CIWC definition, this maximum is 5%. For contrail avoidance purposes, the distinction between clear-sky and subvisible cirrus is not critical because contrail cirrus would result in a warming impact in both cases (Petzold et al., 2025). Therefore, maintaining the ice crystal number concentration in IAGOS as the observational reference, we recommend using the cloud cover in ERA5 to distinguish between cloudy and clear-sky conditions. The thresholds for the CIWC could be adjusted to reduce the number of points classified as in-cloud, but determining the optimal threshold would require further investigation."

Reviewer: Line 39: " MOZAIC [...] which is currently part of IAGOS": MOZAIC is the predecessor of IAGOS.

Authors: Thank you for highlighting this, we have rephrased the sentence to: "Reutter et al. (2020) showed that ERA-Interim, the precursor of ERA5, underestimated RHi when greater than 100\%, leading to an underestimation in the occurrence of ISS when compared to MOZAIC (Measurement of OZONE and Water Vapour on Airbus in-service Aircraft), the predecessor programme of IAGOS."

Reviewer: Line 85: " extensive geographical and temporal area.": extensive geographical and temporal range.

Authors: We have changed area to range.

Reviewer: Line 93: I must apologize – I overlooked that during my initial review: Since you use the ice crystal number concentration a short description of the BCP should be given in the Data and methodology section.

Authors: We appreciate the reviewer making us aware of this. Under Section 2.1, we have added a paragraph describing the BCP, after line 109. This is the description we have added: "IAGOS aircraft measure the concentration of cloud particles in the size range of 5 to 75µm using the BCP, a single-particle optical backscattering spectrometer. The BCP operates using a laser diode, with a polarised light at a wavelength of 658 nm. The light is directed through a silicate glass window onto a target region approximately 4 cm from this window. When particles pass through the target region, they backscatter light with an intensity that depends on their size, refractive index, shape and the angle at which they intercept the beam. From this, the diameters and concentrations of the particles can be derived (Beswick et al., 2014)."

Reviewer: Line 96: Please check the start date of CARIBIC

Authors: Thank you for pointing out this mistake. The start date of CARIBIC has been fixed to 1997.

Reviewer: Line 116: typo: show => shown

Authors: The typo has been fixed.

Reviewer: Line 121: We considered….

Authors: We have considered this comment, and the sentence has been reformulated to: " We considered the Northern and Southern Hemisphere tropics separately due to the inter-tropical convergence zone,…".

Reviewer: Line 132: since the quality flag of RHi wasn't well derived at the time of data collection.

Authors: We have included this in the sentence. It now reads as follows: "However, we use the RHL validity flag to select RHi values since the quality flag of RHi was not well derived at the time of data collection, …"

Reviewer: Line 146: Rephrase: "Since the ARCO ERA5 dataset does not provide RHi, it is derived from specific humidity, temperature and the saturation water vapour pressure over ice are used to calculate RHi."

Authors: The sentence has been fixed to: "Since the ARCO ERA5 dataset does not provide RHi, it is derived from specific humidity, temperature and the saturation water vapour pressure over ice."

Reviewer: Line 189: It would be good to include a sentence explaining why you are investigating the same seasons in tropical regions as in extratropical regions (same for RHi).

Authors: We have included the following sentence: "The same seasonal definition is applied for all regions to allow for a consistent comparison between the tropics and extratropics." In Section 3.2 related to the seasonal and vertical distribution of RHi, we have modified the sentence on line 206/207 to: "The mean vertical distribution of RHi in the

IAGOS and ERA5 datasets per season and geographic region is presented in Fig. 6, using the same seasonal and regional definitions as in Sect. 3.1."

Reviewer: Line 192: S10? Please check all references to the figures in the supplement.

Authors: Thank you for highlighting this inconsistency. We have fixed this reference and the other references to the figures in the supplement.

Reviewer: Line 204: "Less than 5% of our IAGOS measurements show ISS above this temperature threshold." I recommend to delete this sentence.

Authors: This sentence has been removed.

Reviewer: Line 219: "Based on the correlation between RHi and cloud ice particles, the lower RHi could be related to little to no cloudy conditions in DJF (see Fig. S13)." I recommend deleting this sentence. There is no doubt that these two variables are correlated, and besides, the relationship works the other way round: high humidity leads to cloud formation.

Authors: As per the reviewer's recommendation, we have deleted the sentence "Based on the correlation between RHi and cloud ice particles, the lower RHi could be related to little to no cloudy conditions in DJF (see Fig. S13)."

Reviewer: Line 222: typo: as seen in as seen in

Authors: We have removed the duplicated "as seen in".

Reviewer: Line 226: typo: it could due to => it could be due to

Authors: We appreciate this typo being pointed out, it has been corrected.

Reviewer: Line 312: typo: shows => show

Authors: This typo has been fixed.

Reviewer: Line 329: "This could be due to lower mean RHi values and thus ISSRs in warmer months…" ISSRs are not necessarily correlated to the mean RHi values but rather to the high percentiles. I recommend deleting "mean" and inserting the word "less" before ISSRs.

Authors: The sentence has been changed to "This could be due to lower RHi values and thus less ISSRs in warmer months, such as JJA, although North Asia shows the highest ISSR fraction for this season, as seen in Fig. 9."

Reviewer: Line 338: see major comments

Authors: Since this is related to the first major comment, we copy our previous response: Thank you for this feedback. The justification for the lower threshold is that in some instance, such as in the upper troposphere in Europe, some minor deviations are already observable at 75%, but not significant. Therefore, we have adjusted the analysis to not use thresholds below 85% and subsequently edited the text. The paragraph related to line 337 now reads:

"In the extratropic lower stratosphere, lowering the RHi threshold in ERA5 shows a minor improvement in the ETS, but the ETS score still indicates a weak correlation between IAGOS and ERA5. There is a low number of ISSR occurrences in this layer, which combined with the underestimation of RHi in ISS conditions, might make it difficult for ERA5 to predict ISSRs in these instances, resulting in a low ETS."

Reviewer: Line 340: typo: than => that

Authors: Thank you for pointing this out, it has been corrected.

Reviewer: Line 341 - 345: I don't understand why cloudy conditions should be a limiting factor when decreasing the ERA5 RHi threshold to improve its ability to predict ISSRs. Wouldn't that be exactly the situation where decreasing the threshold would "repair" the saturation adjustment?

Authors: We appreciate the reviewer's concern regarding this statement. The issue is mainly related to the increase in false positives in cloudy conditions when decreasing the threshold, which is discussed in the subsequent section. Therefore, we have removed these lines. The end of the paragraph, including Line 341-345. now reads as follows: "In Southern Trans-Pacific, we find that ERA5 shows better prediction of ISSRs when lowering the RHi threshold in DJF, compared to in JJA, MAM and SON, with SON showing the lowest ETS. SON has the highest fraction of cloudy conditions and DJF the lowest. The effect of cloudy conditions on the ETS with different RHi thresholds will be analysed further in the following section."

Reviewer: Line 380: Please clarify. Does the extratropical cold bias of 0.5 K in ERA5 compared to IAGOS refer to the upper troposphere or to an average across all altitudes?

Authors: Correct, this refers to the upper troposphere. The sentence has been corrected to include this: "The extratropical upper tropospheric cold bias of 0.5 K in ERA5 compared to IAGOS is in line with.."

Reviewer: Line 382: I suggest to add behind ERA5 "compared to IAGOS of 0.75- 1 K".

Authors: This has been incorporated. The sentence is now: "The observed extratropical lower stratospheric cold bias in ERA5 compared to IAGOS of 0.75–1 K aligns with results from.."

Reviewer: Line 409: "In instances where the RHi is overestimated by ERA5, such as in South Asia for season JJA, the ISSR fraction is overestimated by ERA5." I suggest to slightly alter the sentence: As expected, when the RHi is overestimated by ERA5, as it is in South Asia during the JJA season, the ISSR fraction is overestimated by ERA5 as well.

Authors: As per the suggestion, the sentence has been modified to: "As expected, when the RHi is overestimated by ERA5, such as in South Asia for season JJA, the ISSR fraction is overestimated by ERA5 as well."

Reviewer: Line 432: see major comments

Authors: Since this is related to the second major comment by the reviewer, we repeat the response from earlier:

We thank the reviewer for this comment. We agree that the original explanation was unclear. Our recommendation concerns the choice between the CC and the CIWC, rather than between the CIWC and the IAGOS BCP. We maintain the IAGOS BCP as the observational reference, acknowledging that it may miss very thin cirrus due to design limitations. We recommend the CC as the more appropriate ERA5 counterpart to the BCP. This is because, as the reviewer also pointed out, distinguishing between clear-sky and subvisible cirrus is not critical for contrail avoidance as contrail cirrus would result in a warming impact in both cases. However, we also acknowledge the CIWC thresholds could be adjusted in order to better match the CC, but this requires further investigation. We have revised the text accordingly in the paper and in the supplement. If anything remains unclear, we are happy to clarify further. In the supplement, the paragraph including Line 44

now reads: “The main difference between the ice crystal number concentration, the CC and the CIWC definition is how the data points are classified. With the ice crystal number concentration and the CC definition, more points are classified as clear-sky compared to the CIWC definition, as the CIWC tends to classify measurements as in-cloud if there is any sign of the presence of ice clouds. This results in a lower probability of observable ice supersaturation conditions in clear-sky conditions when using the CIWC definition, also confirmed by Wang et al. (2025) and Petzold et al. (2025). As an example, the IAGOS ISSR fraction in the North Atlantic under clear-sky conditions, found with the ice crystal number concentration, shows a maximum of approximately 25% at 30 hPa below the tropopause. However, with the CIWC definition, this maximum is 5%. For contrail avoidance purposes, the distinction between clear-sky and subvisible cirrus is not critical because contrail cirrus would result in a warming impact in both cases (Petzold et al., 2025). Therefore, maintaining the ice crystal number concentration in IAGOS as the observational reference, we recommend using the cloud cover in ERA5 to distinguish between cloudy and clear-sky conditions. The thresholds for the CIWC could be adjusted to reduce the number of points classified as in-cloud, but determining the optimal threshold would require further investigation.”

Reviewer: Line 438: delete: “which are weaker.”

Authors: We have removed this.

Reviewer: Line 458: Maybe better: “For future work, it would be helpful to increase…”

Authors: Thank you for the feedback. We have modified the sentence to “For future work, it would be helpful to increase the number of IAGOS aircraft capable of measuring the ice crystal number concentration to further distinguish between cloudy and clear-sky conditions.”

Reviewer: Line 462: You could mention the saturation adjustment.

Authors: We thank the reviewer for this suggestion. In the second point of the conclusion, we mention the saturation adjustment with regards to the PDF of RHi under cloudy and clear-sky conditions. We hope this addresses the reviewer’s comment. If this was not the intended suggestion, we would appreciate further clarification.

Reviewer: Line 502: same as above

Authors: We are unsure whether this refers to mentioning the saturation adjustment, as suggested for Line 462, or to the second major comment. To appropriately address this suggestion, we would appreciate further clarification.

Reviewer: Line 513: Why? Shouldn't it be possible to decrease the RHi threshold for ISS (as long as you have ERA5 RHi).

Authors: Thank you for raising this point. What we meant to say was that the in-situ measurements are necessary to determine the most optimal threshold in ERA5. We have rephrased the sentence as follows: "The lack of ISS may for the time being be improved by decreasing the RHi threshold for ISS, although the optimal threshold value can only be determined for regions where in situ measurements are available to validate it."

Reviewer: Line 536: I don't understand what you mean with "same region".

Authors: We agree that using "same region" is vague. The sentence has been changed to: "For example, when the ITCZ shifts northward toward season JJA, wet conditions prevail in the tropical Northern Hemisphere while dry conditions prevail in the tropical Southern Hemisphere, and vice versa toward DJF (National Oceanic and Atmospheric Administration, 2023)."

Reviewer: Line 540: typo to => the

Authors: Thank you for point this out, we have fixed the sentence to use to instead of the, e.g. "… we recommend to consider the Northern and Southern Hemisphere separately."

Reviewer: Line 561: delete: in the upper troposphere

Authors: We have deleted "in the upper troposphere". The sentence now reads as follows: "We also find that the thermal tropopause is drier in comparison to the dynamic tropopause."