Response to Review

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June 3, 2026

1 General comment

First of all, we thank both reviewers for their helpful comments and suggestions which lead to a significant improved manuscript. This document contains our responses to the reviewer comments. The original reviewer's comments are in blue, our response is written in black. We list the reviewer comments below together with our responses.

Following the suggestions of both reviewers we have tried to shorten the method's part of the manuscript. In addition, we put several additional results, which are kind of side products of the evaluation are put into a supplement. Thus, we reduced the text of the manuscript by about 20%.

2 Response to reviewer #1

I am happy that the authors have taken their time to improve the paper considerably. It is publishable, but I think it could still be improved and I will give my recommendations/suggestions for this below.

My main point, that refers to several locations in the text is that you should be careful to distinguish between ISSRs as physical phenomena in Nature versus ISSRs as represented numerically in a weather model. The discussion of that may easily become philosophical, but I think, it has some consequences: For instance, tiny differences of D due to different methods to compute it are probably systematic ones and using a sufficient amount of data one may prove them as statistically significant (say, on the 5% level). However, the more important question is whether these tiny differences have any physical meaning and I doubt that this is the case. The discussion in Appendix D is another example where, to my view, too much importance is given to this purely mathematical aspect, that has not much to do with the real objects or phenomena in Nature. So to my view, methodological differences should be toned down.

Generally, we agree to concentrate more on the physical aspects of ISSRs instead of mathematical details. However, by definition ISSRs are objects constructed in a mathematical way, i.e. using a threshold value (which is physically based as thermodynamic equilibrium, of course) for distinguishing between different parts of the air masses. In addition, we have to make sure that our insufficient data (in terms of physics, since ERA5 is underestimating relative humidity) does not additionally introduce errors in the evaluation, just because of grids and other more technical issues (this is the reason for the more technical appendices). In addition, we try to use the mathematical properties (as e.g. fractal dimensions) for deriving information of the physics behind, e.g. the seasonal (mathematical) differences of D point to real/physical differences in the formation processes. Overall, we tried to make this more transparent in the current version of the manuscript, including also a disclaimer about possible differences between natural and modelled ISSRs.

A related aspect is that your results, obtained from a model, refer strictly to the model representation of ISSRs. I doubt therefore that such results can be used to develop "a meaningful representation in models on different scales and resolutions" (L 444). To me it is sufficient that the fractal properties of ISSR-representations in models are demonstrated, that there are similarities with results from past related studies on clouds and other phenomena, and it is in particular important that there is a seasonal cycle. The latter has indeed consequences

(and causes) as you show in your paper. These causes and consequences are physical, not mathematical or from the model only, so they are indeed important. I think, your paper does not loose any importance if a statement referring to model development or evaluation is just left out.

The reference to model development has been cut from the manuscript. We tried to make clear that one of the main findings is the self-similarity of ISSRs. We shifted the change of resolution to the supplement. Nevertheless, we see from this exercise that the self-similarity seems to be very robust, which underlines the possible use for further model and data evaluation.

I have still a couple of monor points worth to be thought about:

Eq. 4 and any appearance of "D": It is a bit counterintuitive that D=1 for planar objects. I think most people agree that they are two-dimensional. But the fact D=1 becomes intuitively clear once it is understood that D is the dimension of the perimeter of this object, not of its area. So I suggest to guide the reader a bit by stating that D should be understood as the dimension of the permimeter. This guidance can be repeated sometimes in the paper.

We would agree that the actual definition of D is quite counter-intuitive. However, in former investigations this perimeter-area relation and thus this definition of D has been used; for consistency we stay with this new/old definition. The pointer that D should be understood as the dimension of the permimeter has been added.

Page 8, "total area": it would be good to additionally provide the average area per ISSR, just divide total area by total number. This would fit better to your subsequent evaluation of the pathlenght statisitcs. It comes later in the conclusions, but it would fit here.

The section about the average area per ISSR has been moved.

LL 427-429: I suggest to delete this short par. It just opens a speculation, but does not follow it further. So, best is to delete it.

The aforementioned lines have been deleted.

L 433: I think, the true reason is that in the old studies ISSRs were defined by RHi>100% (as measured), while here it is ISSR =: RHi>90% (as modelled).

ERA5 data underestimates the humidity in the UTLS region. That is why we purposefully choose ISSR =: RHi>90% (as modelled). RHi>90% (as modelled) is closest to RHi>100% (as measured). Aircraft measurements are able to measure subsaturated area much smaller than the ERA5 resolution. A IAGOS flight thus measures multiple short pathlengths where we measure just one. Actually, sensitivity studies show that the actual threshold does not change the quality of the results. This is meaningful, since the humidity gradients at the ISSRs' edges are very steep (see discussion in Spichtinger & Leschner, 2016).

L 571,572: The result that D turns out largest for the coarsest resolution is surprising. Do you have any explanation of it?

This is indeed counter-intuitive, but there is, as we think, a satisfactory explanation. Comparing the total area (summed up per month) across resolutions, we observe the area staying roughly the same. This makes sense as both some supersaturated and some subsaturated pixels will be lost in the process of lowering the resolution. Comparing the total perimeter (summed up per month) across resolutions, we can see the perimeter being smallest for the lowest resolution. This is expected as we smooth out edges. A constant area and lower perimeter should intuitively lead to a lower fractal dimension. Here, we need to remember that the dimension for a single month is not calculated with the sums of area and perimeter, but as a regression slope with the individual areas and perimeters of single ISSRs. Looking at the average perimeter per ISSR, we see a significantly higher ratio for the very coarse resolution. This is caused by the coarsening "melting" ISSRs together that were previously separated by thin subsaturated gaps. Thus, a higher perimeter per ISSR raises the dimension.

There are several expressions that sound a bit strange:

L 9: "slopes equaling fradctal dimensions", better slopes implying fractal dimensions.

L 68,69: "temperature and pressure in terms of ice supersaturation" ???

L 119: "OF" Christensen and Driver.

L 121: "artificially"

Figure 1 caption: The final remark "possible holes..." is unnecessary here. (In fig. 2 it is justified).

L 252: "spaTial".

L 326, 363: "normalized statistical distribution". How normalized? If you mean a probability density function, please say so.

All of the above have been considered and corrected.

L 341: "the distribution presents right-skewed" ???

The distribution is not symmetrical but exhibits a tail for large pathlengths. This causes the mean and the median to differ from another. As we specifically list means and medians, explaining their different values and the resulting implications is important. The wording is correct, an asymmetric distribution with a long tail towards the right hand side of the mean value is called "right-skewed"

L 342: The word "this" is misleading here. It connects two different and independent statements, 1) that extreme pathlenghts shift the mean to a larger value, and 2) that this shifting of the mean by extreme values would indicate two formation mechanisms. I don't believe that this sense was intended. I believe rather that the difference between the summer and winter pdfs points to two different formation mechanisms. However, if I am wrong here and you mean with "discussed later" the discussion around lines 355 +-, I still think that the word "this" does not fit here. Please clarify.

You are right, this line gave the wrong impression, it has been deleted.

L 359. "distinct spatial scales, i.e. processes acting on distinct but different scales" sounds funny.

L 388: "few ten thousand of kilometers". As the equator itself is only a few (4) ten thousand kilometers long, this expression is surprising.

L 410: "probabilities are probably..." ??? please reformulate.

L 427: first sentence is incomplete.

L 452: insert "total" before "area".

L 463: what is "really large"?

All of the above have been corrected.

L 464: good that you have a "realistic pathlength statistics" :-)

This might sound funny but this phrasing is used for a lack of better alternatives.

L 464: I suggest to write "consistent with" instead of "comparable to".

This has been improved.

L 489: "larger" than what?

The average area per ISSR in summer is noticeably larger at 250 hPa compared to 200 hPa and 300 hPa. This was rephrased to mitigate misunderstandings.

L 494: The word "intermittently" is a notion from turbulence theory. It does probably not fit here.

This has been corrected.

3 Response to reviewer #2

The manuscript presents scientifically interesting analyses, and the results and conclusions appear to be well supported by the methods and data. However, substantial editing is required before publication. The paper can be significantly shortened (at least 30% shorter) and more concise so that key results can be more clearly understood and appreciated. Overall, while the study is of clear scientific interest, revisions are required to improve clarity, conciseness and readability.

General comments:

1. Background, methods and results should be separated more clearly to improve readability of the manuscript, key examples are:

1.1. Consider removing LL 69 – 78 (starting from "For the investigation..." until "... dimension D=2α."). It is confusing to find such a summary of the methods in the introduction. A shorter version of this paragraph could instead be moved into the introduction of Section 2 (adding it after L 88) but this is not necessary.

This has been edited.

1.2. Section 2.2.3 can be shortened significantly. Focus on explaining the method used in this study, i.e., how fractal dimension is estimated using the Area-Perimeter relationship. The introduction of the Hausdorff dimension and its comparison to the Area-Perimeter relation (LL 151-162) can be shortened for improved clarity and conciseness. Consider moving LL 162-166 into the introduction (e.g. after L 69) as they focus on background literature and not methodology.

This section has been shortened and edited.

2. Conciseness and accuracy of language could be further improved for improved clarity, key examples are:

2.1. Avoid long strings of arguments within a single sentence. Instead, aim to communicate using short, concise sentences (e.g., start new sentences rather than using a semicolon in lines 25, 33, etc.).

The language has been revised.

2.2. Check for duplicate information and remove it to improve conciseness. For example: 2.2.1. L 32: consider removing “due to the very low background concentrations” 2.2.2. L 131: consider removing “Further details of the method can be found in appendix A.” as this was already mentioned at the start of section 2.2.

The duplicates have been removed.

2.3. Ensure that contrastive connectives (e.g., “however” or “although”) are used appropriately and convey the intended meaning. For example: 2.3.1. L 28: “although” should be replaced with “but” or “however” for improved clarity (and “themselves” should be removed). 2.3.2. L 37: the sentence beginning with “however” does not contradict the preceding argument.

All unfitting uses of "however" and "although" have been corrected.

3. I was surprised to see a section titled “Conclusion, Discussion and Outlook.” In the earlier draft of the manuscript, the section labelled “Discussion” appeared more appropriate. If you intend to separate discussion and conclusion, the standard approach is to present the discussion of results first, followed by concluding remarks and outlook. I suggest reconsidering the organisation: either merge the summary of the main findings (currently labelled “Conclusion,” which does not seem entirely fitting) with their interpretation into a single “Discussion” section, or clearly distinguish between a results-focused discussion and a concise conclusion/outlook section.

This has been restructured, the manuscript now has a merged discussion part.

Specific comments:

1. Figure 5: Consider removing or shortening the plot titles, especially “fine grid, unfilled holes” should be removed and instead explained in the caption. “January 2017” could also be removed as it is the same for all three plots.

The titles have been removed and the captions shortened.

2. L 228: I am unsure whether there is enough evidence to call this “scale break” the separation of “two regimes”. As is pointed out later in this paragraph, the upper limit of the size of the Earth could be a simple explanation of this slight change in slope. Either way, consider using area rather than perimeter to define the location of the “scale break”: this is more intuitive as area is now shown on the x-axis.

The new manuscript uses area to define the location.

3. Section 3.3: The detailed investigation of the effect of filling the ISSR objects on their fractal dimension, as well as the discussion of the different approaches and their interpretability, is appreciated. However, since all subsequent analyses are conducted on unfilled ISSRs, the inclusion of this section in the main text detracts somewhat from the focus on the key results. Consider moving this section to the appendix, with a brief note in Section 2.2.2 indicating that the perimeter is measured including internal holes, accompanied by a reference to the appendix for a corresponding sensitivity analysis. This restructuring would also remove the need to repeatedly specify that “unfilled” data are analysed (e.g., in Figures 5 and 9), thereby improving the conciseness of the experimental descriptions.

The investigation of filled ISSRs has been moved to the appendix. The language has been adjusted accordingly.

4. LL 302 – 311: This paragraph needs restructuring for clarity. It is confusing that the first sentence finishes with “we proceed as follows” but then the method of measuring ISSR span does not immediately follow. In general, section 4 could benefit from being edited to focus on conciseness and clarity.

Section 4 has been edited, restructured and shortened.

Technical corrections 1. L 32: This sentence needs checking. Do you really mean that OLR is sensitive to “small relative perturbations” in water vapour, or instead that it is sensitive to small (absolute) perturbations in water vapour which are large relative perturbations, compared to the low background concentration?

We really mean that the OLR is sensitive to small relative perturbations. The reference Clough et al. (1992) corroborate this point of view. Their section 6 "Effect of the water vapour continuum on fluxes and cooling rates" can clarify your question: Fig. 18 shows the effect of the continuum on the water vapour cooling rate profile, which is significant. With this continuum, even small relative perturbations influence cooling rates. Sec. 6.1 names "the increase in the cooling of the upper troposphere" an "important effect resulting from the inclusion of the continuum model". Fig. 20 and Fig. 22 visualize this: small relative differences result in significantly different cooling rates.

2. L 121: “artificially” is misspelled

3. LL 129-130: This sentence is confusing, I suggest checking it and removing “as represented on a grid”.

4. LL 288, 290, 295, etc.: The word “extension” should be replaced with “extent”.

All of the above have been corrected.