Response to reviews

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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.

2 Response to reviewer #1

Major comment:

Appendix A leads to an important discussion, that it is not completed in the present version of the paper. First, neither circles nor ellipses are fractal in plane geometry. They start to be fractal only in their imperfect numerical representation as gridded objects. So there is probably an impact of the spatial resolution on the value of alpha. Probably there is also an impact of the orientation of the ellipse relative to the x and y directions, but perhaps a small one. So the main question is here, is the fractal dimension something with physical significance or is it a numerical artefact due to the gridding. If the resolution of the grid (in your numerical experiment) would be increased for a fixed ellipse, the dimension should approach 2. So this is a serious problem. What is real and what is just a numerical artefact. For the ERA5 data it is not a priori clear what a change of the resolution would result in. But I think, this is an important question. Perhaps one could test this with a subset of the data (say four months in different seasons) by artificially making the resolution coarser, perhaps 1°x1° or so. It might turn out that the actual result of the paper is that there is a fractal dimension of about 1.5 FOR A RESOLUTION OF 0.25°x0.25°.

The experiment with the frayed square is perhaps better suited to test the approach for ISSRs, because ISSRs are frayed as well, while ellipses and circles are not.

I suggest to transfer this important discussion to the discussion section of the main text and not to hide it in the appendix.

We have rewritten the appendix for clarification, in particular by distunguishing between rectangular and square representations and by providing an analytical calculation of fractal dimension. We keep the discussion of the possible changes in the shapes for forming fractals in the Appendix.

We evaluated the whole data set in terms of artificial coarsening the resolution. Actually, even for a coarse resolution (for $0.5^\circ \times 0.5^\circ$ and $1^\circ \times 1^\circ$) we find fractal behavior as manifested in a fractal dimension $D > 1$. These seems to be a kind of convergence of the values of D towards the finest resolution, but this cannot be corroborated without a much finer data set. The differences of the results across resolutions are sufficiently small ($\Delta D \sim 0.01 - 0.07$) to rule out the possibility of the results not having a physical, but numerical origin. We added some text in the manuscript, mostly in the appendix.

LL 74 ff (and in the conclusions): "we may refine atmospheric models ...". Unfortunately this is an empty promise. Please indicate some possibilities and directions how the knowledge of a fractal nature of ISSRs could be exploited to refine their modelling. Moreover, I have a strange feeling, since the fractal nature of ISSRs is here derived just from a model. So what needs to be refined, for instance in the ECMWF model, since it already produces the fractal dimensions?

We removed parts of this statement from the text. However, our evaluation might serve as a test for models in terms if they are able to represent a fractal structure of ISSRs, as found in the measurements (i.e. ERA5). We stated this in the text and hope that this is clearer now.

Minor comments:

Introduction:

L 31, 32: The sentence "making OLR ... senstitive to absolute changes ..." is a weak statement. What characterises "absolute changes"? Every change is absolute, isn't it? To my opinion, the important issue is that OLR is sensititve to RELATIVE changes of H2O concentration. This can be seen in an old paper by Clough et al. (1992). Absolute changes in H2O concentration are much larger in the lower troposphere than in the UTLS. If only the absolute size of a change would count, the H2O in the UTLS would be totally unimportant.

We agree that the formulation "absolute changes" is imprecise and potentially misleading. Our intention was to highlight that, due to the very low background water vapor concentrations in the UTLS, even small changes can lead to a comparatively strong radiative response. This is more appropriately described in terms of relative changes (or changes in radiative impact per unit concentration), rather than absolute changes.

We have therefore revised the sentence accordingly to *... making outgoing longwave radiation particularly sensitive to small relative perturbations in UTLS water vapor, due to the very low background concentrations (Clough et al., 1992).*

LL 46 ff: It is a bit surprising that the potential fractal nature of ISSRs is quasi presupposed although it is stated just before, that the 2D/3D properties of ISSRs have never been studied. Later in this paragraph follows a lot that would support the hypothesis that ISSRs could be fractal. I suggest to reorganise this paragraph a bit, so that a kind of logic appears, e.g.: First state that many studies have shown a fractal nature of clouds. Then it is natural to assume that also their "birth regions", that is, ISSRs, have fractal properties, and this will indeed be shown in the paper and the fractal properties will be studied.

We changed the text similarly to the suggestion, now the logic should be clearer.

Section 2:

Section 2.2: As far as I know, RH is an output field in ERA5 pressure level data. Why don't you use it directly? Or do you use model levels with interpolation on the mentioned PLs? Another question that becomes essential in section 3.1 when ISSRs are counted is, whether and how do you treat cloud-clearance. q as a grid-mean is an average over the cloud-free and cloudy part of a grid box and the resulting RHi or Si is such an average as well. How is this treated?

RH is an output variable of ERA5. However, for being consistent we used the set of variables p, q, T of the ERA5 data to calculate the saturation ratio over ice S_i. ISSRs are just defined by the saturation ratio being $S_i > S_{i,\text{threshold}}$, so we do not use any cloud clearance. ISSRs may or may not contain clouds, for our investigation this is not relevant, we do not take this into account. Also, for the definition via a threshold this is not relevant, since even an average over a cloudy part ($S_i = 1$) and a cloud-free part ($S_i > 1$) would result into a value $S_i > 1$, thus counting as ISSR grid point.

L 115, 116: I would argue that the orientation of the grid is a mathematical necessity that has no physical correspondence and that therefore the 8-neighbourhood is more appropriate to the physics of the situation.

We accept this argument for using the 8-neighborhood for our investigation.

L 118: Do I understand it correctly, when I interprete this in a way that only grid cells are counted that are completely surrounded by (8) other ISSR cells? Before, I thought that any of these 9 points counts as ISSR, and that in principle two or more points connected by a diagonal would count as an ISSR island with slant orientation? Please provide more explanation, since this is central to understanding.

Each grid cell is classified as ISSR solely based on the thermodynamic threshold ($S_i \geq S_{i,\text{threshold}} = 0.9$), independent of its neighbors. Connectivity is applied only afterwards to group ISSR cells into regions, using an 8-neighbour (Moore) definition. Thus, cells sharing either an edge or a corner (including diagonal connections) are considered part of the same ISSR.

To avoid misunderstandings, we have revised the manuscript and added an explicit clarification of the connectivity definition (see Appendix A and Fig. A1).

Section 2.3 can be drastically shortened and figure 2 can be deleted. It is not necessary to explain these basic things to the reader. Mentioning the python routine should suffice. I suggest to combine it with Section 2.4, which treats a less trivial problem.

We have shortened this section.

L 155: "Analytical manifold". I suggest to modify the discussion a bit. I assume, you are interested in physical objects, not in analytical manifolds. I agree that there is a problem with analytical manifolds, as demonstrated in the appendix. However, ISSRs aren't analytical manifolds, they have internal structure that is not resolved in the ERA5 data and so the determination of their "true" area and perimeter may be an ill-posed problem anyway. To me, it seems not necessary to invoke a special geometric problem that only occurs in the mathematical or numerical treatment, if the real object is represented anyway in a quite crude fashion in the gridded data.

We agree. We have changed the text accordingly.

Section 3:

Section 3.1: Instead of or additionally to the number of ISSRs you could provide the total area of all ISSRs, or the mean fractional area of ISSRs per month (that is, take for each output the total ISSR area, divide it by the area of the 30°-80° zone and average the result for each month), beause this quantity may have more relevance to other topics like contrail avoidance. Cf. also section 4.1 where you show that ISSRs seem to be larger in winter than in summer. My feeling here is that the number is important for checking statistical significance, but areas are physically more significant. Otherwise, I am a bit surprised by the result here, because to my knowledge there are more contrails in winter than in summer which seems to be in conflict with your result. Moreover, my state of knowledge was that there is more ISSR in winter than in summer (see eg. Spichtinger et al. 2003, MetZ, Analysis of the Lindenberg data, or Spichtinger et al.2003, QJ, Analysis of MLS data, Fig. 5). Lamquin et al. (2012, their Fig 10) show a more differentiated picture and your results should be discussed with these papers in mind. A comment on this would be welcome.

Following this suggestion, we have extended the manuscript by including an analysis of the mean ISSR area (new Fig. 15) in the discussion section. This allows us to better characterize the typical size of ISSRs and complements the number statistics presented in Section 3.1. The results reveal a pronounced seasonal cycle, with significantly larger ISSRs in winter and smaller, more fragmented structures in summer.

Section 3.2: The appearance of very small and even one-grid ISSRs is surprising since I thought they were sorted out by your 8-connectivity criterion (c.f. my comment above to L 118).

The 8-neighbour (Moore) connectivity is only used to group ISSR grid cells into connected regions and does not remove small or isolated features. In particular, even a single grid cell or a small cluster of connected cells is formally identified as an ISSR.

The exclusion of very small ISSRs is instead achieved by applying a minimum size threshold (N = 24 grid cells), as described in Section 2.2.1. Therefore, one-grid ISSRs are not expected in the analysis.

LL 262 ff: Since it may be that the resulting α depends on the spatial resolution of the data, please indicate here which spatial resolution underly the α determinations in the quoted studies.

The resolutions of the respective data sets were specified.

Section 4:

LL 288 ff: Although it seems to be quite tempting to invoke the Weibull distribution as one of the extreme-value distributions here, it is probably a wrong application. You are not considering, say, monthly populations of pathlengths and then consider for every month the maximum. Perhaps if would be ok, if you would put the maximum pathlenght of each single ISSR island into a pool and then study the distribution of these pooled maxima. Such excercises would be an application ground for extreme-value statistics. But the fact, that ISSRs have extreme humidity values does not justify to use extreme value statistics for their pathlengths.

We agree that an interpretation in terms of classical extreme-value statistics is not appropriate in our case, as we analyse the full population of ISSR pathlengths rather than a subset of extremes.

In the revised manuscript, we have clarified this. *Former investigations of pathlength statistics showed Weibull-type distributions (Gierens and Spichtinger, 2000; Spichtinger and Leschner, 2016), or at least the distributions could be well represented using Weibull-type fits. However, we emphasize that the application of the Weibull distribution in this context is purely empirical. In contrast to classical extreme-value statistics, we do not consider maxima of underlying distributions, but rather the full population of pathlengths. Therefore, a strict interpretation in the framework of extreme-value theory is not appropriate here. Instead, the Weibull distribution is used as an empirical fitting function that provides a good representation of the observed distributions and allows for a compact characterization via its parameters, which is consistent with above mentioned studies studying ISSR distributions.*

Section 4.1,2: Please check! North-south is latitude and west-east is longitude. The first sentence is hard to understand, the title says "along longitude" and in L 301 it says "North-South span". I am a bit lost.

You are right. We corrected this.

LL 318 ff: In the present paper the exponents of the Weibull distribution are relatively close to 1 (which would be a simple exponential distribution). The early study by Gierens and Spichtinger (2000) found an exponent of 0.5, which is significantly different from the present result. Do you have any ideas what could cause this difference?

In the former studies (Gierens & Spichtinger, 2000; Spichtinger & Leschner, 2016), aircraft data (i.e. in situ measurements) were used for the investigations. Even with an artificial coarsening (as in Spichtinger & Leschner, 2016), the fine structure of the ISSRs is still dominant. The ERA5 data can be seen as representing mesoscale/large scale features. This might lead to the difference in the Weibull parameters. However, the general feature of a scale break at $L \sim 10^2$ km is present in pathlength statistics as derived from aircraft data and ERA5 data.

Section 5:

Point 1: If you determine the areas as suggested above, please add the results as well. The word "heights" should be replace with "altitudes" or "higher levels".

This was corrected.

Miscellaneous:

L 80: "horizontal span" is a strange expression here, later it becomes clear what you mean. I suggest to use pathlength here and you may later use span as well.

We changed the text accordingly, using first the expression "horizontal extension", and later the word "span".

Eq. 4: Check the units or at least write in the text that the formula needs T in K and gives p_si in Pa or hPa. This is unclear.

We added following information to the (now) Eq. 3: *Here, T is given in Kelvin and p_{si} is obtained in Pa; the formulation is valid for temperatures $T > 100$ K.*

L 143: "entail" should be replaced by "include".

"Entail" was replaced.

L 162: please add a comma after "consequence" and add "s" to "become".

Both were added.

L 172: The statement that two dimensional objects have a dimension between 1 and 2 sounds a bit strange. I suggest to replace "two-dimensional" with "plane" or "planar".

This was rephrased.

Figure 5: check the grammar of the caption.

OK

Tables 1 - 3: Are the values actually precise to the order of 10 metres? (As well in the corresponding figures).

The values reported in Tables 1–3 (and corresponding figures) reflect the numerical precision of the calculations rather than the physical accuracy of the underlying data. All quantities are derived from grid-based calculations with fixed spatial resolution, and the resulting values are internally consistent and reproducible at the reported precision.

3 Response to reviewer #2

General comments:

The authors present a very extensive analysis of the fractal properties of ISSRs, and provide a lot of details which makes the study reproducible. However, I think the manuscript is too long and needs to be more concise. There are several subsections that should be removed or significantly shortened. For example, most of the method is standard methodology and is currently overexplained. Sections 2.2 to 2.5 can be shortened significantly, with a focus on keeping key information for reproducibility but removing explanations / justifications of standard methodological steps. Most of these subsections should be removed or moved into an appendix to provide detailed information for readers who are unfamiliar with object detection and the area-perimeter method. Figures 1, 2, 3 and 4 take up too much space, and should be moved to the appendix alongside the detailed technical explanations or should be condensed into one figure to provide a concise, graphical overview of the methodology. The rest of the paper needs similar re-working to enable readers to grasp and better appreciate the extensive analysis and novel results.

Language should also be clearer and more concise. Long sentences should be shortened, split, and / or re-ordered as necessary for improved clarity. There are small but frequent language and grammar issues throughout the paper which impact the clarity of the presented study. A non-exhaustive list of examples is provided in "Technical corrections" below.

Specific comments:

There are some issues with your calculation of area and perimeter of ISSR objects.

1. The Lovejoy (1982) paper that introduced the area-perimeter relation, as well as recent work using it (e.g., Christensen & Driver, 2021; Rees et al., 2024), define it as $P \sim A^{(D/2)}$. It is confusing that you do not follow this convention in your analyses, and I am not convinced that "with Eq. (5) the dimensional relation between length and area, as well as the deviation from the usual dimension, is more obvious". To avoid confusion, I suggest you follow the conventional definition. As a result, Figure 8 and D1 should be re-plotted with area on the x-axis, and perimeter on the y-axis.

2. L. 152 implies that you are counting the perimeter of holes in the ISSR islands towards the total perimeter of the ISSR object. This leads to an overestimation of the fractal dimension. As explained by Rees et al. (2024), including the perimeter of interior holes introduces a strong resolution-dependence of your results, as interior holes tend to fill at coarser resolutions, which would not be desirable. Brinkhoff et al. (2015) study this issue in-depth and suggest removing objects that include holes. Fig. 7 suggests that a lot of ISSRs have holes, so I would not suggest removing these objects from your analyses but instead fill interior holes and do not add their circumference to the object's perimeter. This is standard practice (see e.g., Christensen et al., 2021 & Rees et al. 2024).

3. Single pixel ISSRs / ISSRs with too few pixels are not valid fractal objects. As you explain in LL. 251-252, pixels are approximated as rectangles, so these objects will be assigned dimension alpha = 2 due to the low resolution of the data. Since these measurements represent neither the area nor the perimeter of these small ISSRs correctly, they are invalid measurements of fractal dimension. Objects below a certain pixel threshold cannot be studied with your dataset and should thus be removed. This is standard practice, Brinkoff (2015) discard objects that are smaller than 15 pixels while Christensen & Driver (2021) discard objects smaller than 24 pixels; the exact cutoff for ISSRs should be chosen based on some simple sensitivity tests.

1. We now follow the conventional definition and replotted as per your request.

2. We investigated all properties both on data with filled holes (appendix) and unfilled holes. We choose to focus on unfilled data as this is a much closer representation of reality. Holes can reach considerable size, as can be seen in plots, and should thus be considered a central characteristic of ISSRs. Holes also are of major importance in the examination of pathlengths, particularly for flight paths. Our investigations are partly added to the main text and to the appendix, respectively.

3. We agree. We apply a minimum size threshold of $N \geq 24$ grid cells when defining ISSRs (see now Section 2.2.1) Therefore, single-pixel ISSRs and other very small objects are excluded from the (fractal) analysis.

Please note that the distributions, means, medians, etc. are heavily influenced by the 24 pixel threshold. The mean/median values are much larger in this reviewed version and also the distributions change in the representation of the smaller lengths.

Technical corrections:

As mentioned above, there are typos, language and grammar issues throughout the paper. I provide some examples here but suggest that the authors carefully edit their manuscript.

Please avoid colloquial terms:

- 59, 111, 126, 220 & 488, and footnote on p. 5: remove the word "actually"
- 174 & 255: remove the phrase "kind of"
- 235: remove "quite"

The colloquial language was refined.

Grammar / language:

- g. LL. 7, 180: the word "as" is used incorrectly, please change it to either "such as" or "like".
- 44-46: The logical structure of these sentences is unclear due to multiple contradictions in a row; please rephrase them.
- 50: Starting this sentence with "However" implies that the following statement contradicts the previous sentence,
- which is not the case. Please rephrase this sentence.
- 66 & 96: please remove "respectively".
- 76: please rephrase the sentence "Applying a definition . . . connected areas." as it is unclear.

All of these have been corrected.

Citations:

- I would suggest removing "see" from the citations (e.g., lines 67 and 69), as it appears unnecessary.
- I am unsure whether "cf." is used appropriately (e.g., lines 57 and 113). I recommend either removing it or verifying that it accurately conveys the intended meaning.

"see" and "cf." were removed from citations.

Conciseness

- 94: the brackets (i.e. full 11 years of data) can be removed as this is duplicate information.
- 162: please remove "One has to mention here that"
- 165 – 172: please shorten this explanation, it suffices to say that the Hausdorff dimension cannot be calculated for gridded data, include a relevant citation, and then explain the method you used.

This was implemented.

Typing errors:

- 77: “asses” -> “assess”
- 173 “two-dimension” -> “two-dimensions”
- 199 “can not” -> “cannot”
- 203 missing full stop at end of sentence
- 235: “wide spread” -> “widespread”

We corrected these typing errors.