Reviewer #1

General Comments

This article builds upon Zou et al. (2020) and Zou et al. (2021), by mostly the same authors, which I also reviewed. In their 2021 article, the authors contrasted the occurrence of stratospheric cirrus clouds seen by CALIPSO above the United States with deep convection and gravity waves activity as seen by AIRS. The article under review applies more or less the same methodology to the global scale (also updating their tropopause retrievals to ERA5 from ERA-Interim).

The article under review is quite long, includes many figures and contains interesting pieces of information. Its structure and writing are easy to follow. It is however, in my opinion, quite sprawling, and fires in too many directions. The lack of focus means this reader was often confused. Some choices of figures are not optimal, and I have one major methodological concern. Below I attempt to explain where my confusion came from and suggest ways for improvement.

Answer: Thank you very much for your time and effort in reviewing the present and our previous manuscripts. We considered all the comments and suggestions and revised the entire manuscript.

General Comments $*$ 1

I will start with what I think is a methodological problem. On line 489, you state that "clouds are labeled as deep convection if the cloud top brightness temperature is close to the tropopause temperature with an offset of 7K from the tropopause temperature". If I understand this, and the explanations of Sect. 2.3, correctly, "deep convection" then means "AIRS has observed a cloud with a top temperature close to (ie as high as) the tropopause temperature". In other words, "deep convection" is not derived from a dynamical measurement, but implied by the detection of a high-altitude cloud (taking into account the detection sensitivity of AIRS). Thus your comparison of stratospheric ice clouds (from CALIOP) with "deep convection" (from AIRS) is really a comparison of stratospheric clouds (seen by CALIOP) with high tropospheric clouds (seen by AIRS), i.e. a comparison of cloud detection sensitivities of both instruments. Differences between both retrievals will be mostly attributable to instrument strengths and weaknesses considering various cloud geometries, and not to processes that might lead to formation of stratospheric cloud (like convection). I would appreciate if you could address this point and either take it into account somehow it in your comparison, or justify why you think your comparison goes beyond a comparison of instrument sensitivities. Moreover, this issue creates a problem for the definition of deep convection. AIRS detecting a high tropospheric cloud in the tropics can indeed imply "deep convection" is occurring (as deep convection is probably the mechanism that brought the observed cloud near the tropopause), but in the midlatitudes and the polar regions convection is not required to create clouds near the tropopause. Calling AIRS high tropospheric cloud detections "deep convection" outside the tropics feels wrong to me. Considering this, you might want to change the name of "deep convection" in the paper to something else ("high tropospheric clouds" ?).

Answer: By using the brightness temperature threshold of $+7\,\mathrm{K}$ above the T_{TP}, we identified high altitude clouds from AIRS that have a significant optical thickness with tops in the upper troposphere and lower stratosphere (UTLS clouds). In the tropics, most tropopause reaching clouds with large optical thickness could be related to a deep convection origin. However, in the midlatitudes, high altitude clouds, which are thick enough to be detected by AIRS, can be also related to frontal systems (warm front uplifting), jet stream, mountain wave and contrails. We have changed the 'deep convection' to 'UTLS clouds' and clarified its definition in the manuscript.

The comparison of SICs measured by CALIPSO and UTLS clouds measured by AIRS can be affected by their detection sensitivities. However, the thick UTLS clouds detected in AIRS can well represent fresh deep convection in the tropics. Therefore, UTLS clouds are used as a proxy for deep convection and other high altitude cloud sources in this study. The term 'event frequency' is employed in our study to eliminate somehow the impacts of clouds geometries. The event frequency is the ratio of number of days in which convective system clouds (≥1 detection) occur to the total number of days in a given time period over a given region.

'In this study, temperature differences between AIRS brightness temperatures (BT_{AIRS}) and tropopause temperatures (T_{TP}) from ERA5 are employed to detect high altitude clouds in the tropics and at midlatitudes. A threshold of +7 K above T_{TP} was chosen to identify possible high altitude clouds with tops in the upper troposphere and lower stratosphere, also referred to as UTLS clouds (Zou et al., 2021). In the tropics, most tropopause-reaching clouds with large optical thickness could be related to a deep convection origin (Gettelman et al., 2002; Tzella and Legras, 2011). At midlatitudes, high altitude clouds could also be related to frontal systems (warm front uplifting), mesoscale convective systems and mesoscale convective complexes, jet stream, mountain wave and contrails (Field and Wood, 2007; Trier and Sharman, 2016; Trier et al., 2020). UTLS clouds are considered here as a proxy for deep convection in the tropics and other high altitude ice cloud sources at midlatitudes.

Next to the occurrence frequencies, the event frequency is defined in this work as the ratio of number of days in which UTLS clouds or SICs (≥1 detection) occur to the total number of days in a given time period over a given region. The event frequency helps overcome some of the limitations related to cloud geometries for UTLS clouds and SICs.'

General Comments * 2

> The second issue I have with the paper is that the subject under study is often unclear. The title mentions stratospheric ice clouds. But are we talking about clouds injected by convective overshoots in the Tropics, which are a potential pathway for stratospheric hydration? Are we talking about clouds generated through gravity wave-induced cooling over the polar regions, a subtype of polar stratospheric clouds which can also include nitric or sulfuric acid? Those objects are not the same, they do not occur in the same regions and in the same thermodynamic conditions, they do not follow from the same physical processes, they do not affect the atmosphere in the same way, they do not lead to the same scientific questions. In my view, it is not possible to study those clouds as if they were the same thing, as it is attempted here. Due to this mixing up of very different objects, the article sometimes discusses issues in regions that are irrelevant.
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> Related to this second point, I have strong concerns about how results at polar latitudes are presented together with results at lower latitudes – for instance maps of deep convection events that extend to polar regions. Given the results, I am quite convinced polar stratospheric clouds represent a non-negligible part of the dataset (maybe the whole thing) at high latitudes, even though some of them are filtered out. Presenting results that are partially representative of PSCs in maps that mainly target stratospheric ice clouds linked to deep convection I find particularly problematic. The paper also does not make it clear enough if it wants to consider PSCs as part of the dataset under study, or if it wants to keep them out of the dataset under study. Below I suggest to exclude PSCs not only from the dataset but also from the results presented. This will help the paper clarify its object of study. To fix this second issue, how I see it, the paper could 1) strive harder to eliminate PSC for the input dataset and from the presented results, 2) better explain what is meant by "deep convection" here, as it does not follow the usual convention (note that my first comment above somewhat clarifies that confusion), and 3) either simplify the analysis by removing some figures or split the article in two (maybe move the regional analyses in its own paper).

Answer: Thank you for your helpful comment. We focus only the stratospheric ice clouds in this study. The objective is to investigate the distribution and variation of SICs, and to explore potential relationships between the occurrence of SICs and atmospheric processes, i.e., atmospheric temperature, isentropic transport, stratospheric aerosols, convective system clouds, and gravity waves.

(1) We have revised all figures and results in the new manuscript and limited our analyses to the tropics and midlatitudes (±60°) to avoid interferences with unfiltered PSCs.

(2) We have changed the 'deep convection' to 'UTLS clouds' and clarified its definition in the manuscript.

(3) We have reorganized the manuscript, removed the regional analyses, and added some conclusive discussion in Sect. 3.7 in the revised manuscript.

' A filter criterion for polar stratospheric clouds (PSC) (Sassen et al., 2008), i. e., data are excluded if CTHs are higher than 12.0 km in areas with local winter latitude $\geq 60°$ N and 60° S, is utilized here to avoid possible miscounting of PSC. However, this filter criterion may not catch all low altitude PSCs. Therefore, we limited our analyses to the latitude range of ±60°.'

'In this study, temperature differences between AIRS brightness temperatures (BT_{AIRS}) and tropopause temperatures (T_{TP}) from ERA5 are employed to detect high altitude clouds in the tropics and at midlatitudes. A threshold of +7 K above T_{TP} was chosen to identify possible high altitude clouds with tops in the upper troposphere and lower stratosphere, also referred to as UTLS clouds (Zou et al., 2021). In the tropics, most tropopause-reaching clouds with large optical thickness could be related to a deep convection origin (Gettelman et al., 2002; Tzella and Legras, 2011). At midlatitudes, high altitude clouds could also be related to frontal systems (warm front uplifting), mesoscale convective systems and mesoscale convective complexes, jet stream, mountain wave and contrails (Field and Wood, 2007; Trier and Sharman, 2016; Trier et al., 2020). UTLS clouds are considered here as a proxy for deep convection in the tropics and other high altitude ice cloud sources at midlatitudes. '

General Comments ∗ 3

> The third issue I have with the paper is that it investigates the role of many processes (deep convection, gravity waves, stratospheric aerosols loading from eruptions biomass burning) in the formation of stratospheric ice clouds, but never attempts to summarize its findings in an integrated view, that would for instance rank the importance of each of these processes spatially or temporally. No attempt is made to provide a theoretical framework that would justify why stratospheric clouds are more frequent when a given process is more frequent in a given region or period, and why they are more frequent when another process is more frequent in another region or period. The many figures often plot the evolution of a property of stratospheric cloud against the evolution of another value representative of a process. Helped by the text, we see when/where both values are more correlated, and when/where they are less correlated, but as readers, we are just left with correlation coefficients, with no improved understanding of what is going on. This problem is somehow compounded by the first issue – investigating so many subjects makes it more obvious that no attempt is made to bring them all into a cohesive whole. Making the article(s) more focused would make it easier to integrate findings into a larger context.

Answer: Thank you for this comment. We have reorganized and revised the entire manuscript. This study aims to understand the relationships between SIC occurrences and related processes, e.g., tropopause temperature, double tropopauses, UTLS clouds and stratospheric aerosols.

We investigated the individual and combined relationships between different processes and SIC occurrence. 'Correlation coefficient and long-term anomaly analyses of SICs and all the other processes indicate that the occurrence and variability of SICs are mainly associated with the tropopause temperature in the tropics. UTLS clouds have the highest correlations with SICs in the monsoon regions and the central United States. Tropopause temperature and gravity waves are mostly related to SICs at midlatitudes, especially over Patagonia and the Drake Passage. However, besides the highest correlation coefficients, the cold tropopause temperature, the occurrence of double tropopauses, high stratospheric aerosol loading, frequent UTLS clouds and gravity waves all have high correlations with the SICs. The occurrence and variability of SICs demonstrate a strong dependence on various processes, both locally and temporally.

The overlapping and similar correlation coefficients between SICs and all processes indicate strong associations between all processes themselves. Due to their high inherent correlations, it is challenging to disentangle and evaluate their contributions to the occurrence of SICs on a global scale. However, the correlation coefficient analyses between SICs and all processes and high associations between all processes observed in this study help us better understand the sources of SICs on a global scale.'

Specific Comments:

1. l. 13-20: The past time makes it unclear who did the things explained. "Relations... were analyzed" – analyzed by who?

Fixed.

2. l. 47: "For example, 7 % of observations..." in your 2021 paper, the number given was 2.5 %. Which is correct?

Both numbers are correct. I should have been more specific. '7 % of observed clouds (Noël and Haeffelin, 2007, Table. 1) and 2.5 % of observations (Noël and Haeffelin, 2007, Fig. 3)'. This sentence has been removed from the manuscript due to the revision of this paragraph.

3. l. 148: "in previous studies..." this sentence suggests that the cited articles used ERA-Interim to derive the tropopause altitude. Please mention it explicitely. Also: it's unclear to me why a 2x resolution improvement means the threshold for tropopause can also be cut in half. Other considerations than vertical resolution influence the accuracy of the retrieved tropopause, and the distance you wish to impose on that tropopause to make sure one is in the stratosphere. Would ERA6 improve the vertical resolution 10x, you would still need to consider a larger threshold to account for other sources of uncertainty.

Sorry for the confusing text, the 2x resolution improvement coincides with the 250 m. The 250 m is set as the tropopause threshold because the tropopause height differences between ERA5 and radiosonde and GPS are about 200 m. We have revised the manuscript.

'Tegtmeier et al. (2020a) found that LRT1 height differences between ERA5 and Global Navigation Satellite System-Radio Occultation observations are less than 200 m in the tropics. Based on US High Vertical Resolution Radiosonde Data (HVRRD) data and coarser-resolution Global Positioning System (GPS) data, Hoffmann and Spang (2022) also showed that the uncertainty of the LRT1 heights of ERA5 is in the range of ±200 m at different latitudes. Therefore, a height difference of 250 m with respect to the tropopause is used as threshold for ERA5 data to identify stratospheric ice clouds in this study. '

4. Sect. 2.2: in this section, you explain how from the CALIPSO VFM product you use cloud data, but also aerosol data. I must admit I was confused at that point since I had missed that one of the objectives of the paper was to contrast the presence of stratospheric ice clouds with the occurrence of stratospheric aerosols. As far as I can tell, the paper's objectives are only explained in the sentence l. 122-124. It might be good to expand a bit this sentence to make sure other readers will not miss it.

Done. Aerosols are effective ice nuclei for cirrus cloud formation and variation. Therefore, stratospheric aerosols are investigated in this study to explore their potential impacts on the occurrence of SICs.

'The objectives of this study are to 1) examine the distribution and long-term variation of stratospheric ice clouds and 2) investigate potential effects of atmospheric temperature, stratospheric aerosols, UTLS clouds, and gravity waves on the occurrence and distribution of SICs on a global scale.'

'As aerosol particles provide cloud condensation nuclei and ice nuclei, the occurrence of SICs is expected to correlate with aerosols (Lohmann and Feichter, 2005).'

5. l. 163-165: I don't follow the reasoning that justifies why daytime and nighttime data are used for aerosols but only nighttime data for ice clouds. Why should "aerosols are long-lived" justify using both daytime and nighttime data? Why is increased nighttime SNR (=improved detection abilities) a good reason to use only nighttime data for ice clouds, but not for aerosols? Will not this difference in datasets influence somehow the comparisons between stratospheric clouds and stratospheric aerosols? Please clarify your reasons why keeping only the nighttime data for clouds and using both daytime and nighttime data for aerosols.

To ensure the consistency of data, we have removed the daytime aerosol data from the manuscript.

6. l. 169: PSCs often reach latitudes lower than 60°. In the north hemisphere, they were observed as far down as the Mediterranean sea : https://doi.org/10.5194/acp-7-5275-2007 I'm afraid your criteria to exclude PSCs will still lead to a large presence of PSCs in your stratospheric cloud dataset, and the presented results suggest this to be true. I suggest that PSC *are* stratospheric clouds, and the distinction you're trying to make here between PSC and other stratospheric ice clouds is not really possible – above a given latitude, all stratospheric clouds are PSC, by definition. But not all PSCs are ice. It is unclear to me if the paper wants to include ice PSCs within the boundaries of the dataset under study, or not. Limiting the geographic scale of the study, for instance to only show latitudes below 60°, would put a limit on the importance of PSCs in the dataset considered, and in the results presented.

Thank you. The PSCs are not supposed to be investigated in this study. Even though the filter criteria were used to filter polar stratospheric clouds, some low-altitude PSCs were still obtained in the data. In the revised manuscript, we have limited the analyses to the tropics and midlatitudes (±60°).

7. l. 171: "SICs detected at high latitudes will not be discussed in detail in this work". This is good, but all your maps still show latitudes and results above 60°. For example, the same map will mix stratospheric ice clouds along with polar stratospheric ice clouds. However, at high latitudes, only parts of the observed PSCs are shown, given the filtering described on l. 169. Thus what global maps show at high latitudes is neither representative of convection-based ice clouds, nor of polar stratospheric clouds, and I'm concerned these figures could at a glance be misinterpreted by a too-quick reader. I would appreciate if high latitudes, where the dataset under study is probably dominated by PSCs but omits an undefined amount of them, where hidden from the global maps.

We have limited the analyses to the tropics and midlatitudes (±60°). All data and figures are revised in the new manuscript.

8. Sect. 2.3: Many parts of this section are very similar to section 2.3 of Zou et al. 2021 (a section with more or less the same name). I've found at least one sentence that is exactly the same. Please revise this part to see if you could perhaps just reference the previous paper.

We have revised this section. Please find revisions in the new manuscript.

9. 181: could you please be more specific when you reference the 7 articles here? ie cite each paper separately when it is most relevant.

Done. We have separated them into three subsections with corresponding references.

'Aumann et al. (2006) and Aumann et al. (2011) retrieved deep convective clouds from AIRS at 8.1 μm (the 1231 cm^{-1} atmospheric window channel) in the tropics. The term "deep convective clouds" in their studies refers to clouds tops of thunderstorms in non-polar regions with a brightness temperature (BT) of less than 210 K. When the top of anvil of thunderstorms has a brightness temperature of less than 210 K, the deep convective clouds are considered to reach the tropopause region in the tropics (Aumann et al., 2006). However, the threshold of 210 K is too low for midlatitude convective events (Hoffmann and Alexander, 2010), and a constant brightness temperature threshold for convective event detection may produce ambiguous results at different latitudes and seasons (Hoffmann et al., 2013).'

'In this study, mean variances of detrended brightness temperatures in the 4.3 μm carbon dioxide waveband are used to identify stratospheric gravity waves from AIRS observations (Hoffmann and Alexander, 2010; Hoffmann et al., 2013). Measurements of 42 AIRS channels from 2322.6 to 2345.9 cm^{-1} and 2352.5 to 2366.9 cm^{-1} are averaged to reduce noise and improve the detection sensitivity of the gravity wave observations. Even though the AIRS observations have the highest sensitivity at an altitude range of 30 – 40 km (Hoffmann and Alexander, 2010; Hoffmann et al., 2013, 2018), the averaged BT variance can provide gravity wave information for the lower stratosphere as gravity waves typically propagate upward from the tropospheric sources into the stratosphere.'

'As brightness temperature differences are an effective method to detect volcanic SO_2 from AIRS observations (Hoffmann et al., 2014b, 2016), spectral features of SO_2 at 1407.2 cm^{-1} and 1371.5 cm^{-1} are used to calculate the SO_2 Index (SI)'

10. l. 221: "lower stratospheric ice clouds": What does "lower" mean here? I don't think low stratosphere and high stratosphere were defined so far in the paper.

Revised. 'Global stratospheric ice clouds'

11. l. 222: "Ice clouds with cloud top heights at least 250m above the first tropopause were defined as SICs" I think this has already been explained.

We have removed this sentence from the manuscript.

12. l. 248: "The occurrence of double tropopauses in general greatly impacts the SICs' occurrences associated with double tropopauses. " I'm not sure I understand this sentence. As I read it, I think it means that when there is no double tropopause, there are no SIC associated with a double tropopause? Please clarify.

We have revised this subsection. This sentence is removed from the manuscript.

13. Figure 1. These figures show SICs are quite frequent over the Antarctic Peninsula in all seasons except DJF (Antarctic summertime). This supports the idea that the SIC dataset includes a non-negligible part of PSCs.

We have limited the analyses to the tropics and midlatitudes (±60°). All data and figures are revised in the new manuscript.

14. l. 258: thermal tropopauses are notably hard to retrieve over the polar regions, where the temperature gradient gets mostly flat in the stratosphere. It is not clear to me that retrievals of multiple tropopauses in those regions are neither reliable nor meaningful. What is the meaning of mutiple tropopauses when the temperature profile is flat?

We have excluded high latitude data in the revised manuscript.

15. Figure 2: In this figure, I doubt the relevance of multiple tropopauses that appear above 60°N and above 60°S. See previous comment.

We have excluded high latitude data in the manuscript.

16. Figure 3: This figure is very pretty, but it does not support the discussion very well. The text discusses the differences between years, and the average yearly evolution of the SIC frequencies. This discussion would be better supported by showing the average yearly evolution of SIC occurrence (ie the same Hovmoller plot as Figure 3 but for months averaged over 2007-2019), and maybe in addition a plot showing the monthly anomalies of SICs occurrence averaged over the ±20° region (I'm not sure the discussion discusses the small variations that occur outside of this latitude range).

We have revised the corresponding text for this figure. We want to present the seasonal cycles and inter-annual variability of SICs over different latitude bands using a Hovmöller diagram. We can easily see the variation of SICs over latitude ranges and the time series from a Hovmöller figure.

'To investigate spatial and temporal variations of SICs, monthly averaged occurrence frequencies of SICs in 5° latitude bands from 2007 to 2019 are shown in Fig.2. Seasonal cycles and inter-annual variability of SICs are observed in the tropics and at midlatitudes. SICs in the tropics follow the Intertropical Convergence Zone (ITCZ) over time, i.e., high SIC frequencies in the latitude range of 20° S-20° N move from south to north from boreal winter to summer and north to south from boreal summer to winter. The correlation with the ITCZ suggests that there is a strong correlation with deep convection. Most SICs are observed between 15° S-5° N, which show higher SIC occurrence frequencies (>0.24) and longer occurrence times (November to March of the following year). The SIC frequencies are stronger in the SH tropics, whereas SICs extend to higher latitudes in the Northern Hemisphere. Some SICs are identified at 25° N-30° N from June to August, which are absent in the Southern Hemisphere, which would relate to the uplift of the Tibetan Plateau and the Asian Monsoon region.

At midlatitudes, the frequencies of SICs at midlatitudes are at least twice as low as in the tropics. However, we can still notice an inter-annual variation of SICs at midlatitudes, where SICs are more often observed in winters/early springs. It suggests other sources for the occurrence of SICs at midlatitudes besides deep convection. Therefore, we investigate the correlation of different processes with respect to SIC occurrences in the following sections, including tropopause temperature, double tropopauses, UTLS clouds, gravity waves, and stratospheric aerosol, which are expected to have an impact on cloud formation. '

17. Section 2.3: I find this section quite confusing. It does not include the easiest way to look for a

correlation: a scatterplot. Why not plot first the frequencies of SIC in 5x10° cells against the average tropopause temperature in the same cells? This would let you first conclude on the existence of a correlation between both quantities. Then figure 5 would let you identify regions where the correlation is positive and where it is negative, and finally figure 6 would let you identify possible variations of these correlation signs in time.

The manuscript has been restructured. The seasonal maps of LRT1 temperature and SIC occurrence frequencies are presented in Sect. 3.3 to indicate their possible correlations. A scatter plot could show a correlation, but it may miss the seasonal dependency in the tropics as explained in the text. The correlation coefficients between processes and SIC occurrence frequencies and long-term spatial variability of SICs and all processes are presented in Sect. 3.7 to investigate the relationships between all processes and SICs.

18. l. 289: I understand that a positive correlation means that SIC are more frequent when the tropopause is warm, and less frequent when the tropopause is cold. If that is your understanding too, could you propose some kind of explanation or process responsible for positive correlation? This result is clearly at odds with the findings of the articles you cite, and cannot go by unadressed.

We have revised the text and extended the discussion. The correlation coefficients between the four processes and the SIC frequencies are analyzed together in Sect. 3.7. The positive correlation coefficients between SICs and LRT1 temperature are found in the North American Monsoon and the Asian Monsoon, with high positive correlation coefficients between convective system clouds, gravity waves, and stratospheric aerosols and SICs. The high SIC frequencies can be produced by ice and water vapor injection from convective systems, air cooling induced by gravity waves, and more ice nuclei from stratospheric aerosols when the tropopause is warm.

'While the LRT1-T shows a general negative correlation, there are strong positive correlations over central America and the Caribbean Sea, Philippines and South Chinese Sea, and the Tibetan Plateau to the Caspian Sea.'

'The inherent correlations between all processes may help to explain the positive correlations between SICs and LRT1-T in the North American Monsoon and the Asian Monsoon regions. Even if the tropopause temperature is warm, UTLS clouds, gravity waves, and stratospheric aerosol could all contribute to the high occurrence frequency of SICs. For example, Fu et al. (2006) discovered that deep convection in the Asian Monsoon injected more ice and water vapor into the stratosphere with warmer tropopause temperatures. However, their strong correlation also makes it challenging to disentangle all processes' effects on the occurrence of SICs.'

'However, tropopause temperatures cannot explain some remarkable positive anomalies in SIC frequencies. For example, high SICs in November 2010 to January 2011, December 2011, March 2014, and April-May 2018 over the equator and high SIC anomalies in April-July 2011 at 5°N-20°N. We need to note that the cold temperature as well as the cooling of the atmosphere (Kim et al., 2016) are important for the variation of SICs. And the uplifting motions, gravity waves, the El Niño-Southern Oscillation (ENSO) and quasi-biennial oscillation (QBO) and potentially other effects would all impact the temperature and temperature variations (Abhik et al., 2019; Feng and Lin, 2019; Tegtmeier et al., 2020b) associated with SIC variability. '

19. figure 5: When first reading section 3.4, only figure 5a is discussed. The other three figures are discussed further down. This is unusual and should be adressed somehow in the text or the legend.

We have reorganized the manuscript. The figure was moved to a new subsection, and all subplots were discussed together. Please find revisions in Sect. 3.7.

20. l. 296-300: providing pseudocode is not required for such a simple operation. The explanation lines 294-295 is enough.

The pseudocode has been removed.

21. figure 6: like with figure 3, I'm not sure the plot supports the discussion in an optimal way. The anomalies in SIC frequency shown in Figure 6A are very weak almost everywhere except in the ±20° band – most of the plot is not useful. In my view it would be much more readable to provide simpler line plots that describe the average anomaly in different latitude bands – for example one line for

±20°, one for > 20°N and one for < 20°S. The same quantity of information would be offered, and the correlation/anticorrelation with the temperature anomalies would be much easier to see.

We have adjusted the color palette of the Hovmöller diagrams and reorganized them in Fig. 12. The Hovmöller figures are still used in our manuscript because we think they can give more detailed information about the latitude and time range of the anomalies, which helps us to better understand the related processes. Discussions on Hovmöller figures have been extended in the revised manuscript.

'To explain the tempo-spatial variation of SICs, monthly SIC frequencies and all processes at different latitude bands (5° for each band) from 2007 to 2019 are presented in Fig. 12. The monthly anomalies for each band were computed as the difference between the monthly zonal mean values and the interannual mean of the monthly zonal mean values, which excludes seasonal cycles of parameters. The regionally averaged monthly anomalies of SIC frequencies and all processes with seasonal cycles over the tropics (20° S-20° N), northern midlatitudes (40° N-60° N) and southern midlatitude (40° S-60° S) can be found in Appendix.D.

For global-scale anomalies excluding the effect of seasonal cycles, significant anomalies in SIC frequency can be observed in the tropics. Anomalies of SIC frequencies at ±20° are generally demonstrating contrary features to the LRT1 temperature. For instance, negative anomalies of SICs in February 2007 to July 2007, November 2009 to January 2010, October 2013 to June 2014 (excluding March 2014), and October 2015 to January 2016, January-August 2017, November-December 2019 are compatible with positive LRT1 temperature anomalies. And positive SIC anomalies in January-June 2008, January 2013, June-July 2015, June-December 2016, October 2018 to February 2019 are co-located with negative LRT1 temperature anomalies. During those periods, tropopause temperature variations are important for the anomalous variability of SICs in the tropics.

However, tropopause temperatures cannot explain some remarkable positive anomalies in SIC frequencies. For example, high SICs in November 2010 to January 2011, December 2011, March 2014, and April-May 2018 over the equator and high SIC anomalies in April-July 2011 at 5°N-20°N. We need to note that the cold temperature as well as the cooling of the atmosphere (Kim et al., 2016) are important for the variation of SICs. And the uplifting motions, gravity waves, the El Niño-Southern Oscillation (ENSO) and quasi-biennial oscillation (QBO) and potentially other effects would all impact the temperature and temperature variations (Abhik et al., 2019; Feng and Lin, 2019; Tegtmeier et al., 2020b) associated with SIC variability.

Stratospheric aerosols, UTLS clouds and gravity waves are further analyzed to understand those anomalous SICs. Enhanced stratospheric aerosols due to volcanic eruptions coincide with the high SIC frequencies at 25°S-10°N in November 2010 to January 2011 (Merapi volcano), 5°N-20°N in April-July 2011 (Nabro volcano), 15°S-10°N in March 2014 (Mt. Kelud volcano), 15°S-20°N in April-May 2018 (Ambae volcano) (Global Volcanism Program, 2013; Hoffmann, 2021). In the extra-tropics, the most pronounced positive anomalies in SIC frequency correlate with the ash rich volcanic eruptions of Kasatochi (August 2008, 52° N), Puyehue-Cordón Caulle (June 2011, 41° S), Calbuco in April-May 2015 (41° S), and Raikoke (June 2019, 48° N) (compare with AIRS ash and SO_2 index Hoffmann, 2021). High SIC frequencies around 40° N in January-March 2011 and from December 2012 to January 2013 are co-occurring with high anomalies of UTLS clouds and gravity waves. The tempo-spatial analyses of LRT1 temperature, UTLS clouds, gravity waves and stratospheric aerosols provide explicit awareness of processes on the occurrence and variability of SICs at different latitude bands and time ranges.'

22. l. 307-308: "tropopause temperatures are negatively correlated with the occurrence of SIC... especially over tropical continents" you already concluded that from figure 5. Compared to figure 5, figure 6 adds the time periods and latitude bands where the correlation was positive and where it was negative. This is what should be discussed when focusing on this figure.

We have reorganized the Hovmöller diagrams in Fig. 12. The Hovmöller figures are discussed in more detail in the revised manuscript. Please see the answer to Point 21.

23. l. 314: In my understanding, deep convection is triggered by strong sunlight over water vapor-saturated areas, and occurs primarily in the tropics, especially in the warm pool and over the ITCZ – see for instance https://doi.org/10.1016/j.atmosres.2020.105244 that provides an example of such a definition. "Deep convection" then means convection that is triggered near the surface and generates

vertical motion all the way up to the tropopause (hence the ”deep”). In your plots (figure 7), the warm pool appears as a minor hotspot of deep convection, and the ITCZ is not really visible. Moreover, your plots suggest that deep convection is very frequent over, for example, the northern Atlantic (30°N-70°N) – all during the cold, sun-deprived wintertime (DJF). I find all this very puzzling, and would appreciate if you could clarify the meaning of ”deep convection” that you support in this article. Could you perhaps cite articles that support this definition? (note that my first major comment somewhat fixes that confusion, by clarifying that ”deep convection” in the text really means ”a cloud observed above the tropopause by AIRS”)

We have changed the ’deep convection’ to ’UTLS clouds’ in the new manuscript. UTLS clouds retrieved from AIRS are used as a proxy for deep convection and other high altitude cloud sources in this study. UTLS clouds include clouds from tropical storms and strong convective events from numerous sources, such as storm systems and fronts, mesoscale convective systems, and mesoscale convective complexes in midlatitudes and high latitudes. Please see the answer to General Comment 1.

24. section 3.4: Figure 9 clearly shows that the gravity waves considered in the present article (and derived from AIRS) are mostly related to the presence of the polar vortex – polar gravity waves above 60N in DJF and above 60S in JJA dominate the figures. There is some limited GW activity visible in the tropics, for instance in DJF, but it is clearly minor compared to the polar regions. Gravity waves have been known to trigger PSC formation above mountains for quite some time, especially ice PSCs (see for instance https://doi.org/10.5194/acp-4-1149-2004). Trying to relate this GW activity with SICs will naturally lead to results dominated by PSCs. Again, it is a major problem to me that your paper does not address the confusion between stratospheric ice clouds in the Tropics (that can be generated by deep convection) and stratospheric ice clouds in the polar regions. PSCs are related to the polar vortex and are only partially represented in your dataset, both intentionally (as you filtered out some of them on purpose) and unintentionally (PSCs are optically thin and require specific detection and identification techniques). Given the object of study of your paper, I think it is necessary to make stronger efforts to exclude PSCs from the input dataset but also from the presentation of the results.

Thank you. To avoid the possible mixing of PSCs and SICs, we have excluded high latitude data in the new manuscript. Please see the answer to General Comment 2.

25. l. 385: Figure 10 is in my opinion way too busy to provide the basis for a reliable visual identification of correlations between stratospheric aerosols related to eruptions and SIC. It is too easy in my opinion to visually miss important features. Like with figures 3 and 6, the use of an Hovmoller plot is overkill and your interpretation would be much better served by zonal average plots.

We have revised this figure. For example, the high latitude data is removed from the figure, zonal mean information for the tropics and midlatitudes is added, the color palette is adjusted to proper ranges, and only strong volcanic eruptions are listed in the new plot. Please see Fig. 12. The Hovmöller plots are still used in the new manuscript because the latitude and color-palette optimized figures present more detailed information.

26. l. 413: Reverdy et al. 2012 do not use the CALIOP level 2 product that is used in the present article. Please clarify what misclassifications you imply, supported by more relevant references.

We have revised this subsection and added discussion on the ’misclassification’.

’The eruption of Puyehue-Cordón Caulle is known to have injected significant amounts of volcanic ash (Klüser et al., 2013; Hoffmann et al., 2014a). Moreover, Klüser et al. (2013) show that ”the ash plume is transported very close to and potentially partly within or beneath ice clouds”. In such a case the CALIOP ”cloud fringe amelioration” algorithm might rather classify these detections as ice clouds instead of aerosol (Liu et al., 2019). Moreover, Liu et al. (2019) point out that the aerosol cloud classification for this volcanic plume was particularly challenging due to the dense and depolarizing aerosol.’

As volcanic aerosol is known to induce ice cloud formation and although the MIPAS data is more noisy and also shows some (not discussed) significant anomalies, which are not present in the CALIOP data, we consider the analysis of positive correlations between SICs and aerosol requires a more in-depths investigation to separate causal correlations from potential misclassifications in CALIPSO data.’

27. l. 428: ”three regions...” how or why were these three regions selected? What makes them special a

priori? Why aren't the other regions worthy of their own correlation plots? Besides, it is unclear to me what the three full-page figures 13, 14 and 15 bring to our understanding of what drives the evolution of SIC. The figures-to-text ratio (3 pages of figures for less than one page of text) is off here. The text l.431-459 discusses when SIC occurrence is most correlated with a given metric and when it is correlated with another metric. Maybe only the metrics with an average correlation coefficient above a given threshold could be shown, or figures 13 to 15 could be moved to an appendix.

We reorganized the structure and the regional analyses have been removed from the manuscript.

28. l. 473 "solidly": Do you mean here that your analysis was good?

We have revised this paragraph. 'In this study, a tropopause threshold of 250 m was applied to identify stratospheric ice clouds and stratospheric aerosols. As mentioned in Sect. 2.1, the vertical resolution of tropopause heights in ERA5 was improved by applying a cubic spline interpolation method (Hoffmann and Spang, 2022). When compared to radiosonde and GPS data, the height uncertainty for the LRT1 in ERA5 is less than 200 m (Tegtmeier et al., 2020a; Hoffmann and Spang, 2022).'

29. l.473 "250m is a reasonable tropopause threshold..." again, I do not find this argument convincing.

We have clarified the selection of tropopause threshold in the new manuscript. The vertical resolution of tropopause heights in ERA5 is improved after interpolating with cubic spline interpolation method (Hoffmann and Spang, 2022). And the height uncertainty for the LRT1 in ERA5 is less than 200 m compared to radiosonde and GPS data (Tegtmeier et al., 2020a; Hoffmann and Spang, 2022). Therefore, 250 m is used as the threshold to identify the stratospheric ice clouds. Please see answer to specific comment no. 3 and no. 28.

30. Section 4.2: see my first major point.

We have changed 'deep convection' to 'UTLS clouds'. 'In this study, temperature differences between AIRS brightness temperatures (BT_{AIRS}) and tropopause temperatures (T_{TP}) from ERA5 are employed to detect high altitude clouds in the tropics and at midlatitudes. A threshold of +7 K above T_{TP} was chosen to identify possible high altitude clouds with tops in the upper troposphere and lower stratosphere, also referred to as UTLS clouds (Zou et al., 2021). In the tropics, most tropopause-reaching clouds with large optical thickness could be related to a deep convection origin (Gettelman et al., 2002; Tzella and Legras, 2011). At midlatitudes, high altitude clouds could also be related to frontal systems (warm front uplifting), mesoscale convective systems and mesoscale convective complexes, jet stream, mountain wave and contrails (Field and Wood, 2007; Trier and Sharman, 2016; Trier et al., 2020). UTLS clouds are considered here as a proxy for deep convection in the tropics and other high altitude ice cloud sources at midlatitudes.'

31. l. 494-499: I think this has already been explained in lines 194-199.

It is now removed from the manuscript.

32. Figure 16: same remark as before on the limited usefulness of Hovmoller plots

We revised the Hovmöller plots in Fig.12. The Hovmöller plots could present more detailed information than regional averaged linear plots after optimizing latitude range and color palette. Please see answer to specific comment no. 21.

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