

Atmos. Chem. Phys. Discuss., referee comment RC2  
<https://doi.org/10.5194/acp-2021-1038-RC2>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on acp-2021-1038

Anonymous Referee #1

---

Referee comment on "A global view on stratospheric ice clouds: assessment of processes related to their occurrence based on satellite observations" by Ling Zou et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1038-RC2>, 2022

---

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.

### Major comments

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 section 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 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" ?).

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.

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).

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.

## Minor comments

- l. 13-20: The past time makes it unclear who did the things explained. "Relations... were analyzed" -- analyzed by who?
- l. 47: "For example, 7% of observations..." in your 2021 paper, the number given was 2.5%. Which is correct?
- l. 148: "in previous studies..." this sentence suggests that the cited articles used ERA-Interim to derive the tropopause altitude. Please mention it explicitly. 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.
- 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.
- 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.
- 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.

- 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, were hidden from the global maps.
- 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.
- l. 181: could you please be more specific when you reference the 7 articles here? ie cite each paper separately when it is most relevant.
- 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.
- 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.
- 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.
- 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.
- 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 multiple tropopauses when the temperature profile is flat?
- Figure 2: In this figure, I doubt the relevance of multiple tropopauses that appear above 60°N and above 60°S. See previous comment.
- 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  $\pm 20^\circ$  region (I'm not sure the discussion discusses the small variations that occur outside of this latitude range).
- 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  $5 \times 10^\circ$  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.
- 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 unaddressed.
- 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 addressed somehow in the text or the legend.

- l. 296-300: providing pseudocode is not required for such a simple operation. The explanation lines 294-295 is enough.
- 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  $\pm 20^\circ$  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  $\pm 20^\circ$ , one for  $> 20^\circ\text{N}$  and one for  $< 20^\circ\text{S}$ . The same quantity of information would be offered, and the correlation/anticorrelation with the temperature anomalies would be much easier to see.
- 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.
- 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 ( $30^\circ\text{N}$ - $70^\circ\text{N}$ ) -- 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")
- 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  $60^\circ\text{N}$  in DJF and above  $60^\circ\text{S}$  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.
- 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.
- 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.
- 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.

- l. 473 "solidly": Do you mean here that your analysis was good?
- l.473 "250m is a reasonable tropopause threshold..." again, I do not find this argument convincing.
- Section 4.2: see my first major point.
- l. 494-499: I think this has already been explained in lines 194-199.
- Figure 16: same remark as before on the limited usefulness of Hovmoller plots