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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the origin of subvisible cirrus clouds in the tropical upper troposphere" *by* M. Reverdy et al.

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Major comments

1. PSC inclusion

The Reviewer points out that the paper devotes a large section to the study of PSC as a possible class of SVC. He notes that since our conclusions suggest a very limited presence of such clouds (0.34%), we devote too much space to this question. He also notes that we do not bring solid elements to this problem, although he does not point to any specific problem neither suggest possible improvements.

First of all, we want to point out that we do not propose that some SVC are actually PSC. We suggest that nucleation mechanisms that occur during PSC formation could

be triggered in TTL conditions that match the polar stratosphere temperature-wise. These nucleation pathways would lead to unusual crystal composition including HNO3 molecules, as happens in PSC, but might eventually produce different particles from those observed in PSC. This is why we were careful to label such particles as "NAT-like" and not NAT. Second, the reviewer is right that our results only support the possibility of a very limited amount of SVC containing PSC-like particles, a conclusion we clearly state in the paper. However, we do not agree that this result means we devote too much space to this avenue of research: one of our goals in this paper is to actually quantify which processes are significant for SVC formation amongst the ones proposed, and before engaging in the study of PSC-like particles in SVC we had only a very limited idea of the importance of this process for the whole population of SVC. It's true that our results show that PSC-like particles concern only a small minority of SVCs, but without the analysis we wouldn't know that. However, we agree with the Reviewer that our results report such a small number of SVC supporting a NAT-like particle formation that it can hardly be considered statistically significant. Our conclusions now clearly support this view, and explain with more certainty that the nucleation of NAT-like particles is a process that has no significance for SVC formation (Sect. 5.1 and 6).

2. Local increase of aerosol loading

Here the Reviewer finds the discussion related to the importance of aerosol enhancement as a source of ice nuclei to be poorly explored. Following his remarks, we reorganised this section and analyzed more data to hopefully better describe the impact of volcanic activity on the TTL aerosol loading during the entire period under study (see below). In the first part of his comment, the Reviewer mentions how we need to differentiate between eruptions that resulted in an actual observable increase of aerosols in the TTL (e.g. the Tavurvur and Jebel-Al-tair eruptions) and those that did not (Nyiragongo and Hawaii). Following this comment, we reorganised the section to hopefully present a more comprehensive and better-structured view of volcanic activity during the studied period (june 2006-december 2008). Instead of presenting two example maps of SO2 during a single eruption and discussing four eruptions, we now attempt to document all eruptions affecting the UTLS SO2 concentration levels during this period through column-integrated SO2 time series (from OMI) in three equally wide bands of latitude (30°S to 10°S, 10°S to 10°N, 10°N to 30°N). We confront these measurements with time series of scattering ratio between 19 and 21 km (from CALIOP) to document coincident changes in aerosol loading in the TTL. We switched from using backscatter time series to scattering ratio, as it is less susceptible to changes affecting the molecular backscatter (like calibration issues). We attempted to clearly identify when eruptions occurred along the various time series. In the article, we now present these two time series to document how eruptions during the period of study affect the levels of SO2 and sulfate aerosol concentrations in the UTLS. Moreover, we now describe in the text, in the new Table 4 and directly on Fig. 12 which eruptions led to direct injection of SO2 in the TTL and those that did not. We hope these new information help making our study more conclusive on the possible relationship between eruptions and SVC formation. On the same topic, see also our answer to the specific comments about Section 5.2 below.

In the second part of his comment, the Reviewer finds the influence of biomass burning on SVC formation to be poorly explored. The original article only took a “brief look” at this problem by presenting a single map of average NO2 observed by OMI in February 2007. The Reviewer suggests using instead carbon monoxyde measurements from MLS as a better tracer of soot particles. Following up on this suggestion, we now present time series of carbon monoxyde measured between the 140 and 215 hPa levels by MLS in the same bands of latitude described above. We present these time series consistently with the way we now present time series of SVC cloud cover, of SO2 injection due to eruptions and TTL aerosol loading (see previous paragraph and answer to specific comments below). We discuss the new results in a new section devoted to the influence of biomass burning (5.2.3), and we now discuss eruptions in Sect. 5.2.1 and the Asian Monsoon Aerosol layer in Sect. 5.2.2 We hope these new results document better how the influence of biomass burning on atmospheric

composition changed with time during the period of study, and how this phenomena relates to SVC formation.

3. Influence of convection

The Reviewer notes that our results linking SVC formation to convection only describe C1 SVC detected over Africa during DJF (actually JJA in the original article), which are only a small part of the SVC population first described in the article. This remark echoes Reviewer #2’s first comment. Indeed, in this section of the original article, we made the choice to restrict our analysis to SVC observed over Africa. Our initial choice was motivated by the limited area covered by the brightness temperature maps derived from METEOSAT imagery, and the difficulty to merge temperatures measured from other geostationary satellites. Moreover, we restricted the analysis to SVC detected in the 2006 JJA season, while the rest of the article describes both DJF and JJA seasons between 2006 and 2008. The Reviewer suggest that we should instead investigate the SVC-convection relationship globally and not only around Africa.

We agree with the Reviewer that these restrictions put in serious question the generality of the results presented in this section. Following this remark, we elected to use, instead of the Meteosat maps, the MERG NCEP/CPC brightness temperature dataset that combines measurements from several geostationary satellites (GOES-8/10, METEOSAT-7/5 and GMS). This merged dataset documents brightness temperatures on a global scale (±60°) with a ∼4km resolution hourly since February 2000, and thus includes the entire tropical belt under study. Using this dataset, we extended our study linking SVC formation and convection to the entire C1 SVC population. Due to these changes, we now consider ten times more SVC (744) and back-trajectories (∼7000) than in our original paper. As a nice side effect, our analysis also benefited from the increased time and space resolutions of the MERG dataset. Moreover, we extended the analysis to cover both DJF and JJA seasons and include all such seasons between June 2006 and December 2008 instead of only 2006. Using global BT maps also let us follow back-trajectories further in time (since in our original analysis they often escaped single regions as defined in Table 1 after 5 days), and we now extend our search of intersection between back-trajectories and convective systems up to 15 days prior to SVC detection. Following these changes, we modified Section 5.3 of the present article to include a description of the new brightness temperature merged dataset, and of the new results obtained considering the tropical belt as a whole. Fig. 15-17 (previously 16-18) have been updated to take the new results into account.

Finally, in the same remark, the Reviewer also points out that our abstract implied that the results presented in this section, describing SVC over Africa, were in fact relevant to the entire population of SVC. This was definitely a mistake on our part and we thank the Reviewer for noticing it. The text of the abstract has been modified to reflect the changes described above and the new results obtained.

Specific comments

Section 2. The Reviewer suggest a change to the text that would clarify its meaning. Following this remark, the text has been modified accordingly. The Reviewer points out that Fig. 2 would benefit from an increase in character size. Following this remark, the text has been enlarged in Fig. 2.

Section 3. The Reviewer points out that Fig. 4 and 6 would benefit from an increase in character size. Following this remark, the text has been enlarged in Fig. 4 and 6. The Reviewer requests a clarification of text beginning with “The associated...” located on line 15 of page 14884 (Sect. 3.2). On page 14884 we could only find the highlighted text on line 4. We assumed this was the sentence that needed clarification, and modified the text to make it hopefully clearer.

Section 4. The Reviewer notes that the first sentence of Sect. 4.2 is erroneous, as it states that the largest C1 SVC population is in the Pacific, while most actually are in Africa and Asia in JJA. This was a mistake, and we thank the Reviewer for having spotted it. We have modified the text to be actually correct.

Section 5.1 - Inclusion of nitric acid The Reviewer reiterates his concerns about the space devoted to the importance of PSC-like particles in SVC. See our answer to Major Comment #1.

Section 5.2 - local increase in aerosol concentration First the Reviewer notes that in addition to the SVC cloud cover, the TTL aerosol composition may also affect cloud microphysics. The text now includes this important and relevant point. From then on, the Reviewer expands on his Major comment #2. We hope to have addressed his concerns with our answer and changes to the text and Figures. In addition, he notes that the amount of new and relevant information brought by Figure 12 and Figure 14 is not sufficient to warrant their inclusion in the paper. More specifically, he notes how Figure 12 (maps of column-integrated SO2 concentrations) does not bring specific information useful for the study, as it document only two days of a specific eruption (Nyiragongo). In the revised version of the paper, these two maps (Fig. 12) have been replaced by time series of average column-integrated SO2 (2006-2008) to document the intensity of sulfate emission in the atmosphere due to eruptions throughout the entire period of study (see answer to Major comment #2). Moreover, the Reviewer does not think Figure 14 (Hovmoller diagram and time series of CALIOP backscatter average in the tropical band between 19 and 21 km from June 15th to December 31st 2006) brings any information that was not already published. In the original article, this figure was used to document the evolution of sulfate loading in the TTL after the Soufriere Hills eruption, which was already well documented by Carn et al. (2008) and Vernier et al. (2009). Following the reorganisation of this section, this figure has been replaced by a simpler representation of time series of CALIOP scattering ratio (19-21km) in three equally wide bands of latitude covering the Tropics during the entire period of study (June 2006-December 2008). This figure is now part of Fig. 12 (middle row), and is aimed at documenting fluctuations of TTL aerosol loading.

In the final part of his comment, the Reviewer mentions how the influence of biomass burning would be better explored using CO measurements from MLS onboard Aura.

This comment was addressed in our answer to Major comment #2.

Section 5.3 - Convective activity The final remark of the Reviewer reiterates his/her concerns about the limited subset of SVC used for the analysis of the importance of convection. See answer to major comment #3.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14875, 2012.

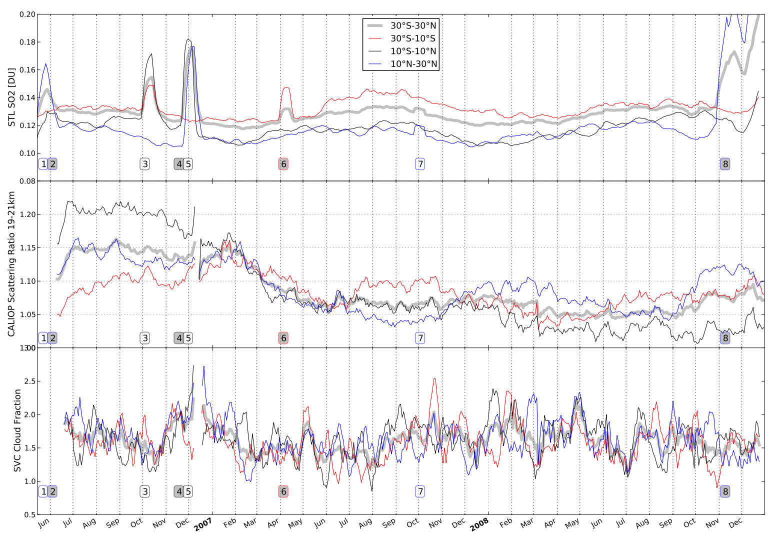


Fig. 1.

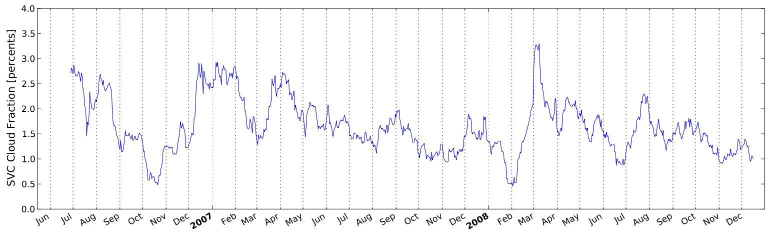

SVC Cloud Fraction [percents]
2007
2008

Fig. 2.

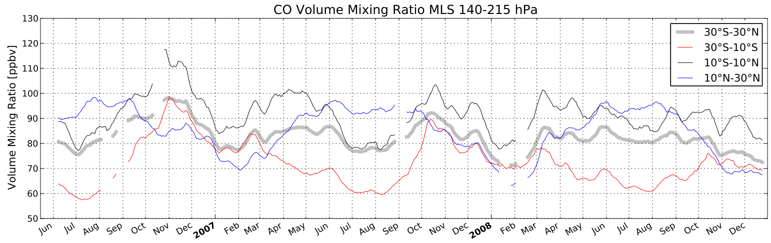
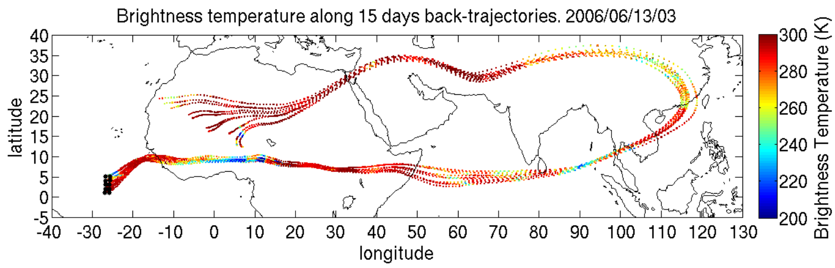
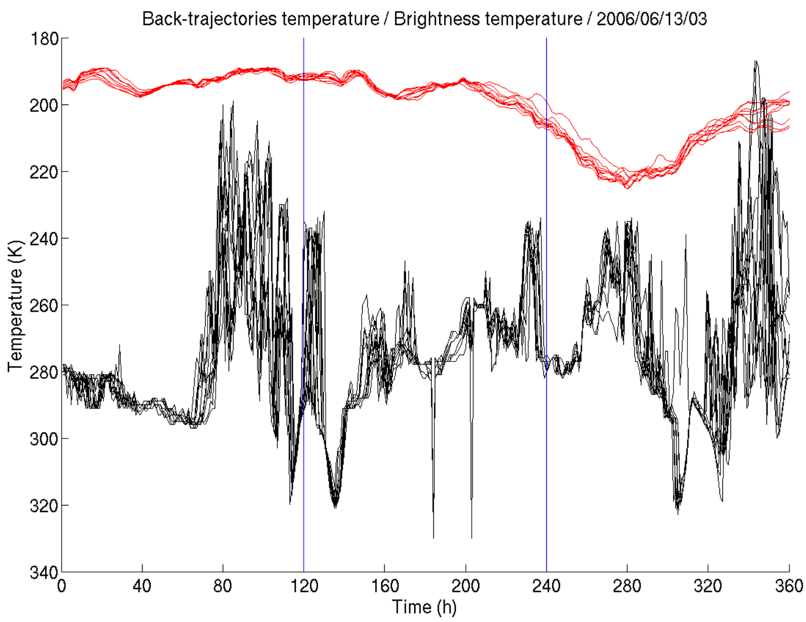

CO Volume Mixing Ratio MLS 140-215 hPa
Volume Mixing Ratio [ppbv]
30°S-30°N
30°S-10°S
10°S-10°N
10°N-30°N
2007
2008

Fig. 3.



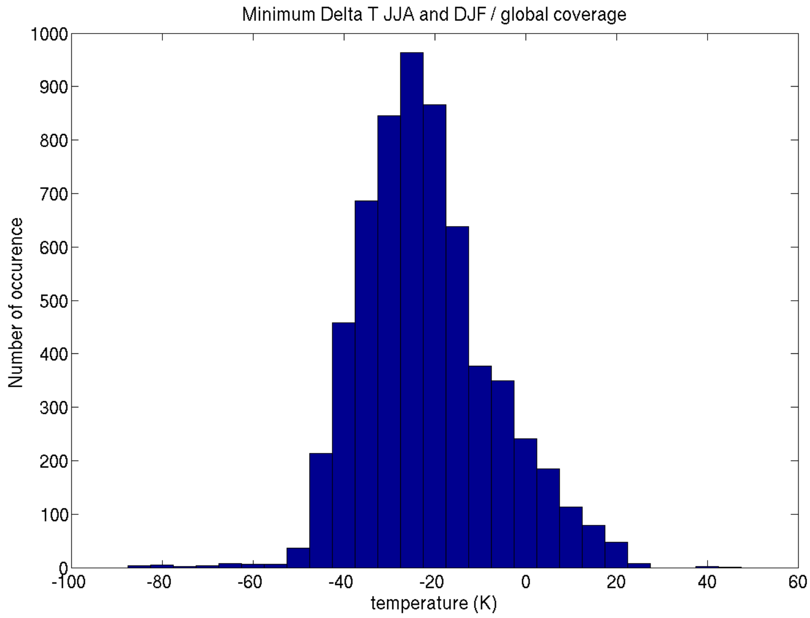
Brightness temperature along 15 days back-trajectories. 2006/06/13/03
latitude
longitude
Brightness Temperature (K)

Fig. 4.



Back-trajectories temperature / Brightness temperature / 2006/06/13/03
Temperature (K)
Time (h)

Fig. 5.



Minimum Delta T JJA and DJF / global coverage
Number of occurence
temperature (K)
1000
900
800
700
600
500
400
300
200
100
0
-100
-80
-60
-40
-20
0
20
40
60

Fig. 6.