Response to the comments by Referee #3

We greatly appreciate the reviewer for his/her thorough review and constructive comments. We have revised our manuscript as much as possible following his/her comments. Our point-to-point response to them is described as follows:

Page 16969, line 22- : The description here is rather literary. I am not sure its original, but would it be possible for the authors to mix it up with Fig. 1 in the following article?
http://www.annalsofgeophysics.eu/index.php/annals/article/viewFile/3407/3453

We have corrected the description about the first observation of PSCs in the 3rd para. of Section 1 and added Jørgensen (2003) as a reference.

Page 16970, line 23: miner -> minor?

The expression has been revised following the comment.

Page 16973- 16974: In subsections 2.2 and 2.3 there is information on the temperature data the authors used. First I would like to make sure if the authors used the ERA-Interim data to see the background temperature field. Though authors mentioned the COSMIC GPS occultation data are incorporated in the ERA-Interim analysis, I just wonder if the ERA-Interim data particularly in high latitudes are as nice as to the COSMIC data. The authors mentioned that the bias is less than 0.5 K, but they should refer to appropriate research results.

The forecast model used in ERA-Interim is AGCM whose horizontal resolution is T255. Before output process, high wavenumber components are removed in the spectral space to make the resolution uniform (IFS Documentation, 2010). In addition, representation of gravity waves depends on parameterization, which does not completely reproduce the characteristics and sources of actual gravity waves. Although the bias between COSMIC observation and ERA-Interim data is relatively small on average, the variance still exists and then the impact of gravity waves should be examined by observational data. Because this explanation is rather lengthy, we have added Dee et al. (2011) as a reference.

Page 16975, line 9-: To derive TNAT the authors followed Carslaw et al. (1994). In view of the following discussion in this paper, I suppose there would be big uncertainty in this derivation. I just warn the authors that they should be careful about this derivation, and suggest that the authors should write its uncertainty and limitation.

As noted in Section 1, previous studies showed that history of cloud particles can be important for PSCs microphysics and that PSCs may fall and removed from stratosphere. These processes are ignored in the threshold-based estimation. We have added a sentence about the limitation of the threshold-based estimation to the 1st para. of Section 2.5.

Page 16976, line 9- : The authors set the grid box as 20 x 5 degrees in longitude and latitude. As the CALIPSO observations are along the track with about 14 orbital circles a day, I just wonder if the CALIPSO observations really cover all the grid boxes defined by the authors. Such a point should be explained.

We chose the size of grid box so that 90% of the grid boxes in the latitude range of 60°S-80°S contain more than 10 CALIPSO observations. We have added the last sentence to the 1st para. of Section 3.

Page 16977, line 9- : For this TNAT calculation, temperatures from ERA-Interim are used on the 1.5 x 1.5 degrees grids, and HNO3 and H2O mixing ratios from MLS are used as those averaged for 20 x 5 degrees grids. Is my understanding correct? Also how do the authors deal with the vertical variations? The authors should explain this point more clearly.

First, HNO_3 and H_2O mixing ratios are calculated for 20° × 5° grid boxes. Then, they are interpolated linearly into 1.5° × 1.5° grids. A linear vertical log-pressure interpolation into the same pressure levels as those of the reanalysis data is also performed. We have added an explanation of this treatment of MLS to the 2nd para. of Section 2.5.

Page 16978- 16981: Section 4 with Figures 5, 6 and 7 is a good introduction to the three types of waves, but I don't find it is essential and I find it may result in redundancy of this paper. Personally I suggest that this section (including figures) may be shortened or deleted.

We regard Section 4 as one of the essential parts of this manuscript, which is helpful to intuitively understand the effect of the respective waves on temperatures in the stratosphere. However, it may be too lengthy, as the reviewer indicated. So, Section 4 has been significantly shortened.

Page 16979, line 15: V -> PV?

The expression has been revised following the comment.

Page 16982, line 4-: It seems to me that peaks of PSC areal extents in Figure 8b are located a little bit higher than those in Figure 8a. In relation to this, I found that there is no discussion about possible sedimentation of PSCs. How do the authors think about this point?

We agree that peaks of PSC areal extents in Fig. 8b are located at altitudes slightly higher than those in Fig. 8b and that this may be caused by the sedimentation of PSC particles. As mentioned above, the threshold-based estimation ignores sedimentation of particles. We have added the last sentences to the 7th para. of Section 3.

Page 16982, line 9- : The idea illustrated in Figure 9 is fine, but is it really understandable as a linear accumulation of the three types of wave effects? In the following I will repeat related questions several times, and quote this point as '(*)'.

It is true that the interpretation of the contribution of planetary waves to PSC coverage ratio shown in our analysis is not simple. We need to distinguish between planetary waves and a vortex shift. To clarify this issue, we have newly added Section 7, which is composed of two subsections. In the 1st subsection, we have argued two points: the reason why the contribution of planetary waves, ΔR_{PW}, can contain a part of the contribution of the vortex shift and the interpretation of our results.

We consider an ideal situation, in which a circular polar vortex shifts from the pole. In such a situation, ΔR_{PW} can take non-zero values despite the fact that PSC coverage ratio is the same as that without the vortex shift. This means that ΔR_{PW} may contain the contribution of the shifted vortex to coverage ratio.

We have reconsidered the meaning of the vortex shift in terms of ozone depletion. The vortex shift increases the proportion of PSCs in the lower latitudes. As mentioned in Section 3, PSCs in the lower latitudes are more important in the depletion of stratospheric ozone because the sun light arrives earlier in spring in the lower latitudes. Thus, it is rather appropriate to interpret ΔR_{PW} as the contribution of planetary waves and/or vortex shift to the ozone depletion.

Page 16982, line 29: They used variance to indicate wave activity, but is it really a good index compared with such as standard deviation? The latter would be nice to think about an estimate of wave amplitude.

Following the reviewer's suggestion, we have substituted standard deviation for variance in the manuscript and revised Figs. 10, 12 and 15.

Page 16983, line 13- : Figure 11 would be good to infer relative importance for the three types of waves. They mentioned the zonal mean temperature field is not effective to delta R, but is it really so and is it meaningless to indicate a contribution from (a) in Figure 9 in these figures? This is also related to the comment above (*).

The contribution of planetary waves defined in this study does not always express the change of PSC coverage ratio from the situation of the unperturbed polar vortex. However, it can still be regarded as an important factor of ozone chemistry. As already stated, we have newly added Section 7 to discuss this point.

Page 16984, line 2: For better readability there should be some descriptions on Figure 11d around here, then the authors may come back to this figure later.

Following the reviewer's suggestion, we have added the descriptions on Fig. 11d to the 8th para. of Section 5.

Page 16984, line 13- : This is related to the comments above (*), but I just assume if it is really conclusively mentioned.

As mentioned above, the contribution of planetary waves defined in this study does not always express the change of PSC coverage ratio from the situation of an unperturbed polar vortex. We have added Section 7 to explain this point.

Page 16984, line 18- : As written in the following for the northern winter case, I suppose it would be a mixture of planetary scale wave effect.

In the Northern Hemisphere, negative impacts of synoptic-scale waves are observed (Fig. 15d). However, this may be due to the filament structure in HNO_3 and H_2O distributions having synoptic-scales rather than synoptic-scale waves. Similarly, this feature in the Southern Hemisphere may come from the synoptic-scale distribution of HNO_3 and H_2O. We have added sentences about it to the 6th para. of Section 6 and 1st para. of Section 7.2.

Page 16985, line 16: January -> August?

We have substituted August for January.

Page 16987, line 8- : This description is also confusing to me in the sense that I have already mentioned above (*).

We have added Section 7.2 about this point.

Page 16989, line 17- : As to the role of planetary waves, is this an appropriate explanation?

Planetary waves can change not only the distribution of PSCs but also the PSC coverage ratios quantitatively in a similar manner to synoptic-scale waves (see Fig. 6b). We have discussed this point in Section 7.1.

Page 16990, line 1- : As to the difference in PSC areal extents between the northern and southern hemispheres, the authors should supply their idea about it.

Optically thin clouds are considered to appear more frequently in the Northern Hemisphere than in the Southern Hemisphere because the average temperature in the polar region through the winter is higher in the Northern Hemisphere. Because CALIPSO may fail to detect such optically thin clouds, the proportion of clouds undetectable by CALIPSO to the actual clouds can be larger in the Northern Hemisphere. We have added this explanation to the 3rd para. of Section 3.

References:

Dee, D. P., Uppala, S. M., Simmons, A. J., Berrisford, P., Poli, P., Kobayashi, S., Andrae, U., Balmaseda, M. A., Balsamo, G., Bauer, P., Bechtold, P., Beljaars, A. C. M., van de Berg, L., Bidlot, J., Bormann, N., Delsol, C., Dragani, R., Fuentes, M., Geer, A. J., Haimberger, L., Healy, S. B., Hersbach, H., Holm, E. V., Isaksen, L., Kallberg, P., Kohler, M., Matricardi, M., McNally, A. P., Monge-Sanz, B. M., Morcrette, J. J., Park, B. K., Peubey, C., de Rosnay, P., Tavolato, C., Thepaut, J. N., and Vitart, F.: The ERA-Interim reanalysis: configuration and performance of the data assimilation system, Q. J. R. Meteorol. Soc., 137, 553-597, doi:10.1002/qj.828, 2011.

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