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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous occurrence of polar stratospheric clouds and upper-tropospheric clouds caused by blocking anticyclones" *by* M. Kohma and K. Sato

Anonymous Referee #3

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In this article, the authors present a study of cloud cover as a function of altitude above the Antarctic region (55°S-82°S) based on spaceborne lidar observations obtained during five austral winters (June-September 2007-2011). They study the correlation between cloud covers at various levels of altitude, focusing on how PSC (15-20 and 20-25km) relate to lower clouds (9-11km). Finally, they relate those cloud covers to situations of blocking highs derived from the ERA-Interim dataset, concluding that such situations improve the chances of simultaneous occurrence of low clouds with high PSCs.

This is a well-conducted study that offers new and important insights into the processes

that lead to the formation of PSCs in the austral polar stratosphere. The analysis is rigorously done AFAICT. The results are put in perspective against previous litterature. The writing is most of the time clear and focused, except for a few confusing instances (see minor comments). Personally, I found several of the results very interesting (regarding e.g. the impact of atmospheric dynamics on the PSC composition and on PSC cover above the Peninsula, or the explanation of the cloud-less region near 13km). Overall, I have no problem recommending this article for publication in ACP, once (of course) the few comments I report below have been taken care of.

"Major" Comments

1. The main complaint I have about the paper is how it deals with the question of whether the clouds around 9-11km altitude are tropospheric, stratospheric or something else. This is in the end rather peripheral to the issues that are really explored in the paper, as most of the papers' conclusions can be reached without referencing it, but it is still interesting and worthy of a better treatment. For instance, the authors state at least four times (20008L7, 20010L20, 20013L25, 20021L15) that the typical winter tropopause height is 7-8 km, concluding that the clouds in the 9-11 km range should be considered stratospheric, or at least "tropopausal". However, they do not support their statement about the tropopause height by data or references. The only estimation of tropopause height attempted by the authors is for JJA 2008, when they report a tropopause near 9-10km on average (Sect. 3.2), i.e. higher than 7-8km. Looking into the references provided by the authors, Palm et al. (2005) report for September 29th 2003 a tropopause at $\sim$13km near 71°S, 8°W (based on temperatures from radiosoundings) and $\sim$13.2km at 72°S, 14°W (based on potential vorticity from UKMO meterological analyses). On September 30th, the same article reports a tropopause at 11.5km near 71°S, 8°W (radiosoundings). These dates are on the verge of the periods considered here (JJAS), but still are considerably above the 7-8 km range. Other references (e.g. Wilcox et al. 2011) suggest that the average south pole tropopause in JJA is at least higher than 8 km. Figures from the Palm et al. paper also show how large

tropopause height fluctuations can get along a single CALIOP overpass of the Antarctic. Fig. 5d in the present paper also shows these variations. My point is that comparing average tropopause heights (during a single season) with cloud altitude distributions cannot really tell whether the cloud population in the 9-11km range should be considered tropospheric or stratospheric. To be more convincing in this regard, the authors would need to document cloud fraction as it relates to tropopause height in individual CALIOP profiles (given how the tropopause height can fluctuate above the Antarctic on a given day). If that is not possible, I would suggest to the authors to focus solely on the altitudes of the clouds without trying to address the "troposphere/stratosphere" question in the main body of the article. In that case, this question should certainly not be presented in the abstract as one that is being addressed by or answered in the paper. The question whether these clouds are tropospheric or stratospheric could be raised in the conclusion as one that needs further investigation. Doing this would remove several confusing instances in which the text mentions upper tropospheric clouds being located above the tropopause, which often reads like a contradiction.

20013.6: The authors state that the discontinuity in cloud cover near 20km should be ignored. It is rather hard to do so. Even by masking the data at 20km, one still finds much larger values above 20km than below. How can one reconcile those values? If I'm not mistaken, this discontinuity is due to the change in CALIOP vertical resolution above (180m) and below (60m) 20.2 km, which leads to a better signal-to-noise ratio above 20.2km than below, which leads to an improved ability to detect clouds above 20.2km (assuming cloud properties stay constant throughout the vertical, which they do not). This change in SNR means cloud fractions above and below 20.2 km should not be directly compared, as the instrument's ability to detect clouds is different in the two altitude levels. This fact might very well affect the correlation coefficients reported in Fig. 2 and 3. The right way to address this problem would be to resample CALIOP data below 20.2km to the same resolution as above (180m) before performing cloud detection. If this is not feasible, the authors should at least mention and discuss this problem instead of asking the reader to ignore it.

Minor comments

20008.4: The abstract and the conclusion both state the study uses satellite lidar observations for five austral winters (2007-2011). However, fig. 2 in Sect. 3.1 only considers JJAS 2008, and fig. 4 in Sect. 3.2 only considers JJA 2008 (fig. 4). Could the authors justify these choices, or extend the period considered in these two figures to cover JJAS 2007-2011 (for consistency)?

20011.17: The authors explain that they derived daily occurrence frequency of PSC/UC in 10° longitude intervals for five latitude bands. It is still unclear to me how this frequency was obtained. Did the authors compute the percentage of CALIOP profiles for a given day in which a PSC/UC was detected (i.e. the cloud fraction)? Or did the authors compute daily vertical profiles of cloud fraction in lon-lat boxes? If that's the case, what is the vertical resolution of these profiles? (I assume the full CALIOP resolution was used, but it should be explicitely stated). What was the horizontal averaging used to produce the CALIOP profiles? What is the daily number of CALIOP profiles in a lon-lat box? Are all profiles in a given box from the same orbit? i.e. document the sampling. Please expand.

20013.11: Following the lack of details in the description of the CALIOP dataset, it is not clear how the two panels of Fig. 2 were created. What is the resolution, is the sampling constant throughout the vertical?

20013.11: I am very unimpressed by the correlation coefficients presented in Fig. 2 and 3. A correlation coefficient of 0.1 is not symptomatic of a correlation, rather the opposite (later in the paper, the authors present correlation coefficients above 50% that actually do report a correlation). For instance, the authors conclude that coefficient correlations above 0.1 means clouds at 15-25km are correlated with clouds at 9-11km, but correlation coefficients below 0.09 means they are not correlated with clouds below 9 km. A 0.01 difference in correlation coefficient does not seem like enough robust evidence to reach these conclusions. The fact that those coefficients are meaningful

at a significance level of 99% (how did the authors obtain that number?) only means that the absence of correlation is robust. However, given how the correlation coefficient evolves with altitude in Fig. 2 and 3 suggests that the link between clouds really does change with altitude. I would suggest to the authors that these low values are due to their using cloud fraction profiles at CALIOP full vertical resolution. This is not needed – investigating correlation coefficient considering cloud fraction profiles at 300 or 600 meters resolution would still be representative of cloud spatial extent while giving much better (read: higher) correlation coefficient results.

20013.11: I do not understand why, if the authors are trying to study the correlation between PSC and tropospheric clouds, they stop at 9 km altitude. In that regard, it would be interesting to see what is going on below. Removing the "troposphere/stratosphere" angle from the introduction would solve this (as suggested in major comment 1).

20013.13: I am not sure how to read "time series for 15-25km are positively correlated with those for 15-25km..." I would assume this to be expected and obvious, i.e. along the identity line in Fig. 2 and 3? Please clarify.

20016.25: Here it looks to me like the paragraph conclusion states the opposite of what it should: right now, the sentence means that “PSCs do not cause anticyclonic PV anomalies”. The opening question of the paragraph was “do anticyclonic PV anomalies cause PSCs”, not the other way around. The concluding sentence should be rewritten.

20017.1: I don’t understand how to read a “geographical latitude-pressure section of equivalent latitudes”. I hope the other reviewers have more experience with this than I, and I defer to their judgement regarding these results.

20018.7: The authors define the acronym "AEL" for Average Equivalent Latitudes, but this acronym is only used once in the rest of the paper, two lines below. I suggest they remove this acronym. Same goes for "CBH", which is used three times in all.

20018.17: This sentence is very unclear. I assume the authors want to express that

since BHs cover $\sim$15% of the region between 55 and 82°S, there is a non-negligible possibility they might affect PSC/UC formation. I think this statement could be removed altogether, as it is confusing and this hypothesis is quickly explored in the following paragraphs.

20018.25: "The simultaneous occurrence of PSCs and UCs is defined as the cases when both PSC/UC frequencies in 9-11km and 15-20km are greater than 10%". Over what time step were these frequencies calculated?

20018.27: "about a half of..." How much exactly? Please present total numbers considering all years in Table 1.

20019.1: The last sentence of this paragraph relates poorly to the one before it. The previous sentence states that $\sim$ half (45% actually) of cases with SVC+UC happen during BHs (3145 cases during BH vs. 3787 cases with no BHs). This value can’t be three times higher than for cases without BHs. Table 1 shows that $\sim$11% of no-BH situations lead to PSC+UC and 27% of BH situations lead to PSC+UC (i.e. roughly 3 times more). If that is what the authors meant, they should clarify the sentence.

20020.25: This paragraph contains a lot of repeated ideas from the last paragraph of Sect. 3.3. Please try to state them only once.

technical comments

20008.24: "and lead irreversible removal..." There’s a "to" missing.

20016.1: "Little PSC/UCs are observed around 14km..." do the authors mean that clouds at this altitude are small, or that there a few of them? If it’s the latter, "few" would be better.

20016.16: What is a "e-folding" vertical extent?

20022.8: "... for providing the CALIPSO data product" should be removed.

references

Wilcox, L. J., B. J. Hoskins, and K. P. Shine (2011), A global blended tropopause based on ERA data. Part I: Climatology, QJRMS, 138(664), 561–575, doi:10.1002/qj.951.

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