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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improved SAGE II cloud/aerosol categorization and observations of the asian tropopause aerosol layer: 1989–2005" *by* L. W. Thomason and J.-P. Vernier

Anonymous Referee #1

Received and published: 11 January 2013

1) General comments:

The manuscript by Thomason and Vernier presents a detailed new analysis of the SAGE II extinction measurements with improved discrimination between clouds or cloud-aerosol mixtures and aerosols with respect to the original so-called Kent method. This dataset is of high scientific interest, because no other long record aerosol dataset covers the 1980s, 90s and up to 2005. The paper investigates the recent finding of the Asian Tropopause Aerosol Layer (ATAL) by the CALIPSO instrument and the SAGE II results show no significant ATAL signal in the dataset before 1999. The most critical point in the analysis is the differentiation between aerosol and clouds. This procedure is described in detail but the robustness of the method is not completely convincing. The reader may think, that clouds can create similar signals. Some improvements in the description of the method are necessary for final publication. ACP is exactly the correct place for the publication of the results. The paper is well written but some technical changes will improve the quality of the manuscript (see comments below).

2) Major comments:

Centroid Method: The description of the aerosol, “artificial” cloud centroid, and the corresponding “mixing centroid function” R is difficult to understand (Sec. 2.3 and 3.2) and not completely convincing why these methods help to detect the ATAL signal:

(a) Are R and a in Sec. 2.3 sensitive to altitude and season? This should be mentioned.

(b) Is it possible to prove the 'mixture' line with model calculation of realistic particle size distributions and mixtures. This would give more confidence to the approach.

(c) I would suggest to highlight the centroid parameter R_c, k_c, R_a, and R_c in Figure 4. This would also confirm why you use two very similar PDF figures instead of one, although all information of Fig. 4 is found in Fig. 6 as well.

(d) The difference between the two centroid methods is only marginal. Please highlight more explicitly where in the PDF diagram you win the information of aerosol signals producing the ATAL signal. Why is the Kent method less sensitive to detect the ATAL signal? Please specify how many additional aerosol measurements you get form your analysis and specify more detailed the altitude dependence on R (e.g. by a figure for 14 km). Is there no dependence with latitude as well?

(e) What are the effects of broken clouds (Fig. 3, clouds not filling the tangent hight layer) in the classification Fig. 6. I would expect smaller extinction for similar extinction ratios. But why you cannot observe extinctions smaller than 2 10^{-4} km^{-1} for ice clouds and where should I find in the classification diagram a pure ice cloud signal?

Like the authors mentioned: “One concern with the analysis would be that clouds are

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still slipping by the analysis and artificially creating an aerosol feature." An additional validation analysis with coincident lidar or in situ measurements could clear these concerns out. Please comment why this isn't an option.

Figure 8 presents one major result of the new classification method, the ATAL signal at 16 km in the global mean extinction ratio distribution. There is a surprisingly good correspondence between the ATAL signal in JJA with the corresponding season of the SAGE II subvisible cirrus climatology in the Wang et al. (1996) Plate 4 at 17.5 km. The ATAL signal is exactly found in the regions with the highest SVC occurrence rates (up to > 60%) for this season. This makes the differentiation even more difficult. Is it possible that the new method is biased by the underlying SVC occurrence or potential trends in SVC occurrence? Is the SVC analysis by Wang et al. biased by potential aerosols?

In my opinion it would be helpful for the reader if the authors would present figures of the CALIPSO results in conjunction with the SAGE II results to highlight similarities in the vertical and horizontal structure.

3) Minor comments

p27535 and Fig.7: Do you need this figure really for your analysis. The analysis and interpretation on the lower altitude aerosol signals is not very detailed. You may skip figure and discussion or present some more details. I would expect signals over northern Canada as well, a region of large and frequent boreal fires.

P27540, l25: Please specify why "at least 2003" is an episodic event ?

Fig. 8: Have you specified in the manuscript why you are using extinction ratio (relative to molecular) instead of extinction?

The last sentence of the conclusion is a speculation and should be deleted. A "recent phenomenon" is not necessarily of "human origin".

4) Technical comments

p27523, l5: Vernier (2009) is missing in the reference section.

l7: “performed” instead of “perform”

p27523, l15: “direct comparison” instead of “detailed comparison”, because you are able to compare the results.

p27524, l15-26: this section of the introduction includes identical sentences of the abstract. It also discuss some final results of the manuscript (l21-26) which confuses the reader at this early stage of the manuscript. Please improve this section.

P27525: “is not a clear cut”

p27531/4: Formula (1) and (2) are nearly identical and (2) can be skipped. A detailed description of the intention why you use the δ offset would be sufficient.

P27537,l23: “saturation ternary solution (STS)” must be “supercooled ternary ...”

Fig. 4: delete "(when right of the green line)", the green lin eis only present in Fig. 6.

Fig. 6: please explain the regions IIa, IIb, IIIa, and IIIb in the caption or better in the corresponding manuscript section.

Fig. 7 and 8: I expect the ATAL signal would be better to spot in a cylindrical projection.

Fig. 11 is too small in the present form.

References:

Wang, P. H., Minnis, P., McCormick, M. P., Kent, G. S., and Skeens, K. S.: A 6-year climatology of cloud occurrence frequency from SAGE II observations (1985–1990), J. Geophys. Res., 101, 29407–29429, 1996.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27521, 2012.