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Interactive comment

Interactive comment on "30-year lidar observations of the stratospheric aerosol layer state over Tomsk (Western Siberia, Russia)" *by* Vladimir V. Zuev et al.

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Review of Zuev et al., "30-year lidar observations of the stratospheric aerosol layer state over Tomsk (Western Siberia, Russia)"

Reviewer: Mike Fromm

This review consists of this review document and the manuscript annotated with comment bubbles. The comments in this document reflect both major and minor concerns. Technical suggestions are identified in the comment-bubble-annotated manuscript.

This manuscript gives a wide-ranging analysis of the aerosol lidar data collected at a western Siberia location. The long temporal span of this data set, for a location far

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Discussion paper

removed from other ground-based lidars, is a welcome addition to the globe's sparse stratospheric aerosol archive. Considering that these data are presumably still being collected adds value for continued monitoring of the upper troposphere and lower stratosphere. Consequently this is a potentially critical work that may offer significant value to our understanding. The authors' approach is similar to many prior works that examine a particular lidar's data set in the context of other data sets and meteorological analyses. They present some individual lidar profiles, long-term time series, and annual cycle analyses. This is a very appropriate study for Atmospheric Chemistry and Physics.

Along with these assets, the paper has several major weaknesses. My assessment is that the weaknesses substantially limit the value of the manuscript in its present form. For the paper to merit publication, attention to these major concerns must be paid.

One major concern is a significant absence of recognition of work that has direct bearing on the analyses and conclusions presented herein. The effect goes beyond that of showing insufficient background work. It extends to the realization that critical scientific aspects of stratospheric aerosol and cloud have not been taken into account in the interpretation of the Tomsk lidar data. Detailed examples are given below. Another major concern is that the Tomsk data may be biased with respect to other similar lidar data sets, as called out specifically in a comment below. The manuscript offers no direct or even indirect comparisons of the Tomsk lidar data with any other aerosol data sets; hence it is not possible to assess the accuracy of the aerosol data in the full range of stratospheric aerosol loading. Another concern is that the authors utilize a local measurement data set of total ozone abundance, but without any qualification. The reader is left with great uncertainty as to the robustness of the results and certain interpretations the authors give for certain phenomena.

Below is a list of questions and concerns, in page and line number order.

Somewhere in this paper the recent review paper by Kremser et al. should be mentioned and cited. Kremser, S., et al. (2016), Stratospheric aerosolâĂŤObservations, processes, and impact on climate, Rev. Geophys., 54,doi:10.1002/2015RG000511.

There is no citation of Vernier et al (GRL, 2011), Ridley et al. (GRL, 2014), Santer et al. (Nature Geo., 2014) and some other very relevant recent papers on volcanoes and recent stratospheric aerosol layer trends.

P2, L10. Consider citing Jäger, H. and Wege, K.: Stratospheric Ozone Depletion at Northern Midlatitudes after Major Volcanic Eruptions, J. Atmos. Chem., 10, 273-287, 1990.

P2, L5. "vulcanian" refers to a specific style of eruption, the characteristics of which are not associated with stratospheric injection. Is the term even needed to make the point of this sentence?

P3, L19. How about measurement frequency? What was the lidar operation frequency and regularity? It would be important for the reader to know what is typical for the number of profiles that go into the 10-day average.

P3, L23. Consider giving the name of the PMT manufacturer.

P4, L4. This statement arouses my curiosity to know if this lidar made any observations of the Chelyabinsk bolide plume in 2013. It was observed by satellite aerosol profilers above 30 km. Gorkavyi, N., D. F. Rault, P. A. Newman,A. M. da Silva, and A. E. Dudorov (2013), New stratosphericdust belt due to the Chelyabinsk bolide, Geophys. Res. Lett., 40,4728–4733, doi:10.1002/grl.50788.

P4, L10. What does the pi character refer to? Please consider stating its meaning.

P4, L20. This is understandable and defensible. But the median tropopause at Tomsk is such that a lot of the lowermost stratosphere is not sampled with the fixed lower limit of 15 km. For instance, many volcanic plumes just above the tropopause have occurred in recent years (Kasatochi is one example). And pyrocumulonimbus smoke plumes are routine in summer, but usually between the tropopause and ∼15 km. It might be worthwhile to try another lower-boundary, for instance based on potential temperature or a tropopause-relative height offset.

Figure 2. The error/uncertainty bars are not defined or even mentioned. Moreover, however they are defined, the majority of them extend farther than the winter/summer range. This suggests to me that this pattern of monthly averages is not particularly repeatable. Auth should consider discussion of the robustness of this pattern in light of the relatively large uncertainty. The feature that stands out here is the March average and uncertainty. Can the authors explain why this month's aerosol amount is so large and variable?

P6, L4. The minimum integrated backscatter coefficient value in Figure 1 and Trickl et al.'s background value are not equal, in contrast to the authors' claim. Figure 1's value is between 1e-4 and 2e-4. Trickl et al.'s 1979 background value is 5e-5, smaller by roughly a factor of two. Zuev et al.'s Pinatubo peak matches well with Trickl et al., but the comparable quiescent-period values are off by about a factor of 2. This sentence should be corrected. The differences in the background values should be acknowledged and discussed. Trickl integrated from a tropopause-relative altitude that probably includes more of the lowermost stratosphere than Zuev et al. yet their integral is much smaller. The wavelength difference is seemingly too small to account for the difference.

P7, L14-19. Discussion of the Brewer Dobson circulation and its impact on the extratropical SAL. Presumably the point here is that the pattern shown by Fig. 2 is consistent with the generalizations here such that a citation is needed for work(s) that show this intra-annual tendency.

Figure 3. What is the averaging period of the data points? Since error bars were shown for Figure 2, they should also be shown here, and the implications discussed.

P8, L17-18. This statement does not reconcile with VEI=4. I recognize that VEI=4 is in the GVP database. So this sentence probably should call out the apparent discrepancy.

P10, L3. The trajectory analysis is largely unnecessary. There is no need to run trajectories for the 8 Aug observation; it's obvious that layer can't be from Kasatochi (and there is no other plausible source). For the September observation (and latter ones), the stratosphere at that time had Okmok and Kasatochi aerosols distributed all over. If the Tomsk back trajectory intersected with any of the Okmok layers prior to its pass over the Kasatochi volcano, that is a more plausible attribution than a 3.5 week trajectory connection.

P10. The impression I got from reading this analysis was that Zuev et al. are attempting to draw general conclusions about the Kasatochi and Okmok plumes and the SAL. If they are instead limiting their assessment of the SAL to just where the Tomsk observations are, they should make that explicit. The more general SAL analysis of these plumes is already published but not cited in this manuscript. E.g. Bourassa et al., Anderssen et al., Kravitz et al. These make clear that the Kasatochi and Okmok plumes were observed at all altitudes from the tropopause to nearly 19 km. Andersson et al. (2015), Significant radiative impact of volcanic aerosol in the lowermost stratosphere, DOI: 10.1038/ncomms8692. Bourassa, A., D. Degenstein, B. Elash, and E. Llewellyn (2010), Evolution of the stratospheric aerosol enhancement following the eruptions of Okmok and Kasatochi: Odin-OSIRIS measurements, J. Geophys. Res., 115, D00L03, doi:10.1029/2009JD013274. Kravitz, B., A. Robock, and A. Bourassa (2010), Negligible climatic effects from the 2008 Okmok and Kasatochi volcanic eruptions, J. Geophys. Res., 115, D00L05, doi:10.1029/2009JD013525.

P10, L12. My impression is that Zuev et al. are attempting to assess the accuracy of the injection height as reported by the GVP. The point made here, and below, regarding the accuracy of the Hmpa for the two eruptions, is of little consequence. I also get the impression that they are using the altitude of the back trajectory endpoint over the volcano as a point of comparison with Hmpa. If I have the wrong impression, perhaps other readers will be similarly affected. So I ask the authors to clarify the wording here. Otherwise, giving the precise altitude of a 3-4 week old trajectory much weight asks more of the trajectory model and the weather analyses than they can promise. Secondly, the Hmpa data in the GVP database is neither tightly constrained. Thirdly, it can be shown easily with CALIOP data that the Hmpa for these two eruptions is even farther off the mark than this analysis suggests. CALIOP data within a few days of each eruption shows that the injection altitude was at least 17 km (Okmok) and 18 km (Kasatochi). These data would offer a much more compelling analysis for this argument than the Tomsk data.

P11, L15. This is way too precise and exclusive. The Sarychev Peak eruption spanned several days, as shown in their table. The time of a 3+ week back trajectory is by its nature too uncertain to permit such a definitive connection.

Figure 10. These strong stratospheric layers at 15 km and their source are not subjected to a back trajectory test. Given that they are a much shorter time post eruption than the prior examples, it would critical to show if they connect to the volcano. If Eyja did not inject material this far above the tropopause, where did this material come from?

P15, L13. Regarding the connection between the weak layer in Figure 12, localized reduced total ozone, and Merapi, a stronger candidate would be Arctic O3 depletion. See Manney et al., (Nature, 2011) "Unprecedented Arctic Ozone Loss in 2011" doi:10.1038/nature10556

P16, L4. This conclusion regarding Nabro's injection height has been exhaustively disputed. There are no citations here of the papers demonstrating the classic, direct injection of Nabro to the stratosphere. http://science.sciencemag.org/content/339/6120/647.4 http://science.sciencemag.org/content/339/6120/647.3 The Bourassa et al. scenario was shown to be improbable by Fairlie et al.(2014). Clarisse et al. (2014), Fairlie et al. (2014), and Penning de Vries et al. (2014) showed, using a variety of satellite data, that direct injection above the tropopause was more consistent with these data than the indirect path via the Monsoon. Fromm et al. (2014) established a root cause for the misattribution to the Asian Monsoon pathway and made connections with prior papers on other volcanic stratospheric aerosol discrepancies. Hence the full weight of Nabro-related papers gives a very different perspective than what is documented here. I would ask the authors to more fully capture these various works in their presentation on Nabro. http://www.atmos-chem-phys.net/14/7045/2014/ http://www.atmos-chem-phys.net/14/3095/2014/ http://www.atmos-chem-phys.net/14/8149/2014/ http://onlinelibrary.wiley.com/doi/10.1002/2014JD021507/full

P16, L18. Often? I'm not aware of any literature showing any direct positive correlation between PSC formation and volcanic sulfur loading. Hence a citation is needed. Fromm et al. (2003) actually showed little (or even negative) correlation between PSC frequency and ambient aerosol loading. Hence it would be important for the authors to substantiate the claim they make here. Fromm, M., J. Alfred, and M. Pitts, A unified, long-term, high-latitude stratospheric aerosol and cloud database using SAM II, SAGE II, and POAM II/III data: Algorithm description, database definition, and climatology, J. Geophys. Res., 108(D12), 4366, doi:10.1029/2002JD002772, 2003.

P17, L6. Not necessarily. In fact, spatial correlations between total-O3 minima (so-called ozone mini-holes) and PSCs are routinely attributable to synoptic-scale dynamics. See Hood et al. and Teitelbaum et al. Exploring polar stratospheric cloud and ozone minihole formation: The primary importance of synoptic scale flow perturbations, Teitelbaum, H., M. Fromm, & M. Moustaoui, J. Geophys. Res., 106, 28,173-28,188, 2001. Origin of extreme ozone minima at middle to high northern latitudes, Hood, L. L., B.E. Soukharev, M. Fromm, and J.P. McCormack, J. Geophys. Res., 106, 20,925-20,940, 2001.

P17, L17. This PSC is consistent with the findings of Fromm et al. (1999) who showed that the cold pool and PSC frequency in the northern winter of 1994/95 was located near the Tomsk longitude. http://onlinelibrary.wiley.com/doi/10.1029/1999JD900273/epdf

P17, L19. Please see the prior comment regarding mini-holes.

P17, L20. This seems to be quite unlikely considering that PSCs are formed inside the vortex, which represents air isolated from the extratropics. Rabaul aerosols, introduced just a few months before the northern 1994/95 vortex season, would not likely have been meridionally transported that far north in time to be in place before the vortex formed and isolation was in place.

P18, L2. It seems highly unlikely (or at least hard to prove) to argue for a PSC in the northern polar vortex one season after a southern hemisphere volcanic eruption. It would be best to state this as speculative, if the statement is to remain.

P18, L6. See the prior comment about mini-holes. At this time in Jan 2007, a localized o3 minimum was at Tomsk's longitude. The ozone signature was likely an artifact of local dynamics. http://exp-studies.tor.ec.gc.ca/cgi-bin/selectMap?lang=e&type1=du&day1=27&month1=01&year1=2007&howmany1=1&interval1=1&intervalunit1=d&hem1=

P18, L26. By postulating that pyroCb smoke could increase "annual average" stratospheric aerosol, it would imply that prior research has come to similar conclusions. I'm not aware of any such finding. Please modify this statement appropriately.

P19, L13. "always" was not proved or demonstrated herein. Hence a citation is needed, or this point should be restated or removed.

P19, L18. It's not clear what is meant here. All volcanic plumes are represented as an initial point source. Please clarify.

P20, L15. What is meant by "thermal speed?" Please clarify.

P20, L15. This is confusing. A plume is buoyant because it is less dense than the surrounding air. This makes it sound as if the surrounding umbrella region is less dense.

P20, L24. "drifting" needs to be clarified. It implies to me that the ozone feature is drifting with the wind, whereas the papers previously offered in these comments show that the minihole is tied to dynamics and thus move with the speed of the synoptic-scale wave, not the wind.

P20, L29. The impact of pyroCb smoke on annual average stratospheric aerosol has not been shown in the literature to my knowledge. Please cite a paper or modify this statement accordingly.

P20, L32. The measures in acres and square km are not equivalent. Please correct this.

P21, L2. What is the reason to call out the Happy Camp fire?

P21, L4. Regarding pyroCb smoke, I believe that there would be many occasions through the years for stratospheric observations at Tomsk. But of course the frequency of such observations would decrease rapidly with altitude above the tropopause. The highest smoke layers in the northern hemisphere to my knowledge are ∼19 km. They are much more likely to be at 12-15 km. Perhaps it would be to your advantage to directly employ the local tropopause height in the search for pyroCb smoke instead of a fixed altitude (e.g. 15 km) that is generally ∼4 km above the tropopause.

Technical Comments See the accompanying pdf of the manuscript.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-792/acp-2016-792-RC2-supplement.pdf

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