Trickl and co-authors (hereafter shortened to "auth") exploit the long-term aerosol-profile record based on lidar measurements at Garmisch-Partenkirchen (hereafter shortened to "Garmisch") to examine the most recent decade of tropospheric and stratospheric aerosol loading. This is a period far removed from the latest super plinian eruption, that of Mount Pinatubo, and thus promotes the study of smaller injections and other aerosol pathways into the upper troposphere and lower stratosphere (UTLS). Auth focus on the period 1997-2011. Within that time frame they compare trends at Garmisch with studies utilizing other aerosol lidars, to check the conclusions/attributions made in these other works. They also delve deeply into the complex circumstances surrounding the Iceland volcanic eruption in spring 2010 and its effects on aerosols over Europe. They conclude that the volcanic ash impact was convolved with other aerosols, from sources close to and quite distant from Europe. Hence, this work is potentially quite valuable and perfectly appropriate for ACP.

However, my assessment of this paper is that it will require substantive revisions before it is acceptable for publication. The science quotient of the paper is not the issue. The primary issue is the clarity with which the analysis is presented to the reader. I found the paper to be difficult to follow, but that can be resolved with great attention to judicious selection of material, improved organization, and clearer English composition.

Substantive concerns are listed first, followed by minor/technical concerns. Wording taken directly from the paper is placed in **bold;** reviewer words are in plain text. Each item is identified either by page number plus line number (or range of line numbers) such as P5, L9-11 for page 5, line 9 to 11. Some items are identified by the section name/number or figure number.

A few suggested references are shown at the end of this report.

Substantive Issues

P9, L17 (Figure 1). If Figure 1 is constructed identically to that of Jaeger [2005], I have a concern regarding the possible aliasing of strong aerosol layers in the lowermost stratosphere. The two Figure 1s look identical for the common time frames, thus it appears that an identical integration method is used. The concern relates to Jaeger's statement: "The integration ranges from 1 km above the tropopause (in the presence of cirrus layers from the top of the cirrus layer) to the top of the aerosol layer at about 30 km." The nature of some UTLS smoke and volcanic plumes is that they can be mistaken for cirrus clouds due to their strong AOD and proximity to the actual tropopause. It would be important to account for the number of lidar profiles that are integrated from the top of a "cirrus" layer when that feature is above the tropopause. This of course will serve to improve the characterization of any strong aerosol layers (ash, smoke, sulfate, or dust) that are above but in proximity to the tropopause.

P11, L1. The choice of the trending period is arbitrary. 2004 is the start of an intensive sampling period preceded by a 1-year gap, and a nearly ½ year gap in 2002. It is unconvincing to me to say **"a slight increase is indicated."**

P11, L15. I find the argument about marking the turning point between Pinatubo decrease and subsequent rise unconvincing. First, the post-2004 slope is unconvincing to my eyes. Next, the entire period 2002-2004 suffers from a significant decrease in sampling. Third, similar backscatter coefficient minima in the post-Pinatubo era are reached in 2000, 2002, 2004, and 2006. I.e. there is no trend in these values. All things considered, the period 2000-2006 does not appear to have enough information to draw such conclusions. Perhaps the authors could take advantage of other European NDACC lidar data during this period to augment this time series?

P11, L25. **"These listings…show that the number of volcanic eruption periods had been strongly underestimated." "Eruption periods"** is not defined in the text nor in the figure caption. This needs to be defined. "Underestimated" by whom? By what paper? By what data set? This needs to be clarified.

P12, L3. It seems this paragraph's discussion refers to Figure 3, a time line that begins in mid-2004. Thus to claim that "the number of mid-latitude eruptions stays low until 2005" reads more into this figure than it gives.

P12, L1. **"Table 1 lists the most significant eruptions at higher latitudes since 2000 that should have had more direct influence on our observations."** What is meant by "should?" What is meant by "direct influence?" This sentence ends a paragraph, and there is no further referral to these pre-2004 eruptions. Hence, the listing of these eruptions seems to have little merit.

P12, L3-13. It is evident that auth are taking issue with Hoffmann [2009]'s discounting of the influence of volcanoes on the stratospheric background. However, their message is awkwardly written and contains subjective terms such as **"the number of eruptions in this latitude range is impressive,"** giving no basis with which to assess impressiveness (or relevance) of these numbers.

Section 3.3, L1-19. The discussion of fires is interesting but it is hampered by the lack of an attempt to discuss it with respect to Figure 1 (or any other views of the Garmisch data). Auth rightly mention that the Garmisch sampling of the Chisholm event (2001) was limited by virtue of the prevailing winds (in the plume's first passage over Europe) and interrupted measurement routine. However, the result is that the reader cannot assess this event with respect to the data presented herein. I.e. little value is added to this paper by all of the discussion. For this reason I suggest that the Chisholm paragraph be shortened significantly.

Section 3.4., PSCs. This section is interesting, but it falls almost completely outside the focus of this paper. The PSC discussion, involving two individual cloud observations 12 years apart, does little to augment the main aim of this paper according to the abstract: **"Here, we focus more on the long-lasting background period since the late 1990s and 2006, in view of processes maintaining a residual lower-stratospheric aerosol layer in absence of major eruptions, as well as the periodof moderate volcanic impact afterwards."** One way to bring this discussion into line with the main thrust would be to relate these individual PSC backscatter enhancements to the overall pattern in Figure 1. Do these PSCs show up in Figure 1? Can an arrow or some other annotation be provided in Figure 1? If the authors determine that this is not

a good idea, I would recommend removing this section. If the section is kept, I suggest auth cite Hervig, (GRL, 1999) and tie in his paper with the 5 March 1996 Garmisch PSC. Hervig's 3-4 March PSC observations over the UK were part of the same large-scale forcing responsible for the Garmisch PSC. Another paper that gives greater context to the March 1996 PSC event is Teitelbaum et al., [2001].

Section 3.3, L14-19. **"The impact of forest fires is discernible during longer background periods, but the contribution is small in comparison with that of major volcanic eruptions."** At this point in the paper, auth have not shown this. The duration **("a few months")** is not shown by referral to Figure 1 or any other figure here. If the information on the duration is not from this paper, the source paper should be cited. If it is evident in Figure 1, it should be pointed out.
P13, L20-28. Auth state that this pyroCb event "**yielded much more pronounced signatures"** presumably in comparison to the Chisholm event. Is that correct? If so, it would be valuable for the reader to see the evidence for that. For instance, is this aerosol perturbation responsible for the extra-sharp increase in backscatter imbedded in the larger, sharp increase caused by Pinatubo (Figure 1)? If so, that would be a very worthwhile feature to point out and discuss at greater length.

P18, L1-4. **"The anti-cyclonic descent…"** It is not clear what is meant by descent here. There is no preceding discussion of any descending air mass or particles, so it is not possible to determine what the authors mean here. Please clarify. Also, this long sentence is awkwardly constructed; it combines two consistent clauses, **"accompanied by…"** and **"forecasted by…"** with the inconsistent clause **"documenting the vicinity…"**

P18, L1-10. It is not clear how this paragraph benefits the reader. What is important about this descent and the stratospheric intrusions? Please clarify or consider removing this paragraph.

P20, L27. Here auth state that aerosols are detected up to 14.3 km altitude. It is not obvious to me what threshold they are using to draw that conclusion; the backscatter is small and not demonstrably different this at altitude between the 4 days plotted. Can auth defendably place on the figure a backscatter threshold for aerosol detection?

<u>Minor/Technical Concerns</u>

P3, L8. The parenthetical "(varying)" is not needed. Consider deleting this.

P3, L15. **"…the also discussed…"** is awkward and unclear. If it is meant that cosmic origins were a competing or reigning idea in the 1950s for the origin of stratospheric particles, that should be the way this statement is formed.

P3, L18-22. "**With the advent of laser sounding…volcanic nature of the stratospheric aerosol."** This is a run-on sentence that combines several ideas. Please break it into smaller, single-theme statements.
P5, L18. What is meant by **"following"** in the sentence: **"The following volcanic period…"**? Can it just be stated that the volcanic period in the post-Pinatubo background period will be examined in depth?

P11, line 3-4. Dates in text are different than dates in Fig 2 caption.

Fig 2. Please consider using calendar dates on x axis.

P11, L27. The sentence starting with **"In the tropics…"** is not a complete sentence and its meaning is unclear.

P12, L5. **"strong increase in the early phase"** Please define the early phase. As mentioned above, Figure 3 starts in mid-2004, so the earliest phase that can be resolved here starts in 2004, long after Hofmann's time line.

P14, L1-10. This material is fully contained in the Fromm et al. paper cited. It does not need to be duplicated here.

P14, L10. I think the major eruption of Pinatubo was 15 June, not 13 June.
P15, Section 3.5. Auth discuss a gradual increase in aerosol in 2006 and show a figure (Fig 7) highlighting a vertically spread aerosol feature in December 2006, peaking at ~20 km. They conclude that it is from Soufriere Hills eruption the prior May. The Prata et al. paper that auth cite can be cited for the plume altitude observed at Garmisch. Another paper worth investigating and citing is Vernier et al. http://www.agu.org/journals/gl/gl1112/2011GL047563/ who show aerosols in December 2006 reaching 30 km.

P16, L14. HYSPLIT is mis-spelled.

Figure 8. Annotating tropopause height on this plot would be very helpful in determining the tropospheric/stratospheric nature of the aerosol location.

Figure 8 discussion. Why is the 30 June profile attributed to Redoubt, which erupted in March, and not Sarychev Peak, which erupted in mid-June? It seems there was time for these aerosols to make to Garmisch by 30 June.

Figure 10. What are the altitudes at which the particles are released?

P17, L23. **"Propagation further southward was prohibited by a low-pressure zone over Northern Italy during the first approach."** This sentence is painful for this meteorologist to read. It is not appropriate to attribute responsibility for "prohibition" of air flow in the dynamics of the atmosphere. It may be better simply to describe the changing position of the parcels of interest and leave it at that.

P20, L25. **"rugged aerosol pattern"** The meaning here is not intuitive to me. What is "rugged" about the aerosol pattern? Do auth mean "ragged?" IF so, even that is not a clear description of the layer structure on which they are focusing. Please consider a more objective description of the feature of interest.

Figure 12 caption. The annotation **"TP17-20"** is consistent with the definition in the caption, but the parenthetical callout to the annotation, **"TP17-23"** is inconsistent.

P21, L11-13. **"The 19:42-CET measurement could not be evaluated in the tropopause region because of signal overflow within an aerosol spike. Smaller spikes were also present at the other measurement times, when the humidity was low to moderate."** What measurement is being referred to? There is no labeling in Figure 12 of "19:42 CET." Is there a 19:42 CET measurement that is not shown in Figure 12? Also, what is a "signal overflow?" Where in Figure 12 should the reader look to see what aerosol data are impacted by this overflow? Much clarification is needed.

P22, L4. **"315 h,"** should be "315 h;" (i.e., a semi-colon vs. comma)

P22, L13. Remove the comma from **"could,"**

P25, L20. Please add a third volcano to the eruption list. Puyehue-Cordón Caulle volcano complex in Chile erupted in June 2011, and polluted the UTLS with a remarkable amount of ash. The injection was into the extratropics, thus no northern hemispheric impact, but from a global standpoint, it must be mentioned.

References: Papers within a single first-author's name are not in chronological order.

Suggested References

Hervig, M. (1999), Stratospheric clouds over England, *Geophys. Res. Lett.*, 26(8), 1137–1140, doi:10.1029/1999GL900167.

Teitelbaum, H., M. Moustaoui, and M. Fromm (2001), Exploring polar stratospheric cloud and ozone minihole formation: The primary importance of synoptic-scale flow perturbations, *J. Geophys. Res.*, 106(D22), 28,173–28,188, doi:10.1029/2000JD000065.