Reply to the review of manuscript ACPD-2012-579

35 years of stratospheric aerosol measurements at Garmisch-Partenkirchen: from Fuego to Eyjafjallajökull, and beyond

Thomas Trickl, November 26, 2012

We thank both reviewers for very carefully reading the paper and the very detailed comments, and we apologize for the unbelievable amount of errors missed. We have been aware that the paper contains a large collection of different topics, and we tried to shorten some of the parts on material already published such as the biomass burning section. In particular, we try to keep the overview section as short as possible since our focus is on the background period since 1997 and the recent revival of volcanic activity. We think that the comparison of our results with those for low latitudes is essential and new: We do not see the trend observed above Mauna Loa after 1997 and this difference must be discussed. Eyjafjallajökull has been the motivation for this effort, and the case study for this eruption, therefore, is an essential part of the paper. However, we agree that the description of the tropospheric observations must be shortened, although some interpretation is retained.

Changes were made in all but very few cases.

Reply to Reviewer 1 (M. Fromm):

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, □ulphate, or dust) that are above but in proximity to the tropopause.

Historically, the choice of TP + 1km as the lower integration boundary was dictated by the poor range-resolution of the old data-acquisition system of 600 m. Since we describe the entire series it is important to stay compatible. There is also some uncertainty on the tropopause height that is derived from measurements 100 km away from here. I added a few words explaining this.

Of course, there is some uncertainty about UTLS particles. After evaluating thousands of O_3 profiles I cannot remember cirrus clouds to penetrate the range of ozone rise. However, tiny fractions of ice particles cannot be excluded to exist 1-2 km above the TP, as sometimes indicated in the 313-nm backscatter profiles. They would be better discernible at 532 nm. A discrimination against smoke is not easily possible.

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."***

We have not been attempting to sell this as a trend, although the text, indeed, looks misleading. We have pointed out the missing significance of the trend. I completely changed the text and removed Fig. 2 to reduce the emphasis on this analysis.

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?

As mentioned, there is no clear trend. But if there is a minimum, it must be within the 2002-2004 period, and not earlier (as now stated). This can be concluded without using other data.

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.*

The eruption periods are series of a few days for which the listings indicate uninterrupted activity. This has been specified in the figure caption! "Underestimated": I changed this sentence to avoid this word.

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.

Changed to a more modest claim! It did not want to extend the data to earlier times since this would be even more time consuming than extracting the data from the listings I used. The tropics are not our business anyway and there is also other work focussing on this.

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.*

I changed the sentence.

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.*

I am truly impressed by the high number of volcanic eruptions in the 5-10-km range! The entire part on the comparison of the two latitudes was rewritten.

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.

I fully agree that the section on biomass burning is too explicit since the cases are already published. I shortened the subsection and made some changes.

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].*

I have been aware that PSCs are somewhat outside the rest of this paper since they cannot contribute to the budget. In Fig. 1 they are well hidden in the noise, and it does not make sense to mark them. However, the observation of PSCs that far south is special and deserves a few words. In contrast to the biomass burning studies, these observations have never been published, and we do not plan to publish anything in the future. This subsection is very short anyway. I removed one of the two figures and shortened the text. Thank you for the references, they are now included!

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.*

The citation was made a few lines above this. However, this paragraph is, indeed, confusing, and I rewrote it to make the message clearer. I also added an arrow in Fig. 1.

*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.*

This phrase refers to the Chisholm case, I added some clarification. The evidence becomes clear from the numbers specified ("as high as ....")! The Pinatubo rise is discussed in the following paragraph! I now shorten this part since detailed information is already published.

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…"***

Thank you for this comment! I changed the paragraph.

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.

The similarity between intrusions and the plume observations in Germany is striking and deserves a few words. This could mean that the geometry of the ash layers is the same as for intrusions (transversely sloped), here even some time after the intrusion ended. I added one a few words, but I do not want to go into more detail. The description of the findings for the second eruption period in May was deleted since it is not relevant to this study.

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?

I admit that the visibility of the curve for April 20 is limited due to the masking by those for April 17 and 23. However, it is clearly visible! Also, the stratospheric background level is changing considerably from day to day. I tried to improve the figure and specified the step positions and the uncertainty level in the text. Please, note that the variability of the profiles diminishes for higher altitudes, and the upper end of the background layer at about 23 km is clearly discernible which adds a lot of confidence about the quality of the analysis. We cannot display the full range because this would result in less visual resolution for the range below 15 km.

Minor/Technical Concerns

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

Deleted

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.*

Changed

*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.*

I am surprised about this: It is completely true! I followed the modification suggested by Reviewer 2.

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?*

It is the volcanic period starting in late 1996. We cannot see any clear signature before this. I slightly modified the text.

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

The dates in the text are correct. I removed Fig. 2 in order top prevent a wrong impression about our view.

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

As mentioned: Fig. 2 was removed.

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

A typical error of text editing on a computer! Changed!

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.*

The statement refers to the figure. Hofmann´s time line is out of reach without a tremendous additional beyond the scope of this paper. It is not the purpose of the paper to do the analysis for Mauna Loa. We just want to study the differences between the mid-latitudes and the tropics since this is what we can do with our own data. And for the mid-latitudes, the volcanic listings for 2004-2011 are very likely sufficient.

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

I agree and deleted whatever is not necessary. The focus is on a brief review since the most important part of the paper is devoted to what is new: the most recent volcanic period and the question about a threshold for significant TST.

P14, L10. I think the major eruption of Pinatubo was 15 June, not 13 June.

I re-examined the reports and found that the strong part of the eruption started even on June 12, which would be the most important date for the trajectory assessment. However, it is true that the maximum was reached on June 15. In any case: I deleted the entire paragraph because the information is already published.

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.

Thank you for these hints. I had not inspected Vernier´s figure with this issue in mind. It is nice to see how the centre of the plume rises to the altitude where we observe the peak! I modified the text.

P16, L14. HYSPLIT is mis-spelled.

Corrected

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

This was not attempted because of the considerable spread of observation times.

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.

This is a very good point. There are two reasons: the altitude ranges of the plumes are quite different and our inspection of the Sarychev plume in the CALIPSO images confirms the plume altitude of 15-20 km on the way from Asia to North America where it arrived on June 22. This is described further below in that chapter. I added a few words to point out the agreement.

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

I think that the figure shows atmospheric columns. No information was distributed, and A. Stohl could not tell me the details after the long time that had already passed.

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.*

My apologies, I am a physicist! I tried to make some acceptable change.

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.*

My suggestion: aerosol layers.

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.*

Changed

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.*

The date is April 19, and DIAL measurements are shown in Figs. 13 and 14, not in Fig. 12! I added "Fig. 13" following "an aerosol spike" (now Fig. 11).

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

Changed

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

Deleted

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.

This eruption and its role on air traffic are well known! However, the maximum eruption altitude is just 13.7 km, reported for a single day. We doubt an important role for our observations. In view of the amount of detail in the paper we decided not to mention it.

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

I think that paper of as single author are treated separately. I am going to ask the Copernicus team.

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.

Thank you for these references! They are now included.

Reply to Reviewer 2:

The Garmisch stratospheric aerosol record is the longest record available in the European sector, and this record has made a number of important contributions to our understanding of the dynamics and measurement of stratospheric aerosol. This manuscript extends that record up to the present and also presents a smorgasbord of additional miscellaneous phenomenon measured at Garmisch, polar stratospheric clouds, the stratospheric impacts of Pyro CBs, and most recently signatures of the

Icelandic volcanic eruption, Eyjafjallajökull, in 2010. The problem with this approach is that these various phenomenon are not all that related, except that they can all be measured with a lidar. Thus the paper lacks a strong scientific focus. The paper and the measurement record would be well served if the authors focused on one major topic. I suggest the long term stratospheric aerosol record, and then showed how the recent volcanic record is captured, or missed, by the Garmisch measurements, how somewhat random events such as polar stratospheric clouds, pyro CBs, tropospheric volcanoes, appear in the record, and finally to spend more time to explain the factor *of 5 or more increase in integrated backscatter between 2008 and 2009, which has then remained at levels similar to the levels observed three years post Pinatubo or El Chichon, but with much larger variability. This change is much larger than the 3-5% per year increase suggested by the Mauna Loa record. The authors focus as much space on 2004-2006 at Garmisch, when that change was rather minor, as they do on the much larger and more recent changes. The authors are also on a bit of a mission to refute the claims that coal burning in China may be impacting stratospheric aerosol, and to support the alternate claim that the changes are due to low level volcanism; however, their arguments along these lines are not based on the Garmisch measurements, but on other measurement records, which have already been discussed along these lines (Vernier et al., 2011). Thus the paper gets distracted from the strength of the Garmisch measurements.*

I would very much like to see this stratospheric aerosol record presented and discussed for what it contributes to our knowledge of recent changes in stratospheric aerosol, what it contributes to the long term evolution of stratospheric aerosol, and how random events, polar stratospheric clouds, Pyro CBs, tropospheric volcanoes, do not really impact the record, while a series of low level volcanic eruptions, do impact the record. The authors are not very discerning about which volcanoes are included in their list at the bottom of Fig. 1 and Table 1. They should focus on only those volcanoes with clear stratospheric signatures at Garmisch, while at most mentioning those without clear signatures or with tropospheric signatures at Garmisch. If, however, the authors choose to maintain the current approach of a rambling discussion of a smorgasbord of Garmisch measurements, and a discussion of recent controversies in stratospheric aerosol, and their opinion of these controversies, based on measurements by others, then I am not sympathetic to publication. In the latter case the paper should be recast into a focused discussion solely related to this latter point about recent volcanism. Then the paper can be judged based on the merits of that argument, while being aware that these points have already been addressed by others more responsible for these other measurements. In what follows I offer specific comments made while reading the paper. Some of it is editorial, while the rest is supporting the points above. I do this in the order of the written manuscript. Since no line numbers were provided, I indicate position with page number.paragraph number, and let the authors find the text in the paragraph based on the comment. Partial paragraphs at the top and bottom of a page are counted for that page.

It has always been our idea to focus on the role of more recent volcanic eruptions, and this is stated. However, this principal part is accompanied by a short overview of the entire series and the discussion of a few minor events. We try to shorten this overview as much as possible, including the removal of figures.

Abstract: Stick to what is new in this paper and its contribution to the science. Save for the discussion subjects such as the reason for the increases in stratospheric aerosol recently observed and the record of recent volcanic eruptions. Focus on the Garmisch measurements.

I removed the sentences on the influence of the quasi-biennial oscillation (published) and the Asian influence.

3.1. Revise as follows, With the advent of laser sounding (Fiocco and Grams, 1964) and regular balloon borne (Hofmann et al., 1975) and satellite (ref for SAGE II) observations, a time series could be obtained. For recent reviews see (Deshler et al., 2006; Deshler, 2008). The measurements have covered a number of important eruptions and have led to a clear : : :

Good suggestion! The sentence is too long indeed and I revised it.

3.1. Some mention should be made of the sulphur gases which are transported to the stratosphere and maintain the non-volcanic fraction of stratospheric aerosol.

Added

3.2. What does the word powerful add, here, and elsewhere? Such strong adjectives generally do not help the scientific content of the statement, but rather confuse it.

Changed

4.1 "rather remote"? Does this mean unpopulated?

Clarified

Pg 4. Given the focus of the paper, described in the last paragraph on this page, on a fairly long period of low level volcanic activity with stratospheric impacts, the extensive material and references on the eruption of Eyjafjallajökull, a tropospheric eruption, are misplaced.

I understand this view. However, we follow the conventional sequence: The introduction first describes what is already known and then, at the end, what the paper is focussing on. Although the Eyjafjallajökull eruption was mostly tropospheric: It has been the most important atmospheric research highlight in Europe in recent years. Thus, at least some of the other work in this special issue must be given credit to.

Section 2/Figure 1. Some mention should be made of the reason for the data gap, 2002-2004.

A sentence explaining the gaps was added.

8.2. Delete 'in the preceding publication'.

Deleted

9.2 Delete, 'Some of these events are discussed in the following section.'

Deleted

9.4. This paragraph is rather speculative. There are no Garmisch data mid 2002-2004 and the last points in 2002 were quite low. A fairer statement would be that the measurements during 2000/2001 were higher than those during 2004, thus the increase observed after 2004 was not apparent in the 2000-2002 data.

We doubt that there is a serious increase. The text had been misleading due to much detail on the analysis of the rather insignificant trend and I changed the entire part on the latitudinal comparison. I also removed Fig. 2 in order to minimize the emphasis on the "trend" result.

9.5 What two latitude ranges? Why are they extensive? Underestimated by whom? Why are the number of eruption periods impressive? It is also not so obvious the importance of the eruptions between 5 and 10 km. Do these influence stratospheric aerosol? Perhaps through increasing the atmospheric sulphur burden, but probably not directly, and their sulphur contribution would have to be compared to the global flux of sulphur to see if it is significant. The more important eruptions are the ones above 10 km, and there were a few days in mid 2005 for this case. But the goal of this figure is not clear. Recall the increase at Mauna Loa began in 2000, so if this figure is to provide information about that record it should extend back to 2000. Overall the extensive focus here on this short Garmisch record with minimal changes is misplaced. The later, larger changes are of more interest.

I completely changed this part. However, this is the "Results" section and more information on some of the issues is found in the "Discussion" section. Indeed, the importance of the eruptions between 5 and 10 km is not that obvious. However, we cannot exclude that some of this air can reach the stratosphere in the vicinity of the subtropical jet stream where pronounced vertical exchange is known to exist during a major part of the year. In the mid-latitudes, 10 km is a typical winter-time tropopause altitude.

Of course, an extension of that figure back the late 1990s is desirable. However, such a job is beyond my present capability. For the discussion our series the period 2004-2011 is acceptable since major volcanic activity obviously did not start before 2006. A closer look at the tropics including the years before 2004 would be interesting, but this is not our main business. What is important here is to show that there are differences in the annual number of eruptions between the tropics and the mid-latitudes that resemble the difference in the observations.

Table 1. It would be nice to include latitude and volcanic explosivity index.

This had been in my mind before the first submission, but I had problems in finding a source. This source is well hidden, but exists of course: A column was added!

10.2. Where is it obvious that the number of mid-latitude eruptions were few prior to 2005, and then increase, Table 1, Figure 1, Figure 3? According to Figure 3 the number of high latitude periods with h > 10 km has remained flat. Next sentence, what is the early phase? Again the authors have not shown the data back to 2000 and are basing these statements on perhaps 5 high latitude eruptions which probably did not influence Mauna Loa's latitude much, if at all.

I cannot guarantee that the number of relevant mid-latitude eruptions in Table 1 before 2000 is complete since there was no chance to look at all volcanoes. But the table contains at least as many important eruptions as listed in other publications I read. Our time series outside the data gaps does not reveal significant signatures before late 2006. This is now clearly mentioned. The trend for h > 10 km is not really flat: The annual number of mid-latitude eruptions at least doubles! What matters are the strong eruptions starting in 2006. The VEI data are now also mentioned.

The next statements regarding the tropical eruption are not supported by tables or figures, and are a bit rambling. In fact this whole section is off the main topic of this paper, which is the Garmisch measurements. It would be much more interesting if the authors could identify those volcanoes which may have influenced the Garmisch measurements, and could document/explain the changes observed in that record, rather than a broad discussion of all the volcanism in the last 10-15 years, and then challenging the conclusions of Hofmann et al. (2009). Those conclusions have already been challenged along these same lines by others, e.g. Vernier et al. (2011).

Indeed, the main focus is on the final ten years of our series. But we see some discussion of the differences with respect o the tropical measurements as an important and new task. The text was re-organized to make this view clearer. The work by Vernier et al. has been discussed in Sec. 5, but I added a citation also here. It is important to mention that our study was not stimulated by any analysis elsewhere beyond the Hofmann et al. paper.

We think that the volcanoes relevant for the observations here are those in Table 1, in particular those with VEI > 3 (except for Shiveluch since the 2001 eruption was very likely less significant than indicated by VEI = 4; this was explicitly discussed with M. Fromm who sent me a list with arguments).

Section 3.3. This section reviews several papers by Fromm et al., in which theGarmisch measurements played a role. Thus all of the detail provided here is not necessary. It has been published. Nor is Fig. 4. The suggestion that fires may become more abundant in the future and an investigation of the past record on this point is, of course, interesting, but again is not what this paper is about, or should be about. The authors should stay focused on presenting their measurements. I would recommend just a short description of the fires locations and the aerosol plumes tracking to Garmisch, then pointing out in Fig. 1 when these events were observed. From Fig. 1 the two main points of the authors, that even large fires contribute little to the overall stratospheric burden, and that their time frame of influence is quite limited, will be immediately obvious.

It is obvious that this subsection is longer than needed for an overview. This work was already published. The focus is more on the impact of moderate volcanic eruptions. I strongly shortened this subsection and, together with that on the PSCs, integrated it into a subsection on non-volcanic sources. The two fires discussed are now marked in Fig. 1.

Section 3.4. This event while interesting, contributes nothing to the overall stratospheric burden and is even more limited in time. Two events in 40 years? I don't see the value for this paper of section 3.4 and Fig. 5 & 6.

These events are not important on a statistical basis, but they are part of our series and it is special that PSCs can be observed that far south. In contrast to the biomass-burning studies we have never published anything about these observations, and no future publication is planned. I shortened this subsection and removed one of the two figures.

Section 3.5 A better title would be - Increase of volcanic activity 2006 – 2012. That way it is consistent with sections 3.1 and 3.2. If could be more interesting to point out the presence of polar stratospheric clouds as they appear in the long term lidar aerosol record.

Good suggestion, I changed the title!

12.2. We do not need all this detail? A simple statement that minimum temperatures were -70_C will convince the reader that a PSC was not observed.

I shortened this part.

12.4. Change to. We now believe that a layer from the Soufriere : : :

Changed

13.1. What is the final round? : : : wide vertical distribution : : : here and elsewhere? What kind of distributions (altitude, size, ???)? The authors intent should be specified. 13.2 Join with previous paragraph and begin. "The only other possibility is the eruption : : : 18 km; however, this eruption was short : : : south." Last sentence is unnecessary.

"during the final round": Deleted (not necessary); rest: modified.

13.3. This paragraph doesn't seem to be related to the Garmisch measurements and should be dropped. Some of the references could be used to qualify the statement, "Because of the strong eruptions occurring in spring 2009: : :"

Changed

13.4. I don't understand this sentence, "The scattering ratios (not shown) outside narrow structures, converted to 532 nm, grew from 1.1 to 1.4 during that period in agreement with the *results of the NDACC lidar before and after the gap." The scattering ratios from the NDACC lidar are not shown either, to my knowledge. What is the intent? Perhaps the integrated backscatter could be shown to be in agreement with the NDACC lidar before and after using Figure 1.*

I removed this sentence since quantitative numbers are given further below anyway.

13.5 – end of this section. I do not understand the point of showing two examples for how the DIAL system could be used. One is sufficient. The discussion primarily just consists of a description of the figures with some numbers added, but doesn't add anything beyond the figures until we get to the last paragraph of section 3.5. I recommend removing Fig. 8 and the discussion of Redoubt. Use Fig. 9 as the example of the utility of the DIAL system, but skip the figure detail and focus on the last paragraph.

I do not agree. This is an interesting period with two subsequent eruptions that can be, nevertheless, distinguished in our observations. I am not interested in presenting the performance of the DIAL although I am glad that the data quality is reasonable. Some description of figures is mandatory. In the Sarychev case is rather explicit since the numbers matter in comparison with other work (of course these numbers are interesting mostly for lidar specialists). In addition, the description of the decay is important for the subsequent Eyjafjallajökull case and the magnitude of the stratospheric background in spring 2010.

Section 4. After roughly 2.5 pages and 3 figures for the period 2008-2011, the most interesting period in the recent Garmisch record, the paper spends 6 pages and 8, out of 19, figures on Eyjafjallajökull, a clearly tropospheric eruption, which was observed for just one month at Garmisch. This emphasis is misplaced, and this section should be shortened considerably. Never the less, I read this section and have the following comments.

I understand this concern and major shortening was attempted. However, the Eyjafjallajökull eruption has been of enormous impact on Europe and has strongly motivated the present study. Our paper was prepared for the related special section. Thus, Eyjafjallajökull must be the principal topic, but our night-time measurements are too sparse for a relevant tropospheric study (the laser of our high-spectral-resolution lidar was damaged during that period). Nevertheless, we want to make the tropospheric part of our observations available, being an EARLINET station and looking at the event as a whole. We shortened the description of the tropospheric analysis (SO_2 paragraph, tropospheric paragraphs in general, Fig. 17 and the related text).

Our main goal in this paper has been to look for a stratospheric impact, this being something nobody else has done. The upper-tropospheric observations and the lower stratospheric ones are closely related since the observations suggest a combined UTLS hump. The conclusions in Sec. 5 were extended.

16.4 – Why is Fig. 10 introduced after Figs. 11-14? I am not convinced that presentation of all these trajectory calculations are necessary.

Figure 10 is placed here since it assists the introduction of this case study. Fig. 17 was removed as mentioned, the subsection on trajectories was re-organized and the tropospheric descriptions considerably shortened.

17.2. Where is the 14.3 km layer observed? It is not apparent on any of the Figs. 11-13. This emphasizes the last statement in this paragraph, because so far nothing here leads me to believe in a stratospheric impact.

Due to the presence of four curves in a figure reduced in size it is, indeed, difficult to follow the different profiles! The step below 14.3 km is clearly visible if you plot the corresponding profile separately. The stratospheric background is rather low and forms a broad hump maximum at about 18 km. Just the magnitude changes from day to day (the far-field calibration based on the Rayleigh return is highly accurate so there is little doubt about this variation). Due to this general shape the additional contributions can be clearly resolved. The "UTLS humps" are stronger than anything observed during the measurements before the eruption! I modified the figure for more clearness, also adding some guess of the stratospheric background above the tropopause guessed from measurements before the eruption. The individual thresholds are listed in the text.

17.3. What am I missing? According to Table 2 Keflavik and Munich have quite similar humidities. The numbers for the first 5 days are: Keflavik (47 60 40 38 76 69 79 37 23 33 57 85) and Munich (52 36 93 69 89 57 81 30 36 40 32 63). These numbers do not imply that Keflavik was significantly moister. In fact one could argue the opposite, but generally they are quite similar overall. What range of humidity is favorable for aerosol growth. Which really isn't aerosol growth as much as aerosol deliquescence, moisture the aerosol will lose when it reaches the next dry area, so the point of this discussion is confusing. For similar reasons I am not sure the point of showing Fig. 14 which is not in RH, but in H2O number density and just serves to show that the air over Garmisch was dry on April 19 as already evidenced from Table 2.

I obviously forgot to mention what time periods are covered by my statements. I completely modified this part, starting now with the aerosol observations.

The humidity in the tropopause region is horizontally rather inhomogeneous and a frontal system approached from the south-west in the evening. Since the Munich radiosonde station is 100 km away (N to NNE) this measurement is important. As to RH: We give the Munich 100-%-RH curve for comparison! This is better than converting our measurements to RH by assuming the validity of the Munich T profile for our site.

17.3. Arguing that the narrow layers in Fig. 13 near the tropopause are cirrus is not hard even if the DIAL water vapor measurements do not show a humid layer. Notice the significant difference between these layers and the previous volcanic layers shown in Figs. 11 and 12 which have widths of 0.5 km or more. Here the layers are a few hundred meters at most. In addition the backscatter coefficient is different by an order of magnitude according to the axes labels, which is confusing. According to the axes labels the majority of measurements in Fig. 13 would appear below the 0.01 x 10^{-6} sr^{-1} m^{-1} *line in Fig. 12. Yet they are both 532 nm measurements. The harder question is whether the cloud formed on a volcanic aerosol layer and that is not well shown here.*

The argument is not hard, and we have never made a full claim. I am also happy if the spikes are related to pure ash, I just forgot to write this! However, as recognized by you, the width resembles more a cirrus than typical ash layers. The entire part was modified.

I re-examined the data: The 10^{-7} in the axis title is a strange misprint not present in the earlier versions of that figure, 10^{-6} is correct. The order of magnitude does not make sense anyway! As to cirrus: As pointed out explicitly we do not argue at all in favour of cirrus. It is too dry! I now discuss both possibilities.

22.3. This paragraph is not derived from this paper, but is again a discussion of a major sub text of this paper, ascribing any recent changes in stratospheric aerosol solely to volcanism, which has been the subject of other work. In this regard the last sentence is unusual. What is it based on? The *tropical tropopause is in the 16-18 km range, yet no tropical volcanoes listed in Table 1 come anywhere close to this level. The only ones which penetrate 15 km are all high latitude volcanoes.*

I presume that you discuss the second paragraph on p. 27 (third paragraph of the Discussion). After the presentation in Sec. 2.3 it is mandatory in the Discussion section to relate our findings to what was concluded in other work. In Sec. 3.2 we now specify the number of eruptions reaching at least 18 km. The threshold for Fig.3 of 15 km was chosen because of the uncertainties in eruption and tropopause heights. I made some changes.

Of course, despite the latitudinal comparison our paper is not on the tropics. Thus, Table 1 just lists the mid-latitude eruptions.

Fig 18 is a nice picture, but is not appropriate here. Particularly considering the very next sentence.

Since we do not try to relate our observations to the Grismsvötn eruption I agree to discarding the figure. I really regret this! If you ever want to use it for your own purpose: there is a copyright on it, and I had to pay for the license.

How is the quasi-horizontal leaking and Figure 19 related to the results presented so far? This is the first introduction of this new point just one paragraph before the end of the paper.

I had already removed this paragraph and Fig. 19 due to a request forwarded by the editor. I think that this example is rather important since all the publications talking about the "mixing zone" in the lowermost stratosphere seem to miss the opposite possibility of mixing below the tropopause. Maybe I use the figure somewhere else later on.