Authors response on "Simultaneous observations of NLC and MSE at midlatitudes: Implications for formation and advection of ice particles" by Michael Gerding et al.

Anonymous Referee #2

We thank the reviewer for the careful reading and the helpful comments. Answers to the specific comments are given below (in italics). New line numbers refer to the manuscript with marked changes.

The authors combine two datasets from co-located instruments. A Rayleigh lidar observes NLC which is a direct measure of ice particles in the mesopause region, while a VHF radar observes mesospheric summer echoes which are by complicated physics linked to the presence of ice particles as well. Both phenomena are known to be closely related from detailed studies at polar latitudes. Both datasets from a mid-latitude site used here by the authors were described in detail before, so no new data is presented. As there is scientific interest regarding the occurrence of NLC at mid-latitudes, it seems nevertheless worthwhile to undertake this combination of the datasets.

Many thanks for this comment. We truly estimate the combination of both data sets to be worthwhile. This has rarely done on a large data set before, and never for midlatitudes. We think to gain additional knowledge from this combination, even if the process for MSE is indeed complex and does not only involve ice particles.

However, the study presented here is not as extensive as the studies at polar latitudes. Basically it is reduced to the comparison of three layer parameters: the lower and the upper layer edge of simultaneous NLC/MSE and their centroid altitude. The only relevant result of this study is a difference of 500 m between the upper edges, which differs significantly from 3.3 km found at polar latitudes. Even the authors do not seem to be surprised, though. They attribute it to reduced thickness of the MSE layer – but it is not clear if this has been shown before, and they don't think it is necessary to show it here as well.

The reviewer is right that the MSE data can also be described by their thickness that is on average lower compared to polar latitudes. For the comparison with the NLC layer we found it reasonable to differentiate between upper and lower edges because the layer thickness on its own does not say anything about the relation to NLC altitudes. Kaifler et al. (2011) were able to do a more detailed analysis based on a much larger data set from high NH latitudes, while Klekociuk et al. (2008) made an initial study on a smaller data set from high SH latitudes. Given our limited data set and the instrumental changes of the OSWIN radar we hesitate to do an extensive analysis like Kaifler et al. (2011). Edge altitudes are comparatively robust against instrumental changes, while, e.g., occurrence rates may not. Furthermore we wanted to focus on potential differences to higher latitudes, which we mainly found in the upper edges.

We have tried to sharpen the description of the relevant results in different parts of the manuscript (see below).

Their only conclusion from the study is that advection is the main process for NLC occurrence at the observation site. This is by no means a new conclusion. From Gerding, JGR, 2007: "We conclude that NLC at midlatitudes are strongly coupled to the advection of

preexisting ice particles from northern latitudes." and Gerding, GRL, 2013 "Comparing NLCs and ambient winds, we find strong indications for the meridional wind (advection) being the main driver for NLC occurrence above our site."

Yes, we have truly claimed this before. But there are other, partly newer publications that propose local processes. During the analysis of the NLC/MSE data we found this additional indication for advection, supporting our previous papers. We see this observation and conclusion as relevant, especially since these combined observations are only possible with our (still unique) combination of instruments.

The authors claim to undertake the first statistical study at mid-latitudes. I acknowledge that this is a difficult task, and with their instrumentation they are also the only ones able to do this. The reason is that the NLC occurrence frequency is low, so with a lot of effort, only a very limited dataset is to be gained. At the same time, this makes these measurements highly valuable, and they should be treated accordingly. I fear that with 64 or 67 hours of data available to this study, it does not qualify to being statistical, or to being representative for NLC. The authors are not clear about the number of events or the number of independent profiles, but there is reason to suspect these numbers are low.

We are happy that the value of our observations is acknowledged. We apologize if the term "statistical analysis" is misleading here. We have replaced it in the revised version (e.g. "comparative" (p. 1 l. 3), "vertical distributions" (p. 3 l. 15), "on average" (p. 8 l. 13)). We now mention the number of 31 days with NLC/MSE in Section 2.4 (p. 7 l. 16). These events are representative for all NLC in terms of their altitude structure, as described in Section 5 (p. 11 l. 31 – p. 12 l. 6 , see also comment below). Of course, they may not in terms of brightness or diurnal variation. Furthermore, the MSE during simultaneous NLC are not representative for all MSE, as already described in Section 5. There are many high and/or weak MSE not represented here. But these MSE just support the presented conclusion about the formation processes.

There is one flaw in their discussion regarding the mean centroid altitude compared to their previous NLC statistics. I think they either made a mistake or the dataset cannot be considered to be representative. A large percentage of the NLC dataset (two third) was not included in this study, which is sad, and the reason wasn't explicated in sufficient detail.

We apologize for the mistake about the mean NLC height in the beginning of Section 5. Indeed, the mean peak height here is 83.3 km. Many thanks for making us aware of this flaw in our discussion. We have revised and extended this section (p. 11 l. 31 – p. 12 l. 6), but we still state that the presented data are representative for all NLC. In the 2013a paper (data 1997-2011, nighttime only) we gave a number for the mean centroid altitude that is typically 0.2 km below the mean peak altitude. The mean peak altitude is easier to identify, and is 82.8 km for all NLC 2010 – 2016. The mean peak altitude for the selected days of simultaneous MSE is 83.0 km (mean centroid altitude 82.8 km). From this data set the very weak NLC profiles (beta < $0.3*10^{-10}$ /m/sr) are removed, e.g. during beginning and end of the event, as well as some profiles with very low NLC (80 km and below), because here the ionization is reduced and MSE are less likely.

For this study we needed to remove NLC during nighttime (or low solar elevation, i.e. ionization) because of missing MSE. Furthermore we excluded the faintest NLC (beta < $0.3*10^{-10}$ /m/sr) because of typically bad SNR and therefore unreliable edge detection. We explain this in more detail in the revised manuscript (p. 7 l. 18-20).

In my impression the potential of the data shown was not fully exploited. The authors dedicate one section to the display of four cases with varied, sometimes intriguing morphology, but no physical explanation is offered. It is therefore not clear why they are shown at all. The following statistics of lower edges makes the reader wonder if the morphology with a double layer is correctly represented. Especially the extreme cases of the statistics would be worth taking a closer look at, e.g. when the MSE lower edge is located 3 km below the NLC lower edge. I also doubt that the statistics of lower and upper edges result in the same correlation coefficient, as they look different to me.

We agree that the events presented in Figure 2 are worth further analysis, and we thank for the encouragement. Nevertheless, this is outside the scope of this paper. While showing very interesting dynamical structures, detailed analysis of these cases needs further information about ionization (electron densities) and turbulence. Wind data is available only with limited temporal and spatial resolution, while temperature data is completely missed for most cases. Figure 2 is presented to make the reader aware of this highly dynamic behavior of NLC and MSE – and the limitations for detection of layer edges from independent, asynchronous instruments. We have improved the description in the revised manuscript (p. 5 l. 20 and p. 6 l. 13 - p. 7 l. 8).

Figure 2 shows some cases with larger differences of NLC and MSE edges. These are worth further analysis, but so far observations of electron density and turbulence are lacking at our site. Other large differences occur for technical reasons like different FOV sizes or asynchronous data.

We have double-checked the correlation coefficients. The "outliers" are mainly single profiles, while the majority of events (dark color in Figures b)) are along a line.

Another critisicm is that the authors invoke an incorrectly simplified image of PMSE physics in particular, by stating that NLC are created by large ice particles and MSE are created by small ice particles, or even simpler, that lidar observes large and radar observes small particles. Here and there some references to our understanding of the physics of PMSE are interspersed, mostly when some explanation for some discrepancy is needed.

We thank the reviewer for making us aware of the insufficient explanation of MSE physics. We have added some explanation, e.g., in the beginning of the Introduction (p. 2 l. 6-8, see also below) and mention this topic also in the Abstract (p. 1 l. 5). The potential influence of the MSE physics on our results is, e.g., more clearly described in Sections 2.3 and 2.4, now (page 7).

Questions and comments regarding science are following sorted by line numbers. A second set of comments with more technical corrections is appended.

p. 1, l. 1 This is the very first sentence, and it is not very precise: radar measurements are not a direct observation of ice particles, you shouldn't make such a statement in the very beginning. And they can also be observed optically by eye or camera. And why the focus on ground-based observations here? Its not yet clear what you are after.

We have changed the first sentence to "We have combined ground-based observations of ice particles in the summer mesopause by lidar (then often called Noctilucent Clouds, NLC) and radar (then called (Polar) Mesospheric Summer Echoes, (P)MSE) for a first comparative study on ice cloud altitudes at midlatitudes (Kühlungsborn/Germany, 54° N, 12° E)."

p. 1, l. 2 Second sentence: that's too much of a simplification, reality is more complex

We have added "but require sufficient ionization and turbulence at the ice cloud altitudes" at the end of the sentence.

p. 1, l. 4 "allows for some insight" – yes, but that's now a very complicated task

Phrase is changed to "…provides some rough information about …".

p. 1, l. 5 I feel the need to object to the "statistical study". It is only 67 hours of data. It is more a compilation of cases, but not statistics.

We have deleted "statistical", now saying "comparative study" (p. 1 l. 3) and avoid this term with respect to this study (cf. above).

p. 1, l. 6 MSE is not a direct measurement of ice clouds

Yes. Limitations are now mentioned, e.g., in the second sentence, in the Introduction and in Section 2.3. See also below.

p. 1, l. 18 and from space. You mention "stations", which I read as ground-based, but then cite results from satellite observations in the second sentence, so it's worth being included in the first sentence that there are also satellite observations.

We thank the reviewer for the careful reading. We have changed the sentence to "Noctilucent Clouds (NLC, also known as Polar Mesospheric Clouds, PMC) and Polar Mesospheric Summer Echoes (PMSE) are observed since several decades mainly in the polar regions by ground-based and space-based instruments as well as by human eye […]." (p. 1, l. 19-22)

p. 1, l. 22 this might be the most suitable place to explain in necessary detail the relation between lidar-observed NLC and radar-observed (P)MSE, and not only give the citation. The differences are not restricted to occurrence and vertical extension, but the physical mechanisms are very different. As you only give this information piece by piece throughout the manuscript, you might want to take the chance to make this very clear here. Then the reader won't be misleaded and then be surprised while reading that it's in fact more complicated then you had hinted.

We added a first sentence on the complex origin of PMSE here ("Later on it was revealed that PMSE additionally require sufficient ionization of the ambient air to get the ice particles charged and turbulence to produce plasma structures for scattering of the radar wave." p. 2 l. 6-8) and provide information about the implication of these requirements throughout the manuscript.

p. 2, l. 12 already here it would be useful to have the physics of PMSE explained

We added here "ionization and turbulence provided".

p. 2, l. 14 that's not very obvious. It could have been created within the NLC layer for all we know

The formation of ice particles from condensation nuclei happens most likely at the mesopause, i.e. above 85 km, where supersaturation is largest (e.g. Rapp and Thomas, 2006). At these altitudes NLC are extremely rare. Typical altitudes of NLC and MSE are mentioned on p. 2 l. 4/5.

p. 2, l. 25 equating “local ice formation” with “observations of PMSE” is too much of a simplification

We changed this part to “Li et al., JGR 2010, revealed from PMSE observations average ice particle radii being larger above 85 km than below. This can be explained by …” (p. 2 l. 34/35)

p. 3, l. 16 you should motivate why the diurnal variation of NLC is of relevance for this paper if you cite it

We deleted this part of the sentence.

p. 3, l. 23 do you not normalize to density? I thought the common technique is to normalize to density and then take the ratio of the Mie scatter to this?

The reviewer is right. We have added this information (“…from the aerosol backscatter normalized to the molecular backscatter, the molecular backscatter cross section, and a reference air density to quantify the cloud brightness”, p. 4 l. 5/6).

p. 3, l. 27 i.e. smoothed with 15 min width?

Yes. We have changed the phrasing.

p. 3, l. 32 now you proceed with the radar. I suggest subsections per instrument. You started the paragraph by mentioning the commissioning of the instrument in summer 2010, and with no word you give any numbers on observations statistics!

Many thanks for the suggestion of subsections. The observations statistics is given in the last subsection 2.4, which is now introduced earlier in an overview sentence (p. 3 l. 23-25 and p. 7 l. 10-24).

p. 5, l. 1 There is a break here. There was a description of the radar dataset and then, with no subsection change, the text continues with different types of agreement between the observations

We made a new subsection here and added a short introduction (p. 5 l. 20).

Fig. 2 it might help the reader to indicate times with solar elevation above 5 deg, as it seems to be important to PMSE occurrence

Many thanks for this suggestion. Done.

p. 5, l. 7 “often filled the same volume” the expression is not elegant, it’s not very precise and it’s not even true when I look at the figure!

We changed the phrasing to “showed good agreement” and give more precise information thereafter (p. 5 l. 28-30).

p. 5, l. 12 the observation of MSE is not a detection of ice particles, once again

Ice particles are necessary for MSE. Therefore we can conclude from the presence of MSE to the existence of ice in the observed volume.

p. 5, l. 23 especially Fig. 2d seems to be a case with lots of features sparking many questions regarding the physics. No explanation is given! That’s a bit frustrating to the reader.

We agree that this event is very interesting and has the potential for further studies. Unfortunately we have no electron density and turbulence measurements available to explain, e.g., the gap in the MSE at 3:30 UTC. Examination of gravity wave dynamics

would be very interesting, but is beyond our scope for this paper. To make the reader aware of the high potential of this event we added (p. 6 l. 13-15): "The variable structure of the ice layer with double layers indicates a highly dynamic behavior of the atmosphere with presence of strong gravity waves. Nevertheless, a detailed examination of the dynamical structure is beyond the scope of this paper."

p. 5, l. 23 Again on this paragraph, it is not clear what the intention is. You want to show four cases to make what point? That you also see features that others have described? It is not comprehensive, there is no explanation given, no conclusion is drawn, so why? You show layers with intricate morphology, but you do not do justice to this. In the following you restrict yourselves to three parameters only.

We are sorry for not describing the purpose of these examples. We added a new paragraph at the end of the subsection (p. 7 l. 1-8): "The examples shown above demonstrate the different relations of the NLC and MSE layer edges and the different degrees of accordance of the layers. This is in general agreement with observations at polar latitudes (e.g. Klekociuk et al., 2008; Kaifler et al., 2011). The examples indicate an often good concurrence of the lower edges but a worse agreement of the upper edges. If solar elevation (i.e. ionization) is sufficiently large, NLC are often but not always accompanied by MSE. The latter might be explained by missing turbulence, but this cannot be proven here because a lack of appropriate measurements. Periods with MSE but absent NLC can be caused by mainly small ice particles, resulting in lidar signals below the NLC detection threshold. In the following we neglect profiles of NLC without MSE as well as MSE without NLC to be sure that for this study all requirements for the observation of small and large ice particles are fulfilled (see below)."

p. 5, l. 27 MSE that are too high to be observed by lidar? Surely there is no limit at e.g. 85 km for the lidar? And MSE that are too weak to be observed by lidar? They are not observed by lidar in any case.

We deleted the ice cloud "too high" for lidar but left "too weak". The ice cloud may produce an optical signal below the threshold but is detected by the radar, e.g. in case of small particles. We add "detected as MSE" for clarity.

p. 5, l. 30 might be worth giving an update on the occurrence rate: 188.5 h / 3337 h is ~ 5 %. And is 3337 hours the "operation time" or the time with high-quality data suitable for NLC detection? Cause that might be significantly lower than the operation time. And it is only this that is relevant information for scientific purposes, the former is of interest to the laser engineer only.

We prefer not to mention the occurrence rate of NLC in general, as only NLC accompanied by MSE are used here. The 3337 h are the number of hours suitable for detection of NLC with ß>0.3. We added the term "usable" for clarity (p. 7 l. 20).

p. 5, l. 29 I am surprised by the low number of 67 hours. You are throwing away 64 % of your precious, rare data on NLC. Might be worth to state why: So many hours due to solar elevation below 5 deg, so many hours due to missing PMSE at night, so many hours due to radar downtime

We do not distinguish why the data are not used here, but have added a short list of reasons (p. 7 l. 18/19). Indeed, we would be happy if we could use more of the rare NLC data here, but at our site many NLC profiles either show a quite low backscatter

coefficient (20-30% with ß<0.3, estimated from Fig. 4 of Gerding et al., JGR, 2013) or appear during nighttime, when the ionization is typically too small to support MSE (cf. 8.5 h of solar elevation below 5° on 21 June).

p. 5, l. 32 it makes you wonder if the study is representative for NLC, if you only use 36 % of the data. . . Fig. 1, 2 the five events shown amount to 17 hours out of the 67 hours. So I extrapolate that your statistics is based on 20 events? You withhold that number, but you should give it

As mentioned above, we do not see a significant difference between the layer parameters of the NLC used here and of all NLC. We discuss this in more detail in Section 5. Therefore we consider our results representative for all NLC.

We added on p. 7 l. 16-18: "These data are distributed across 31~days with an average ice cloud duration of 2.2 h. For this study it is not relevant whether the ice observation is uninterrupted in time or not, because the layer parameters are derived based on individual (but smoothed) profiles."

p. 6, l. 4 as shown in Fig. 1, but what about the multiple layers in Fig. 2d? These are several hours at least. In a dataset this small, it would be worth taking very good care of this.

We added on p. 7 l. 29 "In the rare case of a double layer we take the lower edge of the lower layer and the upper edge of the upper layer together with the absolute maximum."

p. 6, l. 4 1931 profiles a 2 minutes are 64 hours. But you said the NLC data was smoothed with 15 min running mean, so only 256 profiles are independent, aren't they, and not 1931?

The reviewer is right, not all profiles are independent. We added on p. 7 l. 30/31: "…, even if the respective smoothing needs to be taken into account for interpretation."

p. 6, l. 7 82.6 km for the lower edge seems quite high, how does this compare to polar latitudes? This is 82.1 km, I checked, so you might want to discuss this

The ice layer altitude is on average increasing with latitude, which is related to the changing temperature profile (smaller likelihood for supersaturation at, e.g., 82 km at 54°N compared to 70°N). Following the suggestion, we have added a short section in the discussion (p. 12 l. 15-18): "In their Table 3 they report also quasi-identical lower edges of NLC and PMSE, even if the z^{low} at higher latitudes are observed 0.5 km above the midlatitude values. This latitudinal difference of z^{low} can be explained by the general increase of NLC altitudes with latitude (Lübken et al., GRL, 2008; Chu et al., GRL, 2011) which is related to the ambient temperature structure."

p. 6, l. 14 any physical explanation for the 4-5 km difference?

We already tried to provide an explanation in the following sentences.

p. 6, l. 15 "can also be explained" and what was the first explanation if this is the second? The "morning twilight" is no obvious physical explanation

We rephrased the sentence before to make clear that this is a first explanation (p. 8 l. 8/9: "… in cases of MSE onset in the morning twilight where sometimes the MSE only agrees with the uppermost part (i.e. largest ionization) of the ice layer").

Fig. 3b there are MSE altitudes 3 km below the NLC altitude, you didn't mention this

We now expanded the explanation of the few larger altitude differences by "Rarely, the different size dependency of lidar and radar signals can lead to MSE even a few km below the NLC." (p. 8 l. 12/13)

Fig. 5b I can't believe that this distribution has the same correlation coefficient as the one in Fig. 3b. Can you check this number again?

We double-checked this number without finding an error.

p. 8, l. 1 no ice particles are visible for radars

We changed the phrasing to "detected by".

p. 9, l. 4 so this is evidence for local formation of ice clouds then?

Potentially, but a final proof cannot be given from the available data.

p. 9, l. 9 "as expected" you should state the observations and then draw conclusions, and not expect something

We added "from previous observations" (p. 11, l. 1). We want to make clear that this is not the first observation of southward wind during NLC/MSE.

p. 9, l. 16 atmospheric conditions like haze and solar background are the same to the two lidars, so they can't be the reason for a smaller dataset in one? Either it's a technical limitation or operational?

The potassium lidar at 770 nm suffers more from hazy conditions than the RMR lidar at 532 nm due to enhanced scatter of the longwave fraction of solar radiation. We added "at near-infrared wavelengths" (p. 11 l. 8).

p. 9, l. 17 seven events are how many independent profiles?

The seven events cover more than 25 h of data, but this number includes also periods during night/twilight, when the MSE has not set in, yet. The temperature data set for these days is much larger because it is not limited to NLC. On the other hand, temperatures have been calculated every 15 min with 2 h integration. We hesitate to provide all these numbers in the paper. We do not observe that the ambient conditions change drastically within the individual events, i.e. an event-wise classification is justified.

p. 10, l. 12 as you showed, multi-year is not enough to be either statistical or representative

We replaced "statistical analysis" by "analyses of average layer parameters".

p. 10, l. 15 The mean peak altitude of this study is 83.3 and not 82.6 km. This was the mean lower edge. So this does not compare at all to the centroid altitude statistics and must be explained. Either you made a mistake, or this study is not representative at all.

We are sorry for this mistake and thank the reviewer for his careful reading. As described above the selected cases are still representative for NLC in general. We have corrected the numbers and explain these now in more detail.

p. 10, l. 24 and the lower edge in Kaifler et al. (2011) is 82.1 km, which is 500 m below your results

As mentioned above, we have added and explained this difference in the Discussion (p. 12 l. 16-18).

p. 10, l. 30 you didn't evaluate the thickness of the PMSE layer, so you need to cite for this statement

We have added a reference (Kaifler et al., 2011).

p. 11, l. 5 is this a result of Kiliani et al. (2013)? 150 km is not a large distance at all, I'd be surprised

This is a result of Kiliani et al. and relates to a mean wind speed of 7 m/s (or 23 m/s for their upper limit). However, we see the old phrasing potentially misleading and changed it to (p. 12 l. 32/33): "In this period, the ice particles typically travel 150-500 km southward. Before, the ice particles remained small (< 20 nm) for more than 60 h."

p. 12, l. 12 if -14 dB gives similar results than -12 dB, then -12 dB is not the noise limit, or am I wrong?

We rephrased "The threshold is set to -12 dB based on the noise limit of the radar." to "The threshold is set to -12 dB to be above the typical noise limit of the radar." (p. 14 l. 5)

p. 13, l. 5 here, in the conclusions, this is the first time that structures in the plasma are mentioned

As mentioned in the above comments we now explain the complex origin of MSE much earlier. We thank the reviewer for making us aware of this.

Technical corrections:

p. 1, l. 8 Please don't italicize indices (low, NLC, MSE, I mean: typeset with z_NLC in LaTeX)

Done

p. 1, l. 10 expression: "typically do not expand much above". (expression: ".." in the following always means that I feel the language could be improved here)

Changed to "typically do not stretch much higher than the NLC" (p. 1 l. 13).

p. 2, l. 2 expression: "indicator for temperatures being below the frost point"

Deleted "being"

p. 2, l. 4 "we utilize"

Changed

p. 2, l. 6 expression: "particular important"

Changed to "in particular"

p. 2, l. 6 "partly used" that might be an unfortunate expression. You might mean all kind of things.

Deleted "partly"

p. 2, l. 10 the observations do not gain additional information

Changed to "give additional information" (cf. Reviewer #1)

p. 2, l. 16 expression: "observations to examine this question"

Changed to "solve this question"

p. 2, l. 16 delete “obviously”

Done

p. 2, l. 24 expression: “extend several kilometers higher”

Changed to “stretch several kilometers higher” (p. 2 l. 32)

p. 3, l. 11 expression: “observations are performed”

Changed to “made”

p. 3, l. 15 you already noted in line 11 that it is daylight-capable

We kept this but added “… and replaced the former RMR lidar used for nighttime NLC statistics.” (p. 3 l. 27)

p. 3, l. 19 “of _60 murad”, you already mentioned that it is narrow

Deleted “narrow”

p. 3, l. 22 Noctilucent Clouds -> NLC

Done

p. 3, l. 22 remove “in the NLC altitude “

Done

p. 3, l. 30 “evaluated manually”

Word order changed

p. 3, l. 31 “identified by software” you mean by some algorithm, which could be described here, or not

Changed to “by an algorithm”

p. 4, l. 2 “For reception”

Done

p. 4, l. 3 please spell 6 as six, 7 as seven, throughout the manuscript

Done

p. 4, l. 4 expression: “Time series resulted in length of 34.1 s”

Changed to “Time series of 1024 data points are acquired within 34.1 s.” (p. 5 l. 8)

p. 4, l. 5 expression: “the available time resolution for observations amounted to 2 min”

Changed to “the time resolution for MSE observations is 2 min.”

p. 4, l. 12 expression: “Due to the not available absolute calibration”

Changed to “As we do not have an absolute calibration of the radar, we use SNR as an approximation for the echo intensity.” according to the suggestion of Reviewer #1 (p. 5 l. 17).

p. 5, l. 1 expression: “different types of agreement” that could be phrased somehow better

Rephrased to “Similar to previous studies we find partly very large agreement between NLC and MSE, while there are differences in other cases” (p. 5 l. 21/22)

p. 5, l. 2 if it is the first or last event or one in between doesn't matter, I think

Changed to "shows an events that was observed on 17 June 2010"

p. 5, l. 6 you might want to start a new paragraph for the discussion of each case

Done

p. 5, l. 18 growed to -> grew into? Or maybe: developed into

Changed to "grew into".

p. 5, l. 20 expression: "slightly after each other"

Deleted

p. 5, l. 23 This paragraph starting at p. 5, l. 1 should be put into a separate subsection with paragraphs

Done

p. 7, l. 1 expression "more pointlike"

Rephrased to "only ~1/1700 of this" (p. 8 l. 11)

p. 7, l. 4 delete blank between 4 and .

will be done late when the \marginpar command is removed

p. 7, l. 6 (Fig. 4, right)

Done

p. 8, l. 1 "regions extends" one s is too much

Corrected

p. 8, l. 2 "getting finally visible for lidars"

Rephrased to "and become …"

p. 9, l. 2 "new ice layer" well, "new" in what sense, maybe "another"?

Changed

p. 10, l. 10 observation probability == occurrence frequency?

Changed

p. 10, l. 13 "the first RMR lidar" doesn't really matter here if it was the first?

Changed to "previous" to make clear that it was not the lidar used for the current study

p. 10, l. 31 descend -> descent, also p. 13, l. 21

Corrected

p. 11, l. 1 expression: "hint to the conclusion"

Changed to "suggest" (cf. Reviewer #1)

p. 11, l. 1 expression: "the layer of only small particles"

Deleted "small"

p. 11, l. 16 to allow "for"

Corrected

p. 12, l. 8 “which” is slightly smaller

Corrected

p. 13, l. 18 extent

Corrected