

Interactive comment 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

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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.

However, the study presented here is not as extensive as the studies at polar latitudes.

C1

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.

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.”

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. 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.

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

C2

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.

Another criticism 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.

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.

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

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

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.

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

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.

C3

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 misled and then be surprised while reading that it's in fact more complicated then you had hinted.

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

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

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

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

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?

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

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!

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

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

C4

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!

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

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.

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.

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.

p. 5, l. 30 might be worth giving an update on the occurrence rate: $188.5 \text{ h} / 3337 \text{ h}$ is $\sim 5 \%$. 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.

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

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

C5

number, but you should give it

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.

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?

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

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

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

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

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?

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

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

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

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?

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

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

C6

tative

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.

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

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

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

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?

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

Technical corrections:

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

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)

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

p. 2, l. 4 "we utilize"

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

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

C7

kind of things.

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

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

p. 2, l. 16 delete "obviously"

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

p. 3, l. 11 expression: "observations are performed"

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

p. 3, l. 19 "of ~60 murad", you already mentioned that it is narrow

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

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

p. 3, l. 30 "evaluated manually"

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

p. 4, l. 2 "For reception"

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

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

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

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

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

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

C8

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

p. 12, l. 8 "which" is slightly smaller

p. 13, l. 18 extent

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