Report #1

Submitted on 26 Jun 2024
Anonymous referee #3

Anonymous during peer-review: Yes No
Anonymous in acknowledgements of published article: Yes No

Checklist for reviewers

1) Scientific significance
Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?
Excellent **Good** Fair Poor

2) Scientific quality
Are the scientific approaches and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Note that papers do not necessarily need to be long to be scientifically sound.
Excellent **Good** Fair Poor

3) Presentation quality
Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?
Excellent **Good** Fair Poor

For final publication, the manuscript should be

accepted as is
accepted subject to **technical corrections**
accepted subject to minor revisions
reconsidered after **major revisions**
rejected

Were a revised manuscript to be sent for another round of reviews:
I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The revised manuscript "Gravity waves above the Northern Atlantic and Europe during streamer events using Aeolus" by Wuest et al. reads much better now. My comments have been adequately addressed.

Still, there are a few corrections that should be made before publication in AMT. My main point is that there are still a few occurrences in the text that suggest Ekin would be a lower limit, which however is not clear.

Please find my specific comments below.

Thank you for your comments. I addressed them below and changed the manuscript accordingly.
Two additional remarks:

- According to the request of AMT, I revised the color scheme of figure 7 and 8, I did not change the content.
- In lines 471 – 472 (version with marked changes) there is written "Aeolus measures a horizontal line-of-sight velocity; this means that it is particularly sensitive to GWs whose air parcels oscillate horizontally (inertia GW) and parallel to the line of sight." I changed the term in brackets from inertia to inertia and mid-frequency GW as also mid-frequency GW show a horizontal direction of oscillation.

SPECIFIC COMMENTS:

(1) l.16/17: Since the abstract is a very prominent place in the manuscript, you should avoid the uncommented use of "lower limit" because later in the manuscript it turns out that this expectation does not necessarily hold. Suggestion:
... this is strictly speaking a lower limit for the kinetic energy density.
->
... assuming a perfect instrument performance, this would be a lower limit for the kinetic energy density.
Thanks for the suggestions, I included it.

(2) l.100: the region -> a prominent region
Changed

(3) l.142: Just a comment.
My previous reviewer comment about the streamer structure was just a misunderstanding. What I was trying to say is that meridional transport of air causes zonal variations in the TO3 distribution.
I think your text in the revised manuscript is OK as is.
Thanks for the clarification.

(4) l.213: height -> magnitude
Indeed!

(5) l.343: later -> latter
Done

(6) l.393: 250 -> 250 hPa
Done

(7) l.393: please resolve "GFS"
Done

(8) l.472: I do not understand the following statement:
"The larger the zonal wavelength and the smaller the meridional one, the closer the measured amplitude comes to the true amplitude."

Just before you were talking about Aeolus limitations regarding short meridional wavelengths. This would suggest that Aeolus would be more sensitive to long meridional wavelengths.
With this sentence I wanted to say that not only the averaging along the flight direction plays a role for detecting a wave and measuring its true amplitude but also the orientation of the wave: if the wave is oriented perpendicular to the line of sight, the averaging effect is much larger compared to a parallel orientation. As Aeolus looks to the side (35° off nadir), waves with longer zonal than meridional wavelengths are less attenuated. However, since this sentence is confusing and the main message is already given two sentences before the one we are discussing, I deleted it.

(9) l.475: bias -> high-bias
Done

(10) l.508: please remove "lower bound" from the caption of Fig. 10 because Ekin may be severely high-biased due to measurement noise.
I deleted the whole figure due to the comments of reviewer 2.

(11) Caption of Fig.11, second line:
left part -> right part
Corrected

(12) l.520: please avoid the expression "lower bound" because Ekin may be high-biased
I replaced the sentence in brackets with "estimates"

Report #2

Submitted on 27 Jun 2024
Anonymous referee #1

Anonymous during peer-review: Yes No
Anonymous in acknowledgements of published article: Yes No

Checklist for reviewers

1) Scientific significance
Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?
Excellent Good **Fair** Poor

2) Scientific quality
Are the scientific approaches and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Note that papers do not necessarily need to be long to be scientifically sound.
Excellent Good **Fair** Poor

3) Presentation quality
Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?
Excellent Good **Fair** Poor

For final publication, the manuscript should be

accepted as is

accepted subject to **technical corrections**

accepted subject to **minor revisions**

reconsidered after major revisions

rejected

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I would be willing to review the revised manuscript.

I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published) SHORT SUMMARY
The paper titled "Gravity waves above the Northern Atlantic and Europe during streamer events using Aeolus" has been revised somewhat to address points raised by this and other reviewers' comments. It has been improved, however there are still a number of issues that this reviewer has with the manuscript in its current form. These would need addressing before publication by AMT can be recommended by this reviewer.

Thank you for your comments. I addressed them below and changed the manuscript accordingly.
Two additional remarks:

- According to the request of AMT, I revised the color scheme of figure 7 and 8, I did not change the content.
- In lines 471 – 472 (version with marked changes) there is written "Aeolus measures a horizontal line-of-sight velocity; this means that it is particularly sensitive to GWs whose air parcels oscillate horizontally (inertia GW) and parallel to the line of sight." I changed the term in brackets from inertia to inertia and mid-frequency GW as also mid-frequency GW show a horizontal direction of oscillation.

GENERAL COMMENTS
1) This reviewer still has concerns about the method used to extract GW perturbations and there is no substantial development of the analysis technique or a wider analysis which might help to diminish these concerns. The authors have instead sought to clarify in the discussion the reasons for their previous method choice, which they do by considering the tropopause as the main non-GW perturbation source, and by using the 'repeating spline' method to avoid 'strong deflections'. Although the reviewer would prefer to see a consideration for other important wind perturbation sources in the troposphere, the authors do now clearly discuss this limitation of their method. Indeed, the authors have added some caveats into the text to clarify the shortfalls of the GW extraction method as a consequence of the techniques they are using.
The analysis technique of adapting a spline to the data is common for the analysis of gravity waves (see, for example, the citations in the introduction of Wüst et al. (2017)). In order to avoid exactly this discussion about the repeating spline approach, I published a paper (Wüst et al, 2017) where I introduced it, compared it to the "normal" spline method and applied it to measured data. Even though there are other techniques than the adaption of a spline to extract gravity wave signatures from vertical measurement profiles (local or global horizontal filters, if enough data are available in the horizontal, for example, or vertical filters such as the Butterworth filter, which is a high-pass fast Fourier transform filter), there is no agreement in the community, which technique is the most appropriate one, or a publication known to me, where the optimal technique is published; every technique has its advantages and disadvantages. For the spline technique, the greatest challenge in this work is the tropopause; we discussed this challenge and split the analysis into tropospheric and stratospheric part.
Comparing the different approaches for the extraction of gravity waves from vertical measurement profiles is not the scope of this manuscript. Therefore, we do not use further analysis techniques here.

2) The focus of the study has not been widened much in response to the previous comments by all reviewers. It seems a little unusual to focus on just streamer events (and one specific type at that – see author comment on P05.L107 on page 5 of their response to RC2) and only using Aeolus, for an AMT paper, which is why the suggestion to widen this focus was made previously. Would this article be better suited to a diWerent journal if it is to be so focused, as the authors have acknowledged that it is?
This manuscript represents a significant part of the results achieved in an ESA project. The project belonged to a call which focused on innovative applications of Aeolus data, so on other ones than the originally planned. The AMT homepage says that this journal is dedicated to the "discussion of advances in remote sensing, as well as in situ and laboratory measurement techniques for the constituents and properties of the Earth's atmosphere." We tried to use the Aeolus data for the investigation of a possible gravity wave source. This source was not explicitly addressed before, nevertheless it can be expected that gravity waves are generated by these streamer events. Additionally, the purpose of Aeolus is not the investigation of gravity waves. So, from our point of view and probably also from ESA's point of view, this study discusses an innovative aspect of an advanced remote sensing instrument. That is why we think that this manuscript fits into the scope of AMT.
Of course, it would be of interest to widen the study, however, we have here practical limits (time, budget, obligations in new projects).

3) Further changes are necessary to some of the figures before they are of publishable quality. Notably, figure 10 is very diWicult to understand with the filled contour method currently used to plot the data. Although the authors have clarified the dates corresponding to each week and have defended their colour scale choice, they have not changed the plotting technique to make it easier to read. Furthermore, it is very diWicult to follow the story being told by this figure, in tandem with the corresponding text.
I removed figure 10. Finally, this figure does not provide much info – the most important info is: the amount of data is not enough to derive a temporal resolution of less than one week and this is to low for observing a phenomenon which lasts only some days. The manuscript is adapted accordingly.

SPECIFIC COMMENTS
PXX.LXX Comment

P03.L63 However, for parts of the GW spectrum, equation (3)... (Please insert a comma after 'spectrum' to avoid ambiguity.)
Done

P03.L87 The phrase "especially the precision, since it is not removed through the detrending procedure in contrast to a bias" is now a little confusing. Why is the precision being removed? Yes, you are right, I re-formulated it. In the same sentence a saw a mistake, which I also corrected: "the error …, which is lower than originally expected" → "the error …, which is higher than originally expected" (the precision is lower, but not the error).

For Aeolus, which is more challenging, the precision or the accuracy?
Sorry, I don't understand the question. Challenging in which context? In the context of deriving the error or handling it, for the analysis of the Aeolus data, … ? For Aeolus data, the error is not separated according to bias and precision. Validation campaigns mentioned in the manuscript provided hints, that the random error, so the precision, is higher than the bias. More info about the source of the random error are now provided in the text according to the comments of another reviewer (l. 240 ff. in version with marked changes).

P08.L178 This now reads a bit better, the authors have clarified that the variable being added is only being corrected for the satellite's orbital node and it is clearer.
Thank you.

FINAL COMMENT
Overall, no significant changes have been made to the scientific method, scientific quality or presentation quality for this manuscript, and so it is not yet possible to recommend that the
article be accepted for publication without some of the changes outlined above and previously.
There have been some good alterations to the text which add important clarifications, however further revisions to the method and quality are still required for this reviewer to accept the paper.

Referee Report:
amt-2024-18-referee-report.pdf

Report #3

Submitted on 04 Sep 2024
Anonymous referee #4

Anonymous during peer-review: Yes No
Anonymous in acknowledgements of published article: Yes No

Checklist for reviewers

1) Scientific significance
Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?
Excellent Good **Fair** Poor

2) Scientific quality
Are the scientific approaches and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Note that papers do not necessarily need to be long to be scientifically sound.
Excellent Good **Fair** Poor

3) Presentation quality
Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?
Excellent Good **Fair** Poor

For final publication, the manuscript should be

accepted as is
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rejected

Were a revised manuscript to be sent for another round of reviews:
I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The revised manuscript "Gravity waves above the Northern Atlantic and Europe during streamer events using Aeolus" by Wüst et al. discusses the use of Aeolus data to derive the kinetic energy density over the Northern Atlantic and Europe during streamer events. These events are typically associated with increased gravity wave energy densities. A key challenge is the high noise level in Aeolus data, complicating the detection of gravity wave signals. To mitigate this, the authors average the data over a larger region, reducing uncertainty in the daily time series and enabling a more accurate correlation with streamer events. The study finds minor enhancements in kinetic energy density that may be linked to streamers, with horizontal distributions indicating other potential gravity wave sources as well.

As noted by the referees, the paper aligns well with the scope of AMT and offers valuable insights for the atmospheric dynamics community, especially for those focused on deriving gravity wave distributions from Aeolus measurements. In the revised manuscript, the authors have addressed most of the points raised by the referees and made the necessary changes to the text. However, several issues still need to be resolved before publication.

Thank you for your comments. I addressed them below and changed the manuscript accordingly.
Two additional remarks:

- According to the request of AMT, I revised the color scheme of figure 7 and 8, I did not change the content.
- In lines 471 – 472 (version with marked changes) there is written "Aeolus measures a horizontal line-of-sight velocity; this means that it is particularly sensitive to GWs whose air parcels oscillate horizontally (inertia GW) and parallel to the line of sight." I changed the term in brackets from inertia to inertia and mid-frequency GW as also mid-frequency GW show a horizontal direction of oscillation.

General comment:

Although the averaging approach to resolve the streamer events despite the high noise level in the Aeolus data is interesting, I wonder if the authors would have detected the two events without prior knowledge of the total ozone column measurements. When examining the timelines in Figs. 8 and 9, the enhancements in Mean E_kin are not as pronounced as described in the text and are only noticeable when one knows what to look for. Therefore, the authors should clarify the primary objective of this study already in the introduction—namely, analyzing streamer events in terms of kinetic energy density using Aeolus data after they have been detected by other means. This clarification will help avoid potential confusion or disappointment for the reader who might expect the identification of gravity waves and/or streamers based on Aeolus data, given the title of the paper.

Yes, that's true and I added this info in the abstract, intro and summary.

Additionally, the unique benefits of using Aeolus data compared to other satellite data for streamer analysis should be more thoroughly elaborated in the text.

I added some info in the paragraph starting in line 99 (version with marked changes).

Specific Comments:

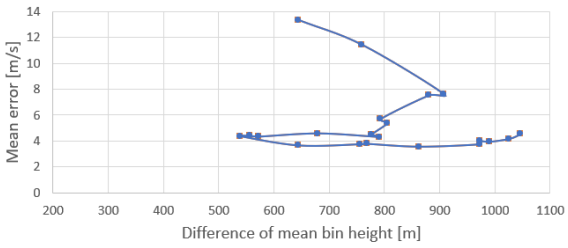
1. The term "Atmospheric Dynamics Mission" (L. 15) and its acronym "ADM" (L. 17) are still used in the abstract and should be removed.
Done, I also deleted it from table 1 (ADM-Aeolus Level-2B Algorithm Theoretical Baseline Document).

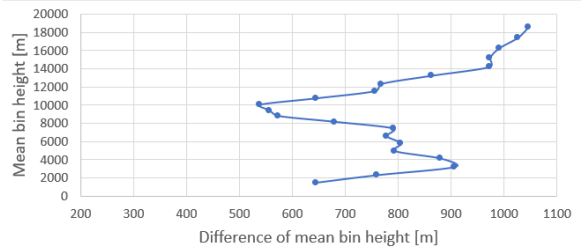
2. L. 204: Could the authors please clarify why Mie-cloudy winds, which have a smaller random error, were not used? I suspect this may be due to poor data coverage across the vertical profile. However, it might be possible to replace some of the noisy Rayleigh-clear winds with Mie-cloudy winds if they are available in the same region and altitude.
The L2B winds are available as Rayleigh-clear, Rayleigh-cloudy, Mie-clear and Mie-cloudy. The best quality has Rayleigh-clear and Mie-cloudy winds (Rennie et al. 2021). Mie-cloudy winds have smaller random errors (with a standard deviation of about 2.8–3.6 ms^{-1}) than Rayleigh-clear winds (4.0–7.0 ms^{-1}) winds (Rennie et al. 2021). However, there exist more Rayleigh-clear wind data than Mie-cloudy data (see figure 1 of Rennie et al. 2021). Additionally, Mie-cloudy data is hardly available in the upper part of the vertical profile, as clouds are very rare above the Northern Atlantic upper troposphere where the atmosphere is stably stratified and therefore suitable for gravity wave analyses. Therefore, we decided to use Rayleigh-clear data for the analysis when starting the project. I inserted this info also in the manuscript (l. 217 ff., version with changes marked).
We now checked whether it would be an option to merge both data sets. Both data products have different horizontal resolutions. Since March 2019, Rayleigh-clear data is averaged over 87 km, whereas Mie-cloudy are averaged over 12 km (Rennie et al. 2021). That is not ideal since it would make the analysis slightly inconsistent but would may be worth the effort, if enough profiles are available. We used the VIRES tool to for the time period of the first streamer (Nov. 1st – 8th, 2020). You find the screenshots at the end of the document. As you can see, Mie-cloudy winds are hardly available above the tropopause. So, we conclude that the Mie-cloudy winds are not helpful in this context.

3. L. 211 ff.: Relevant information on the altitude-dependence of the Aeolus wind error can be found in the following publications by ECMWF:

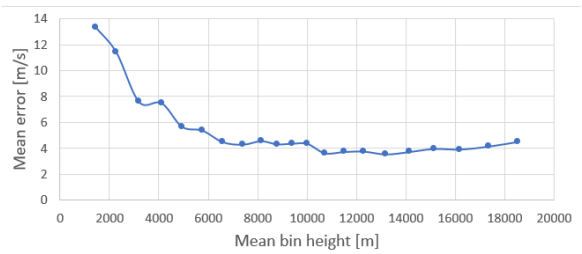
- Rennie, M.P., Isaksen, L., Weiler, F., de Kloe, J., Kanitz, T. & Reitebuch, O. (2021): The impact of Aeolus wind retrievals on ECMWF global weather forecasts. Q. J. R. Meteorol. Soc., 147, 3555–3586. https://doi.org/10.1002/qj.4142
- Rennie, M.P. & Isaksen, L. (2024): The NWP impact of Aeolus Level-2B winds at ECMWF, ESA Contract Report, 02/2024. https://www.ecmwf.int/en/elibrary/81546-nwp- impact-aeolus-level-2b-winds-ecmwf

I suggest adding a short paragraph on the wind error performance, focusing specifically on the Rayleigh-clear winds during the period of the two case studies.
I combined this with the results of the next comment and inserted a paragraph starting in line 240 (version with marked changes).





4. Line 230: The restriction to profiles that cover at least 21 height bins seems quite strict and might be relaxed to enlarge the dataset without compromising accuracy, especially since a spline fit is applied to the profiles. A few missing data points within the profile may be acceptable under this approach.
Yes, that's true and I checked it. For my analysis, I used the same data as for figure 4 (so Nov 2020, 25 – 70 °N, 0 – 20°E).
Since we are looking for gravity waves, it is important that the stratosphere, which is stably stratified, is covered by the data. As stratospheric interval 11 km and higher is used in the manuscript. This corresponds approximately to level 1 – 9 which cover 18.6 – 10.7 km. Nine data points are not much and since we analyzed vertical wavelengths from 5 to 10 km, the profile should at least cover those nine height bins.
I then checked how many profiles of the data basis mentioned above cover the bins 1 – 9 and the bins 1 – 21. In the first case, I get 2412 profiles, in the second 2216. So, analyzing additional profiles that cover the absolute minimum of data points would give me 9% more profiles or in other words, profiles that cover the bin range 1 – 9 cover also the bins 1 – 21 in the absolute majority of cases. Therefore, adding this comparatively very low number of profiles will very likely not change the results.
In this context, I found an error in the text: l. 261 (version with marked changes) – there was written level 1 is the lowest altitude, it is the highest. I changed it.



5. Fig. 3: Adding the measurement tracks of Aeolus to the map would be beneficial, as it would provide a clearer impression of the coverage of Aeolus wind data in the region of interest.
Done

6. I assume that the three maxima for the Rayleigh-clear HLOS wind error shown in Fig. 4 are related to differences in the thickness of the vertical range bins, which in turn affect the signal-to-noise ratio and consequently the wind error. Could the authors please verify this assumption?

That's an interesting idea, thank you. I checked it, however, it does not seem to hold, at least for this part of the data set.
For my analysis, I used the same data as for figure 4 (so Nov 2020, 25 – 70 °N, 0 – 20°E). I calculated the mean height of a bin, the difference between the mean bin heights and the mean error per bin for the bins 1 – 21. Next to this text, you find the results.
The mean error depends most clearly on the height (lowest plot): it varies around 4 m/s for ca. 6 km height and higher, below it can reach up to 13 m/s. The thickness of a bin (so the difference between the mean bin heights) shows a minimum around the tropopause and maxima in the upper part of the profile and around 4 km height (middle plot). The mean error can be relatively low for all bin thicknesses (upper plot, error values around 4 m/s for mean bin heights between ca. 550 m and 1150 m).

7. L. 428f.: Please rephrase this sentence: "They could be the results of the relatively high Aeolus error, specifically the if it is due to a low precision."
Changed into "They could be the results of the relatively high Aeolus error, specifically if the error is due to a low precision."

8. L. 496f.: Please rephrase this sentence: "Figure 11a) and b) depict the height of the first and the first as well as the second maximum per Aeolus wind profile."
Done

Technical Corrections:

1. Throughout the manuscript, units should be written in exponential form (e.g., m s^{-1}, J kg^{-1}) to comply with the AMT style guidelines.
Changed in the text and in figures 4, 7, 8, 9, and 10

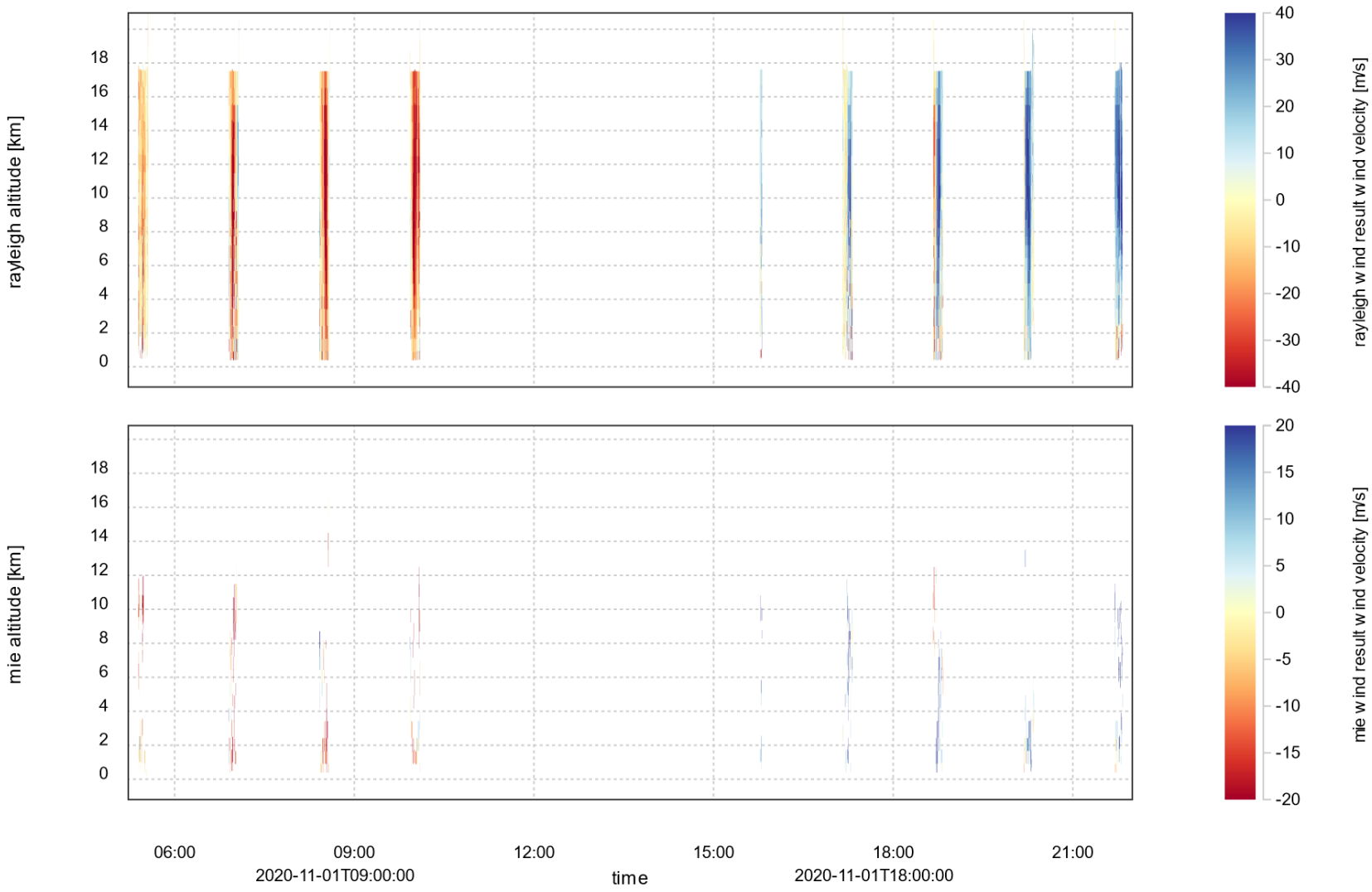
2. L. 343: "latter solution" instead of "later solution"
Done

3. L. 393: "250 hPa" instead of "250"
Done

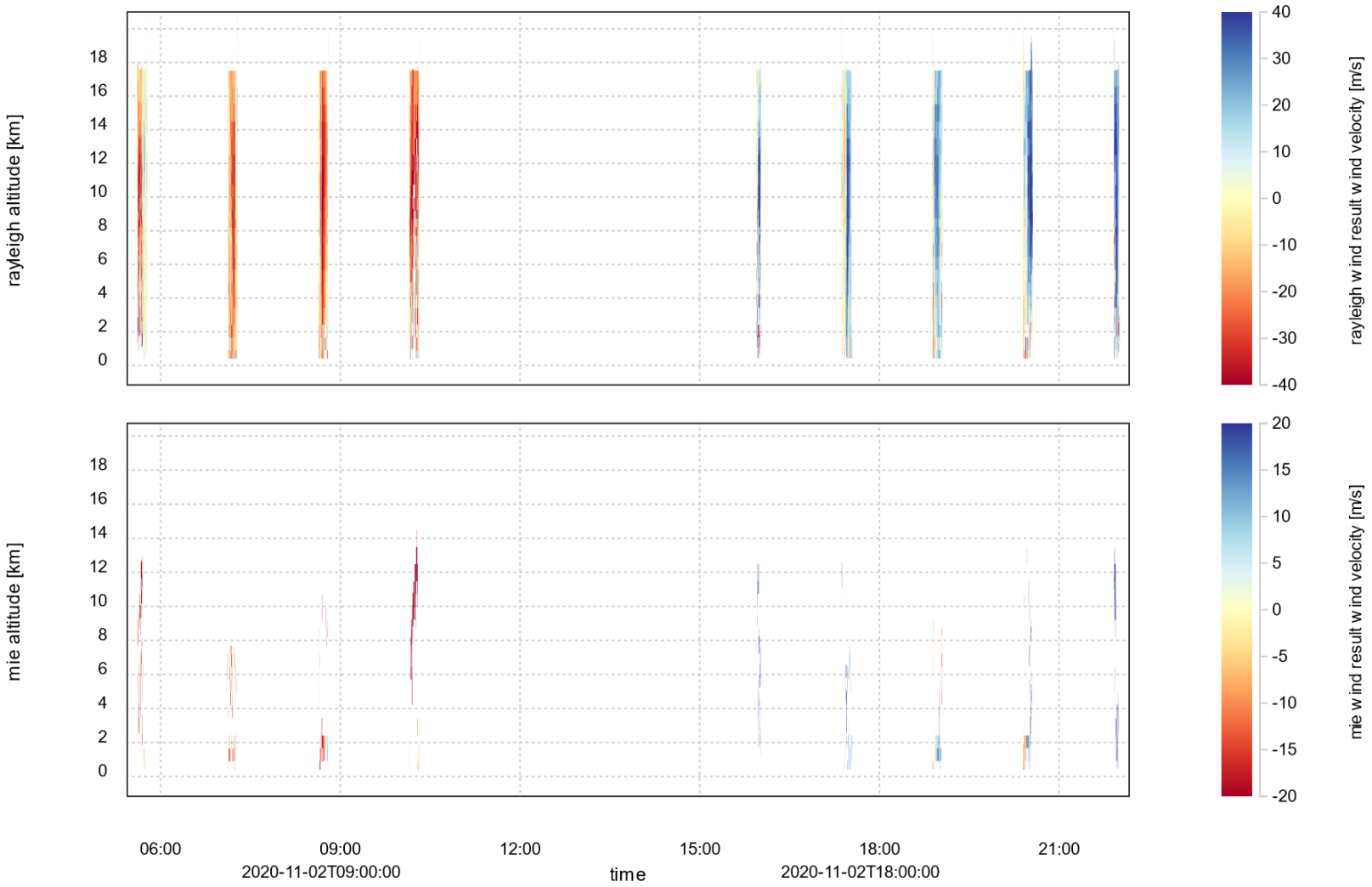
4. Caption of Fig. 11: "… the right part includes also the secondary maximum".
Done

Addressing these points will strengthen the manuscript and ensure it meets the high standards expected for publication in this field.

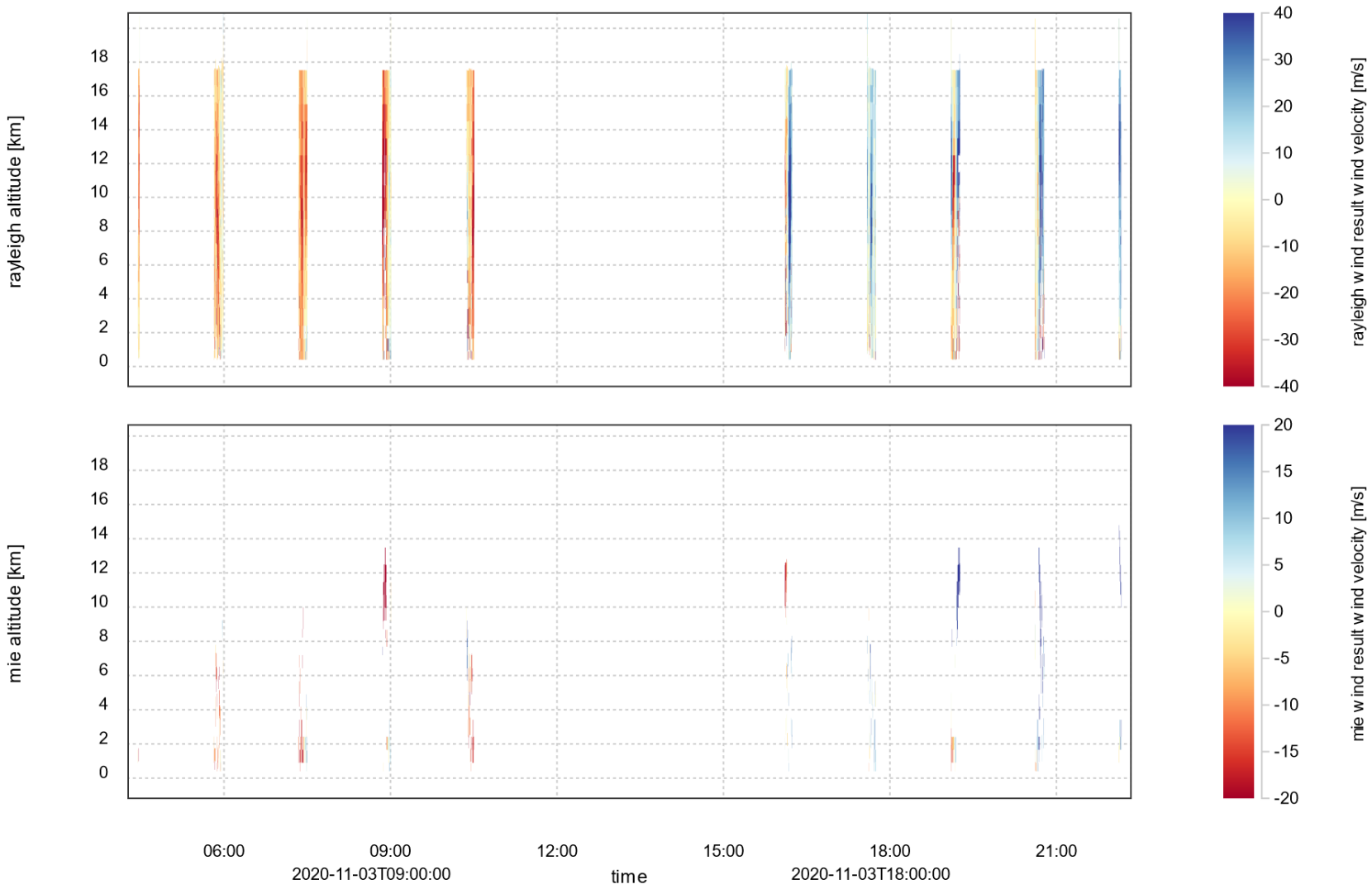
1st November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



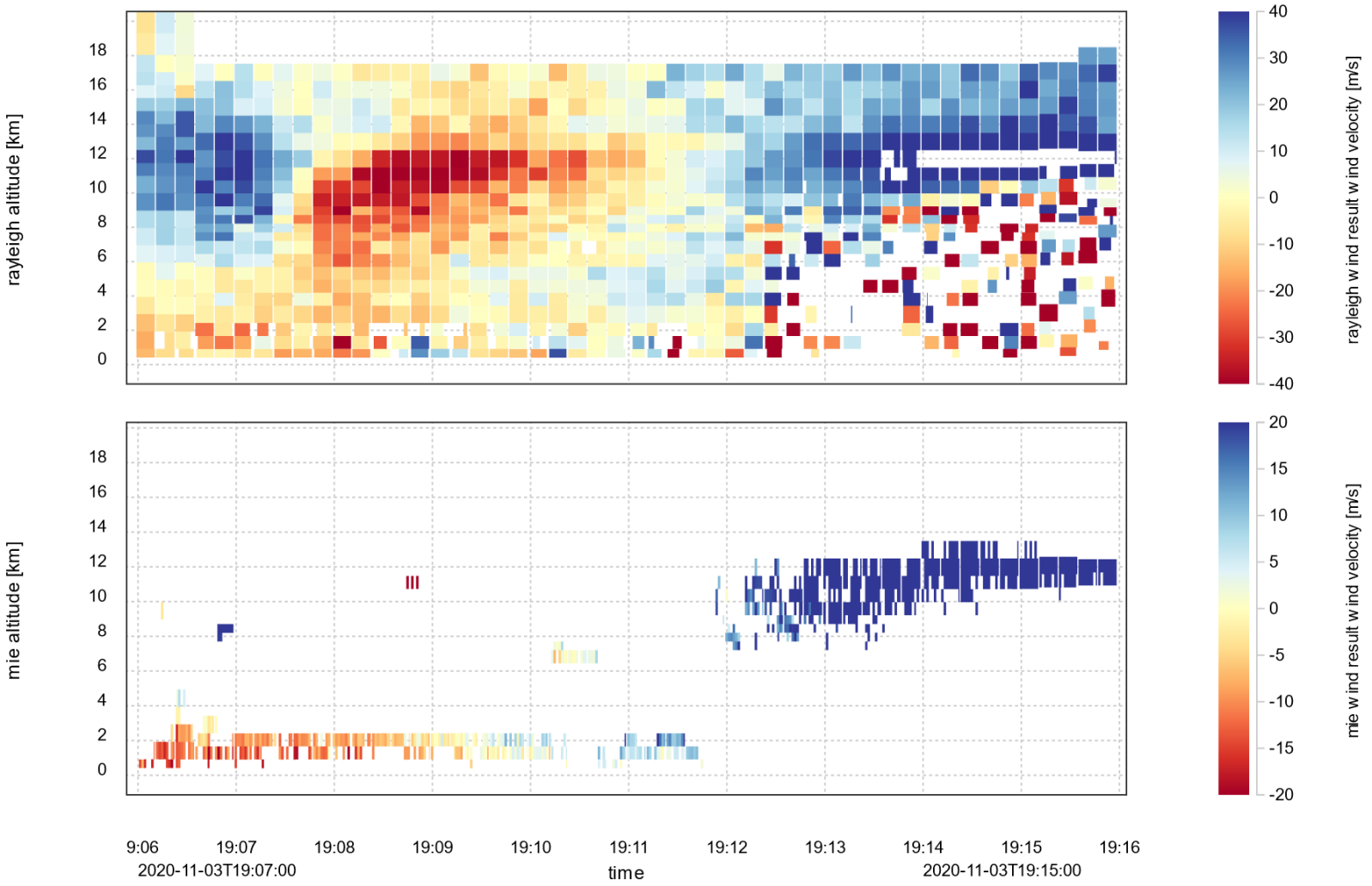
2nd November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



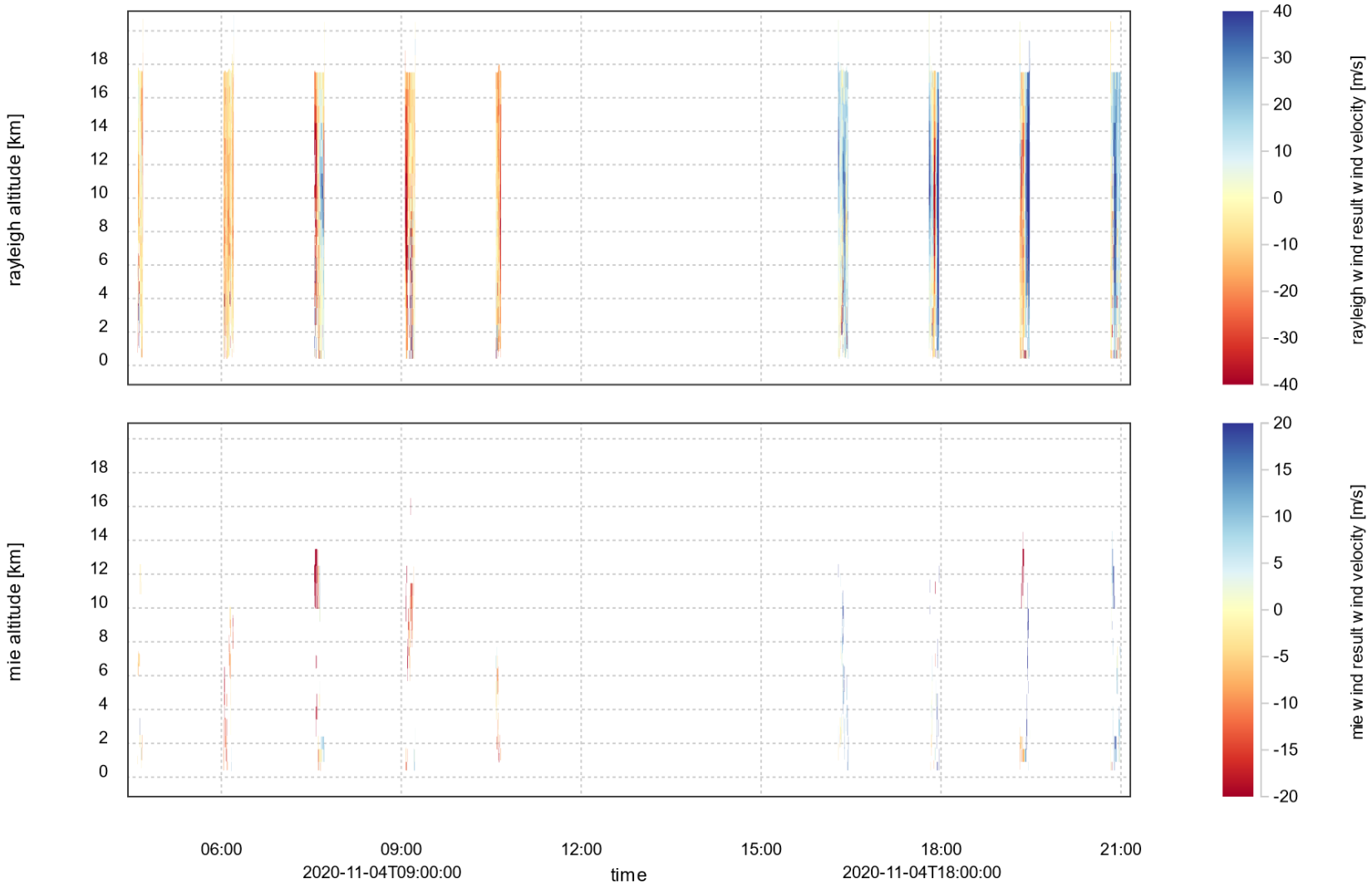
3rd November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



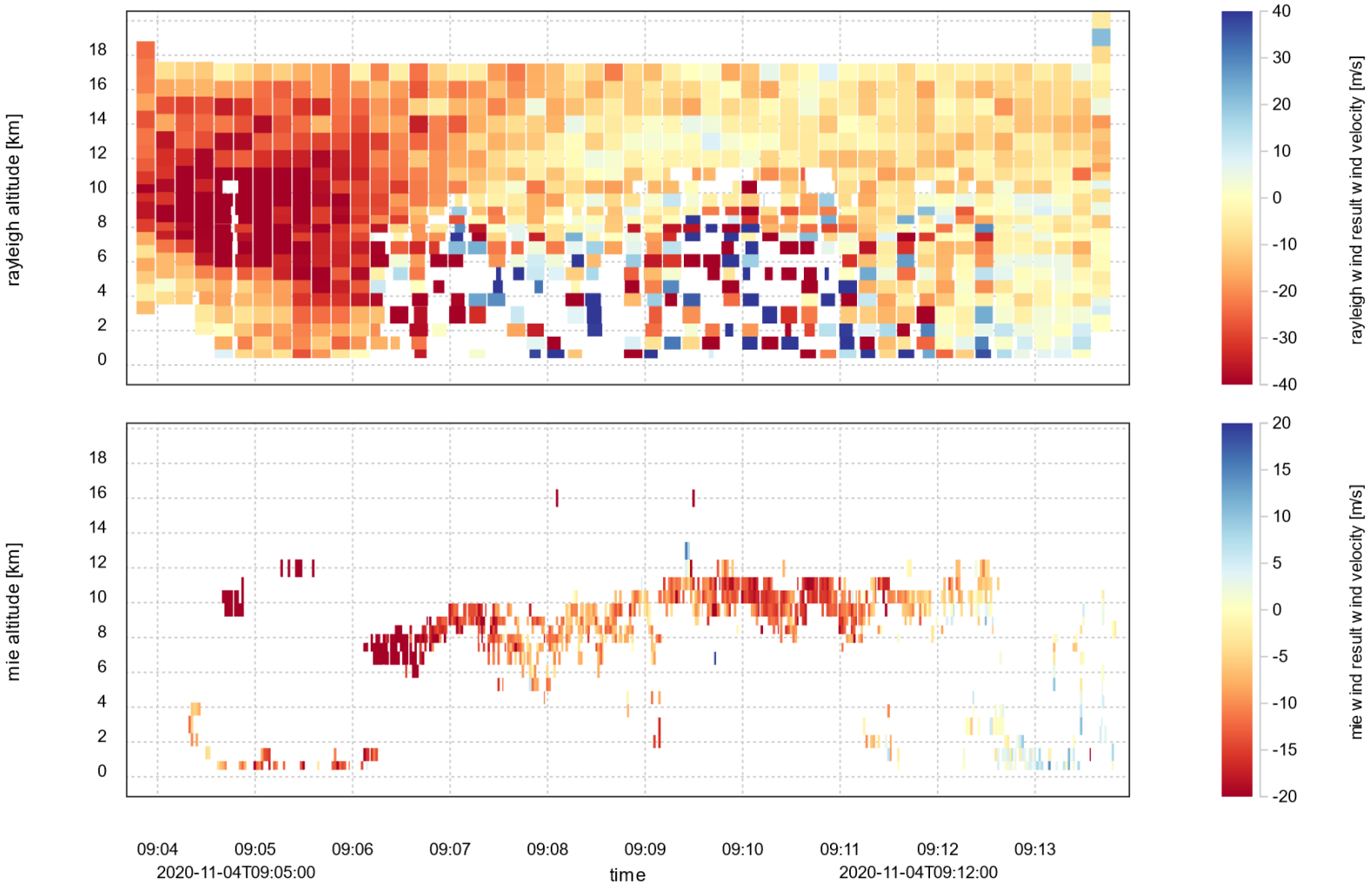
Zoom into the data of Nov, 3rd



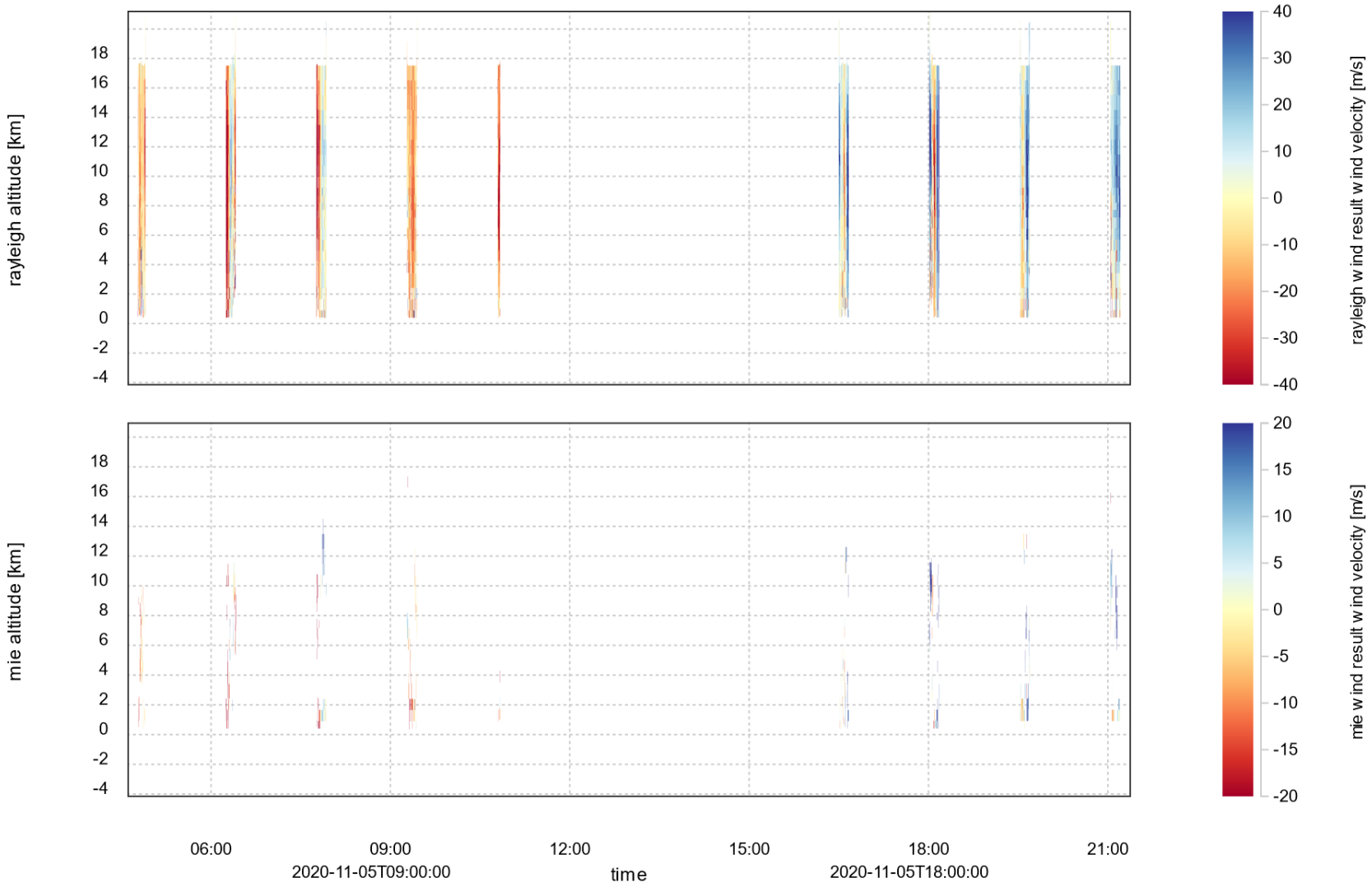
4th November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



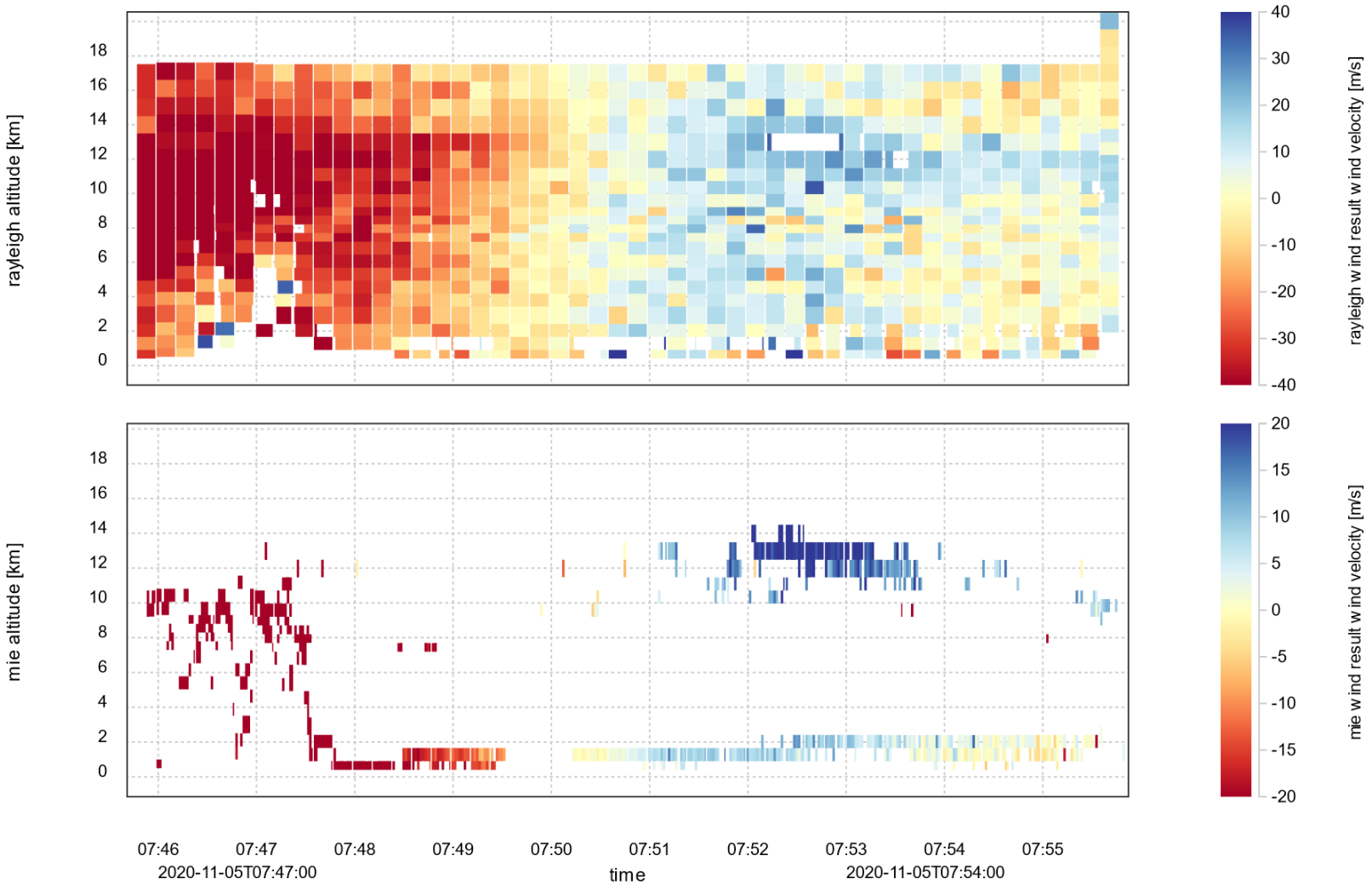
Zoom into the data of Nov, 4th



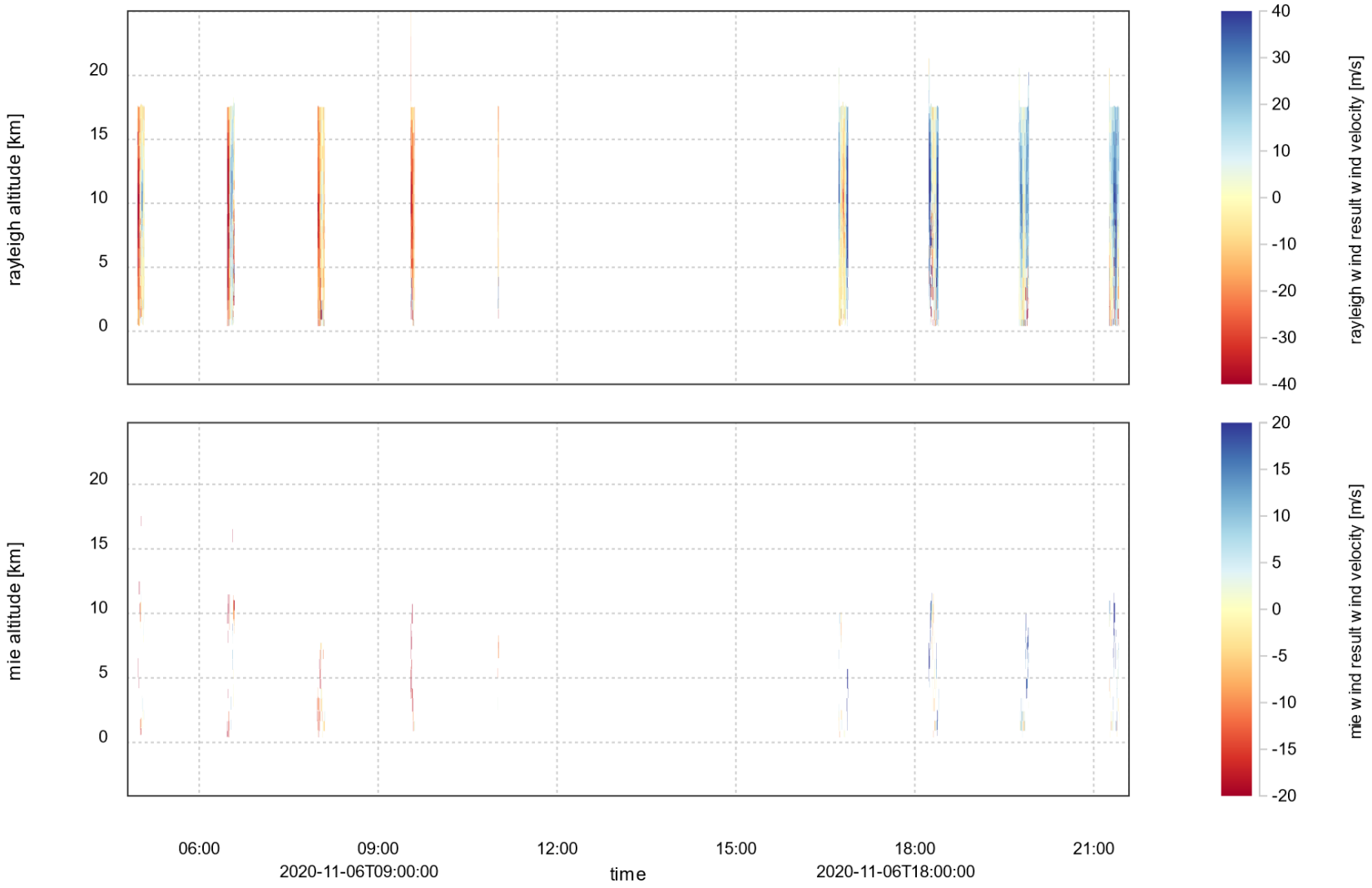
5th November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



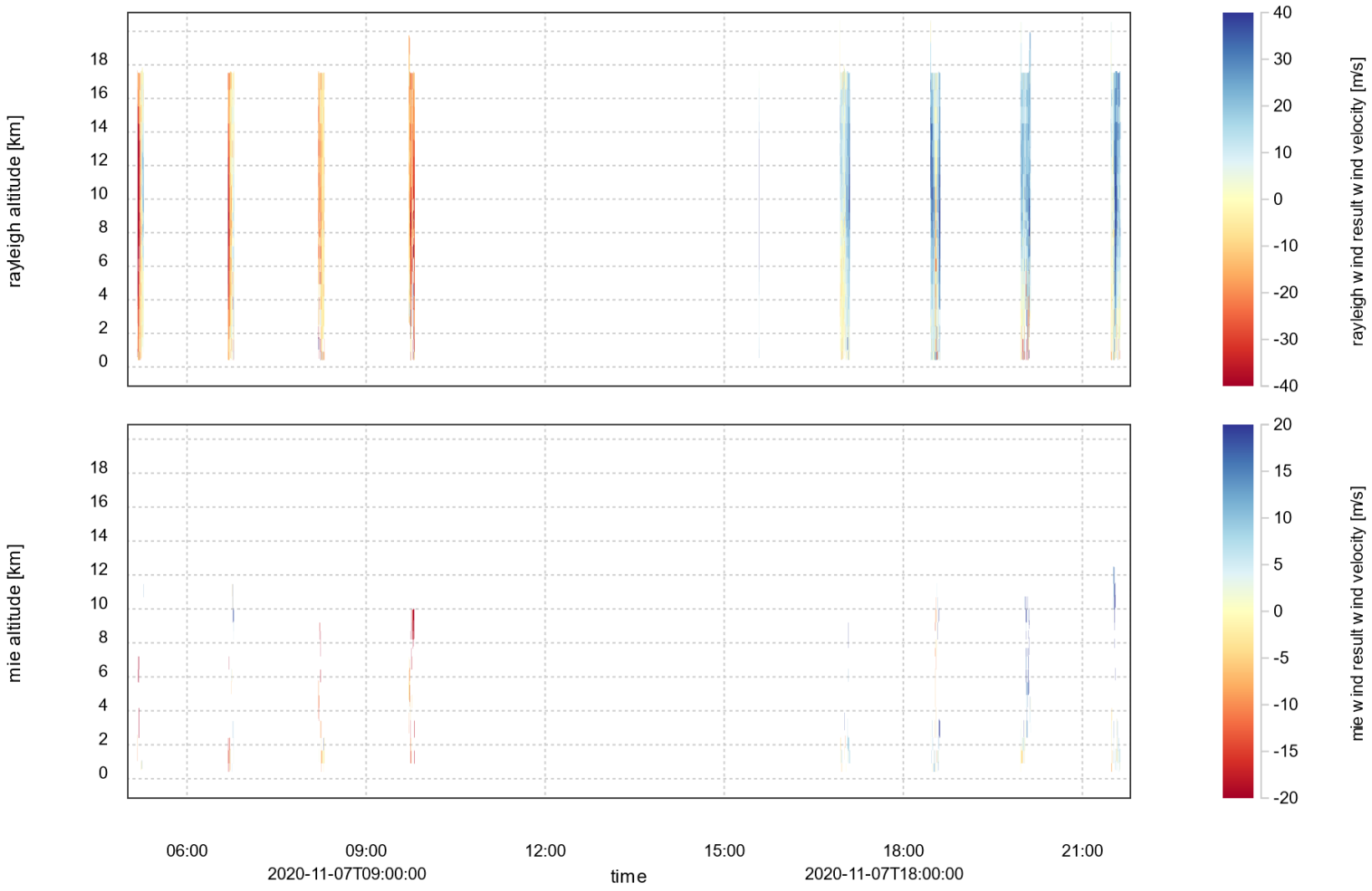
Zoom into the data of Nov, 5th



6th November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



7th November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest:



8th November 2020, Rayleigh-clear winds (upper plot) and Mie-cloudy winds (lower plot) in the geographical area of interest: