Answer to the comments of referee 2 (RC 2):

We would like to thank the anonymous reviewer for his / her valuable comments and additional information. I found them very helpful and changed the manuscript accordingly in most cases.

Since the other reviewer had some serious concerns about the nomenclature “E_kin,low” (Aeolus doesn’t have the best precision, which might lead to a bias in the derivation of E_kin; that’s why “E_kin,low” might be misleading”), I changed it to “E_kin”. This is just an info in order to reduce the risk of confusion when having the first version of the manuscript in mind.

Two further remarks: when I read the manuscript, I found three further minor issues, which I corrected:

- In table 1, I changed the unit of the variable “alt” to from “M” to “m”. Furthermore, I replaced “RD7” in row 4 of this table by “ADM-Aeolus Level-2B Algorithm Theoretical Baseline Document“
- p.14, l. 295: I changed “section 5.1” to “section 5”. This was a relict of a previous version.

I tried to avoid page and line numbers but where I used them I refer to the new version with accepted changes.

Throughout this review, the format "PXX.LXX Comment" is used to refer to the page number "PXX" and line number "LXX" in the originally submitted manuscript corresponding to each comment.

SHORT SUMMARY

This is a review of the paper titled "Gravity waves above the Northern Atlantic and Europe during streamer events using ADM-Aeolus", submitted to Atmospheric Measurement Techniques. This paper investigates the potential for Aeolus measurements to provide gravity wave (GW) signals during so-called "streamer events" above the Northern Atlantic and Europe, and in particular, to derive a lower limit for the GW kinetic energy density (E_kin_low) during such events. Two example cases are analysed, and a temporal correlation is found between the daily averaged E_kin_low and the occurence of each streamer event, with enhanced values observed for each. The authors also consider the spatial distribution of the kinetic energy density signals from Aeolus during one of these events, however no significant pattern can be found. Cubic splines are used to approximate the atmospheric background and retrieve wind residuals, and the analysis is split into a tropospheric and a stratospheric part to exclude the tropopause wind maximum.

This manuscript is interesting and has the potential to fulfil the scope of AMT, the results are presented in a balanced manner and some of the scientific quality is good. However, a major revision is likely to be necessary in order to address the following three issues.

(i) The study itself is rather focused and could be improved by both a broader development of the GW analysis technique and a wider analysis of the streamer events mentioned and/or of other similar events.

(ii) There are some outstanding questions regarding the validity of the analysis technique for measuring GWs, with the inherent uncertainties in the Aeolus data. This may just require some clarification.

(iii) Although there is a good clarity and concision to the overall writing of the manuscript, the figures and presentation of results will require further improvement to a higher quality.

Therefore, publication can be recommended only after the following issues and suggestions are resolved or considered.

GENERAL COMMENTS

1) The term "ADM" (abbreviation of Atmospheric Dynamics Mission) was omitted from the satellite's name by ESA several years ago. Please update all instances of "ADM-Aeolus" to "Aeolus" to conform with convention, noting this previous AMT review in particular, general comment #2: https://egusphere.copernicus.org/preprints/2023/egusphere-2023-1924/egusphere-2023-1924-RC1-supplement.pdf

Corrected

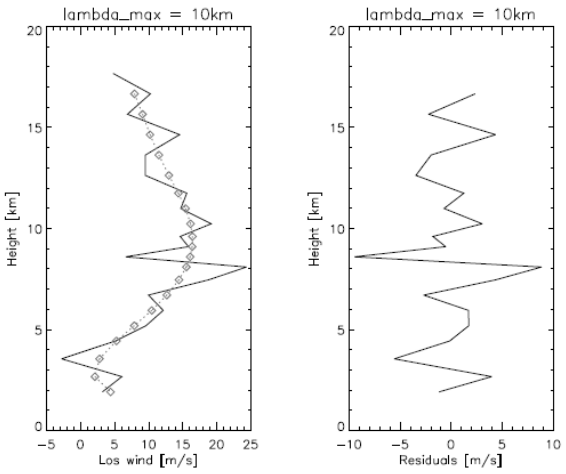
2) P01.L23 With respect to issues (i) and (ii), it is not completely obvious how sharp regions of vertical wind shear, which may define the slanted edges of jet streaks and other non-GW tropospheric wind phenomena, are prevented from contaminating the signals appearing in the GW analysis of Aeolus data. Are these non-GW features captured correctly in the background using the cubic spline method? This is particularly an issue in the troposphere, since the stratosphere is generally stable and stratified, such that vertical perturbations in horizontal wind can be more easily attributed to GWs here. Attached to this review is an along-track Aeolus "quick-look" profile time-series for 2020-02-10 in the region of interest, showing an intense jet streak associated with one of the streamer events in question (Source: https://aeolus.services/). Might there be an increase in non-GW wind perturbations, which inherently occurs during these dynamic streamer events, which could contribute to the results and/or explain some of the temporal coincidence that is found? If this is accounted for, some clarification of this within the text would be helpful. Optional Suggestion: Perhaps an example of the raw Aeolus data from the time period being analysed could be shown at some point to help orient the reader?

When extracting gravity waves from vertical profiles (this includes not only wind, but also other parameters such as temperature or ozone), it is unfortunately a common problem: not every signal in the residuals can necessarily be attributed to a gravity wave. Individual residual profiles can be checked by eye for unusual signals, but for a large number of profiles, some thousands as is the case here, this is only possible in parts. This is also what I did: I looked at some de-trended Aeolus profiles. In many cases, the peak in the tropopause fits well into the oscillation observed before and after, i.e. it does not “disturb” the vertical wavelength (see attached figure, this is an Aeolus profile from 4th Nov. 2020 (time period of streamer 1) on the left side and the detrended version on the right side).

Furthermore, in order to reduce the risk of signals in the residuals that are not attributable to gravity waves, it is helpful to exclude typical areas that lead to the generation of "artificial" signals in the residuals. The most important areas here are the atmospheric pauses, in our case the tropopause. That is why I restricted the analysis to the height range below 7 km and above 11 km.

Additionally, I used a method that can better reproduce strong deflections than the classical spline method due to the variation of its starting points.

In order to make this whole topic a little bit clearer, I included an additional part in the discussion where I specifically address this problem (p. 23, ll. 474 ff.).



3) P23.L450 - P23.L460 The question of whether or not the detrending method is affected by the tropopause wind maximum as an artefact, contaminating the GW signal, is quite an important one. Do the authors think this peak in residual winds is a consequence of this artefact, or is it more attributable to GW filtering, as this paragraph suggests? Will Aeolus have the capability to measure an increase in GW amplitudes which is caused by secondary GWs? An expansion of this discussion and perhaps an exploration of further GW statistics would be useful, particularly to answer these and similar questions.

We cannot say with certainty whether the peak in the area of the tropopause is due to gravity waves or to insufficient de-trending in the area of the tropopause. For this reason, I excluded this area from the analyses as described at the beginning of section 5, but also pointed out in the discussion (section 6, the part you address here) that there may well be physical reasons for this peak. A hint that the detrending performs not too bad is that both, negative and positive residuals versus height (Figure 7b) are highly symmetrical: this suggests that, at least on average, the detrending does not introduce strong biases. This is now mentioned at the beginning of section 5 (p. 16, ll. 326 ff). Whether Aeolus is sensitive to secondary waves depends on the amplitude of those waves.

An expansion of the discussion would be of interest, however, from my point of view, it would be highly speculative.

4) P26.L484 The question of the manuscript seems rather focussed to me, and could be expanded and generalised a little so that this work is more applicable as an atmospheric measurement technique for future gravity wave analyses using Aeolus. Whether this is formed from a slightly broader GW analysis, looking deeper than the kinetic energy density, or whether additional events are required to test the conclusions that have been drawn; both may help to answer some of the current questions that remain about this study.

The question in this manuscript is indeed focused. We focused on one possible source of gravity waves (streamers, which to our knowledge have not been explicitly addressed as a source of gravity waves in the literature) and selected the two most clearly observable events that fell within the project period. Streamers are not known to be a strong source of gravity waves, and there is only one publication dealing with gravity waves from Aeolus. This is an extremely high amplitude gravity wave. Due to the spatial extent of a streamer event and the challenging data quality of Aeolus for these analyses, it was necessary to examine a relatively large spatial area, and even in this case, i.e. when analysing the two clearest streamer events, only a significant but not unusual increase in the kinetic energy density in the stratosphere is seen. The combination of TIMED-SABER, e.g. to derive the potential energy density, adds little or no value due to the different overflight times and locations. The extension to other gravity wave sources would dilute the focus of the manuscript.

5) Are the authors aware of work such as Wiegand and Knippertz, 2013 (https://doi.org/10.1002/qj.2112), Wernli and Springer, 2007 (https://doi.org/10.1175/JAS3912.1) and Madonna et al., 2014 (https://doi.org/10.1175/JAS-D-14-0119.1)? These may be useful to read and as a references.

Thank you for the citations. Madonna et al. (2014) refer to potential vorticity (PV) streamers as defined by Appenzeller and Davies (1992), see first page second column of their publication. We refer to another term of streamers following Offermann et al. (1999). Krüger et al. (2005), who investigate the same kind of streamers as Offermann and we, write in their manuscript: “The term used in this study should not be mistaken with the terminology of “streamers” of a smaller-scale first introduced by Appenzeller and Davies (1992) and Appenzeller et al. (1996), describing stratospheric intrusions into the troposphere.” Also, Wernli and Sprenger (2007) and Wiegand and Knipperts (2013) refer to PV vorticity streamers.

SPECIFIC COMMENTS

P01.L16 Replace "Aeolus on ADM" with "Aeolus". ALADIN is the instrument onboard, as mentioned later in the article.

Done

P01.L17 (also P03.L77) The phrase "from the ground to the lower stratosphere (20 - 30 km)" is a little ambiguous. The ground is of course at 0 km, and the stratosphere also begins lower than 20 km, so this should be rephrased slightly.

I changed it to „ from the ground to an altitude of ca. 20 – 30 km ...“

P01.L25 Do the results indicate this, or is this an implicit assumption? As the authors mention later, Banyard et al. 2021 has shown that this is possible, although only for the single case of a strong GW.

That’s a good point. I changed the sentence to “The derivation of GW signals based on Aeolus data is possible, however, we collected about 100 profiles to statistically reduce the uncertainty of the daily averaged E_kin.”

P02.L31 It's slightly confusing to say that GWs dominate atmospheric dynamics over other wave phenomena, even if this is qualified by specifying that this is "especially above 75 km height". Perhaps this part could be rephrased? This study does not go higher than 24 km.

I changed the sentence to “Especially above 75 km height, GWs dominate atmospheric dynamics through the deposition of energy and momentum, even though there exist wave phenomena with larger periods and wavelengths in the atmosphere (Houghton, 2002).”

P03.L84 The phrase "Challenging is the accuracy of Aeolus" reads a little strangely.

I changed it to “One challenge is the accuracy of Aeolus,…”

P05.L107 Can streamer events be linked to strong cyclones?

Good question. In our cases, the dominating pressure system was an anticyclone (see also https://earth.nullschool.net/#2020/11/04/0900Z/wind/isobaric/250hPa/orthographic=-20.72,38.04,312 for 4th Nov. 2020, 250 hPa, and https://earth.nullschool.net/#2020/02/09/0900Z/wind/isobaric/250hPa/orthographic=-20.72,38.04,312 for 9th Feb. 2020, 250 hPa).

In this manuscript, we focus on tropical-subtropical streamers, i.e. air is transported northwards from the tropics. As can be seen from the two internet addresses given, the zonal jet is strongly deflected northwards here. Accordingly, there should be an anticyclone to the east of the zonal jet. I would therefore expect this type of streamer to be identified with high pressure areas. However, as I have not found any relevant literature on this and as we only treat one specific kind of streamer (so not the polar ones) in this manuscript, I changed “anticyclone” to “pressure system” in the text to keep it more general.

P05.L112 low latitude or low potential vorticity?

Clarified (low latitudes).

P08.L172 Could you please clarify how you corrected the hlos wind for the satellite observation geometry?

We already explained it in the following section. I added a hint towards it in brackets.

P08.L176 - P08.L185 Could this paragraph be written a little more succinctly? It's easy to get lost here at the moment.

I rearranged this paragraph – it is not shorter now but I hope that it is easier to follow the argumentation.

P12.L251 For clarification, is the use of varying starting points applied in order to combat both the insufficient approximation of extrema in the background and the artificial oscillations generated?

Yes, it is.

P13.L268 This sentence is a little unclear, could it be rephrased slightly?

I changed it to “Based on this algorithm, maxima of mean E_{kin} over the area or time period defined at the beginning of the algorithm can be identified, thus GW hotspots in place or time.”

P14.L291 By "CIRA" are you referring to the COSPAR International Reference Atmosphere? This needs defining.

Yes and done.

P21.L378 Is it possible to suggest reasons for the GWs captured in the analysis of these two streamer events? E.g. Is it unstable shears, geostropic adjustment, the result of interaction between other GWs? It would be good to have some additional discussion on this topic here.

That’s a little bit tricky, since it is more speculation than knowledge, as we have no information about the precise location of the GW we observe and therefore can’t trace them back. I added the following: “GFS data at 250 hPa, for example, show a strong anticyclone, which is linked to downward vertical movement in the centre near the position of the streamer in both cases investigated here (e.g. https://earth.nullschool.net/#2020/11/04/0600Z/wind/isobaric/250hPa/orthographic=-29.56,45.15,312 for streamer 1, and https://earth.nullschool.net/#2020/02/09/0600Z/wind/isobaric/250hPa/orthographic=-29.56,45.15,312 for streamer 2, last access: 27th May 2024). Additionally, a strong vertical shear of the horizontal wind can be observed when addressing different heights. Also frontal activity is present (https://www.wetterzentrale.de/reanalysis.php?jaar=2020&maand=11&dag=4&uur=000&var=45&map=1&model=dwd for streamer 1, and https://www.wetterzentrale.de/reanalysis.php?jaar=2020&maand=2&dag=9&uur=000&var=45&map=1&model=dwd for streamer 2, last access: 27th May 2024).”

P22.L442 Aeolus measurements are "off-nadir", even though ALADIN looks to the side of Aeolus, it is not a limb viewer.

I changed it to “Aeolus as an off-nadir viewer (35° incidence angle) looks obliquely through the atmosphere and collects all information along the line of sight.”.

P23.L447 It might be useful to explain here a little more about why it is the lower limit of the kinetic energy density that you are measuring.

I changed it to: “In conclusion, that underpins that E_kin is strictly speaking only a lower limit: this is due to the effects just mentioned and to the fact that the instrument measures only along the line of sight and therefore not the whole 3D wind vector. However, a low precision of the Aeolus measurements can lead to a significant bias in the derived E_kin values.”

P26.L503 - P27.L517 This is a good and useful discussion which highlights some of the potential future benefits of a succeeding mission to Aeolus and raises important suggestions for optimising its suitability for GW analysis.

Thank you! I appreciate.

FIGURE COMMENTS

Figure 1 The latitude and longitude labels need to be larger and not obscured by the colour bar. Is there a higher resolution version of the image shown?

The latitude and longitude labels are now larger and not obscured by the colour bar any more. The resolution is improved.

Figure 2 Please add latitude and longitude labels.

Done. To make it more concise, we use the same projection as for figure 1 now.

Figure 7 It could be made a little clearer within the plot itself that 7b shows the negative and 7c the positive residuals.

Done

Figure 8 Since 8c is just the green plot from 8b repeated, is there a way to simplify the reading of the entire figure somehow? Having labels on the figure to quickly identify which subplots correspond to the tropospheric and stratospheric parts may be helpful.

Done

Figure 9 Can these figures be superimposed on one another, or is it better to separate the tropospheric and stratospheric components for all figures? It feels like this could be presented in a slightly clearer way, as you have to look quite carefully to understand what each subplot shows at the moment.

Combined in one figure

Figure 10 The filled contour plotting here is a little confusing, would a different colour scale be better to use? Perhaps a different plotting technique and layout would make it easier to see the changes from week to week? Please put the dates as well as the week numbers.

I inserted the dates. When I created this figure, I tried different colour scales. In the end, I chose this one because I think it is relatively intuitive (low values = blue, high values = red, another colour for medium values).

Figure 11 It is not immediately clear that these figures show the heights of the profile's first and second wind maxima. Is it possible to show this as some sort of density plot with height on the y axis and two variables on the x axis (Var 1 being the height of the first maxima, Var 2 being the heights of the first and second maxima)? Histograms may be fine if they are slightly altered, but they are a bit unclear at present.

I changed the histograms. For me, they seem to be more intuitive now.

All Figures: In general, most of the figures need some improvement in quality to make them clearer and easier to understand. An additional figure to give more context for Aeolus measurements may be helpful, and a couple more may be required for any further analysis that is conducted.

TYPOGRAPHICAL ERRORS

PXX.LXX New (Old)

P02.L33 which can (which are can)
Corrected

P02.L35 GWs (GW)
Corrected

P02.L37 GWs (GW)
Corrected

P02.L38 improving (improve)
Corrected

P03.L59 However, parts of the GW spectrum, equation (3) can be simplified (However, for parts of the GW spectrum equation (3) can by simplified)
Are you sure? I would like to express that equation (3) holds only for s specific part of the GW spectrum

P03.L63 frequently (frequent)
Corrected

P03.L71 Doppler (doppler)
Corrected

P03.L75 Aeolus (Aeolus on ADM)
Corrected

P03.L81 These data have (These data has)
Corrected

P05.L135 clearly (definitely)
Corrected

P06.L139 reaches latitudes of 70 degrees N (reaches regions latitude of 70 degrees N)
Corrected

P08.L174 netCDF (ncdf)
Corrected

Check Kruger
Does this comment really refer to this page and line? Is it maybe an artefact?

P12.L243 adaptation (adaption)
Corrected, also later in the manuscript (p. 14, l. 291)

P20.L365 Figure 7 (Figure 6)
Corrected

P26.L484 The question which we addressed in this manuscript was whether (The question, we addressed in this manuscript was, whether)

Corrected