

Reponse to reviewer #2

We would like to extend our sincere thanks to Reviewer #2 for their continued feedback on this new iteration. We appreciate the commendation that the results feel more robust, and we agree that addressing the remaining points has further improved the paper's clarity and focus.

We have addressed all of the reviewer's comments, which are detailed below.

I commend the authors for addressing all my comments -- which was quite some work! I am satisfied with most of the revisions and I feel the results are very robust now.

I have a number of remaining minor/technical comments, and only a couple of bigger 'complaints' from my side. While due to the overall amount of comments it almost sums to another major revision, it all should be nevertheless fast and straightforward to address.

#

My two remaining complaints:

- 1) The continuous color scale in most figures, would be much easier to interpret and guess the numbers by eye, if the color scale had discrete steps.
- 2) Too much focus and speculation about the MJO and related topics in the discussion, while I can't notice any MJO-like signal in any of the figures.

We agree completely both suggestions. Continuous color scales can indeed be difficult to interpret precisely. We have updated all relevant figures (Figures 2, 3, 4, 5, 6, and 7) to use discrete color scales.

Upon review, we agree that our discussion of the MJO was overly speculative and not directly supported by the temporal resolution of our analysis. The dominant signal in our figures is indeed the seasonal cycle. Accordingly, we have completely rewritten the relevant paragraphs in the Discussion section (previously lines 560-573 and 612-620). All speculative mentions of the MJO have been removed. The revised discussion now focuses squarely on the robust link between the observed gravity wave activity and the seasonal cycle of major tropical monsoon systems. Similarly, we have refocused the discussion on the QBO to be less speculative and more directly tied to recent findings from the Aeolus mission itself, incorporating the suggested literature.

There are numerous specific comments related to 1) and 2) listed below:

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Specific comments:

- l.28: specify the study period.

Done

--> same for the 1st paragraph of section 2.1, the analysis time period should be specified also there.

Done

- l.29: remove "referred to as SO3 in"

Done

- About the GWD scheme used in ERA5, this should be mentioned again in the 2.1 subsection and in your discussion regarding Fig.3.

We added the following line in the 2.1 subsection:

(lines 106-108)

For representing sub-grid scale gravity waves, the ERA5 configuration used in this study employs a non-orographic GWD parameterization that is not directly forced by model-diagnosed convection (Orr et al., 2010). Instead, the scheme launches a globally uniform spectrum of waves from the troposphere.

And the following line in the discussion:

(lines 425-428)

The difference between the two datasets, shown in Fig.3c, quantifies this discrepancy. The plot is overwhelmingly positive, indicating a systematic and significant underestimation of GW kinetic energy by ERA5 throughout the tropics. The regions of greatest underestimation, where the difference exceeds 10 J/kg, align almost perfectly with areas of deep convection, as identified by the low OLR contours. This last element reinforces the conclusion that ERA5's key limitation lies in its representation of convection-driven wave activity. This finding is consistent with the fact that ERA5's non-orographic GWD scheme is not directly coupled to model-diagnosed convection, highlighting the need for improved parameterizations to better capture these sources.

- l.83-84: perhaps describe more specifics of RBS in your region of study in the previous sentence (like what resolution exactly it has in the UTLS height range), and here just state that RBS are different in the extratropics/polar regions.

We have revised the text to provide the typical vertical resolution for our study region upfront and have clarified that the Range Bin Settings are geographically dependent. The revised sentence now reads:

(lines 83-86)

within the tropical UTLS region of this study, the vertical bin size is typically between 0.5 and 1.5 km. The distribution of these range bins is determined by a dedicated range bin setting (RBS), which varies geographically to meet different observational goals, with distinct configurations routinely used for the tropics, extratropics, and polar regions.

- l.104: interpolated how? (refer to methods subsection)

Thank you for pointing out that the description of our interpolation method was not sufficiently detailed. The revised text now reads:

(lines 108-111)

For this study, wind components are retrieved on the native 137 model levels. To prepare the data for analysis, the geopotential height of each model level is first converted to geometric altitude. The vertical profiles are then linearly interpolated from this native geometric altitude grid onto the standard 100 m high-resolution grid used for all datasets in this study.

Regarding the suggestion to refer to the Methods subsection (2.2), we ultimately chose to include this concise, two-sentence explanation directly within the description of the ERA5 dataset in Section 2.1. We believe this approach provides the necessary clarity for the reader at the moment the dataset is introduced, without requiring them to cross-reference another section for this specific detail.

- l.124-125: reference needed

Added reference to Schmidt et al., 2016 that directly references this method.

- l.132-133: to make the sentence more concise, substitute "is intentional. This... by treating it as" --> serves as a comparison with

Done

- l.167-168: Ern et al (2023) proved this, also Zagar et al. (2025) detail more about Kelvin waves and shear.

Reworked the following paragraph:

(Lines 164-171)

Furthermore, data assimilation studies have demonstrated that the inclusion of Aeolus wind data directly impacts the representation of vertically propagating Kelvin waves in numerical weather prediction models. This impact is explicitly linked to the background wind, with the largest analysis changes occurring in regions of strong vertical wind shear (Žagar et al., 2021, 2025). This highlights the importance of direct wind observations in these critical regions. Indeed, direct analysis of Aeolus observations (without assimilation) confirms that Kelvin waves are well-resolved, showing good agreement in wave variances when compared to reanalyses (Ern et al., 2023). This implies that the characteristics of Kelvin waves seen by Aeolus are robust and may differ from those in reanalyses not assimilating Aeolus data.

- l.226-227: "The lower bound is set..." --> sentence can be removed without loss of any info

Done

- l.228: tropopause height, specify how it is defined, e.g. cold-point from model levels?

We have removed the redundant sentence and have added a clear definition of how the tropopause height was determined (WMO thermal definition) at lines 235-238.

- l.278-279: cite Lux et al (2022) for this

Done

- Figure 2: please improve the color scale, make separate colors every 1 or 2 J/Kg -- i.e. make it discrete, not continuous -- add another color beyond yellow.

Done

- l.314: "Boreal spring 2019 to Austral summer 2020" is an extremely awkward way to state your analysis period. Simply JJA 2019 to MAM 2021 does the job, without confusing the reader.

Done

- l.315: patterns, describe which ones.

We have explicitly described the two main large-scale patterns observed:

(lines 329-332)

This comparison reveals both key similarities in two large-scale patterns: first, the confinement of most GW kinetic energy to the equatorial belt (approximately 15°S–15°N), and second, a distinct seasonal migration of this energy. However, there are also significant differences in the representation of regional wave activity.

- l.318-319: --> The reader should note that also some variance from equatorial waves, centered at the equator (by definition), will be inevitably present to some small degree.

Added as is at lines 335-337

- l.324-325: In ERA5 a lot is missing especially in the active monsoon regions // further away from the Equator, please note this in the text.

The following text has been added on the following paragraph discussing differences:

(lines 360-362)

ERA5 tends to represent GW activity as a relatively smooth, zonally elongated band, with modest seasonal modulation and appears to significantly miss wave activity both in the active monsoon regions and in more structured events further from the equator.

- l.334-338: Again, I feel seasonal monsoon convection is missing in this discussion.

We agree that the paragraph discussing subtropical jet sources needed to be more clearly contrasted with the convectively-generated gravity waves from tropical monsoon systems. To address this, we have added a concluding sentence to the paragraph that explicitly makes this distinction and clarifies the relative importance of the two sources as indicated by our results. The new sentences read:

(lines 356-359)

These jet- and front-generated waves are dynamically distinct from the deep tropical convection associated with the major seasonal monsoon systems. While the subtropical jets produce notable GW activity, our results indicate that the most intense and geographically extensive hotspots are found within the equatorial belt and are closely tied to these monsoon systems (Kang et al., 2017; Wright and Gille, 2011).

- l.349-356: Perhaps mention that this will be looked in more detail in the next subsection in relation to OLR.

--> Figure 3 confirms your GW hotspots follow deep convective systems

We have added a sentence at lines 378-380 to transition the reader to the next section

- Figure 3:

--> Especially in panels a-b, I see the same problem as with Fig.2: please make the color scale discrete (in every panels would be best)

Done

--> Longrange --> Longwave!

Done

--> I think there might be some mask where positive values (red) are not completely transparent to the stippling, so it appears light gray.

Stippling within blue regions looks fine.

I assume the stippling is intended to be the same everywhere

Fixed

- l.373: "the observations" --> more precision needed here -- Aeolus and ERA5 (HLOS) GW and their difference.

Done

- l.375-377: need to mention the OLR patterns that coincide with your GW hotspots

The new text now reads:

(lines 403-404)

This migration of high Ek is systematically co-located with the seasonal cycle of convection, with the hotspots consistently falling within the low OLR contours (below 220 W/m²).

- l.386-388: again your figure shows this is the case -- GW hotspots following low OLR regions, please discuss this and state the OLR values shown in the figure.

The revised paragraph now includes the following sentence:

(Lines 412-414)

The strong spatial correlation shown in Figure 3a between the most intense kinetic energy observed by Aeolus and the lowest OLR values (< 210 W/m²) provides evidence that these mechanisms are the primary drivers of the observed GWs.

- l.397-399: Better to simply state it's a good estimator of cloud top temperature and thereby convection depth.

We have adopted the more concise physical description of OLR as suggested. Furthermore, we recognized that explaining OLR after having already discussed its patterns was structurally awkward. Therefore, we have moved this new, concise definition to the beginning of Section 3.2, where the OLR contours are first introduced to the reader. The text now introduces OLR as:

a reliable proxy for deep convection as it indicates cold, high-altitude cloud tops and thus the depth of convective systems (Zhang et al., 2017).

- l.400: "primary weakness"

--> not sure about calling it a primary weakness...

These convective GW's are not parameterized basically, from what I understand from your introduction?

This result from Fig. 3 highlights the need to parameterize convectively generated GWs better.

The former paragraph got reworked. We agree that using the word weakness was misleading as ERA5 does not have a proper parameterization:

(lines 425-428)

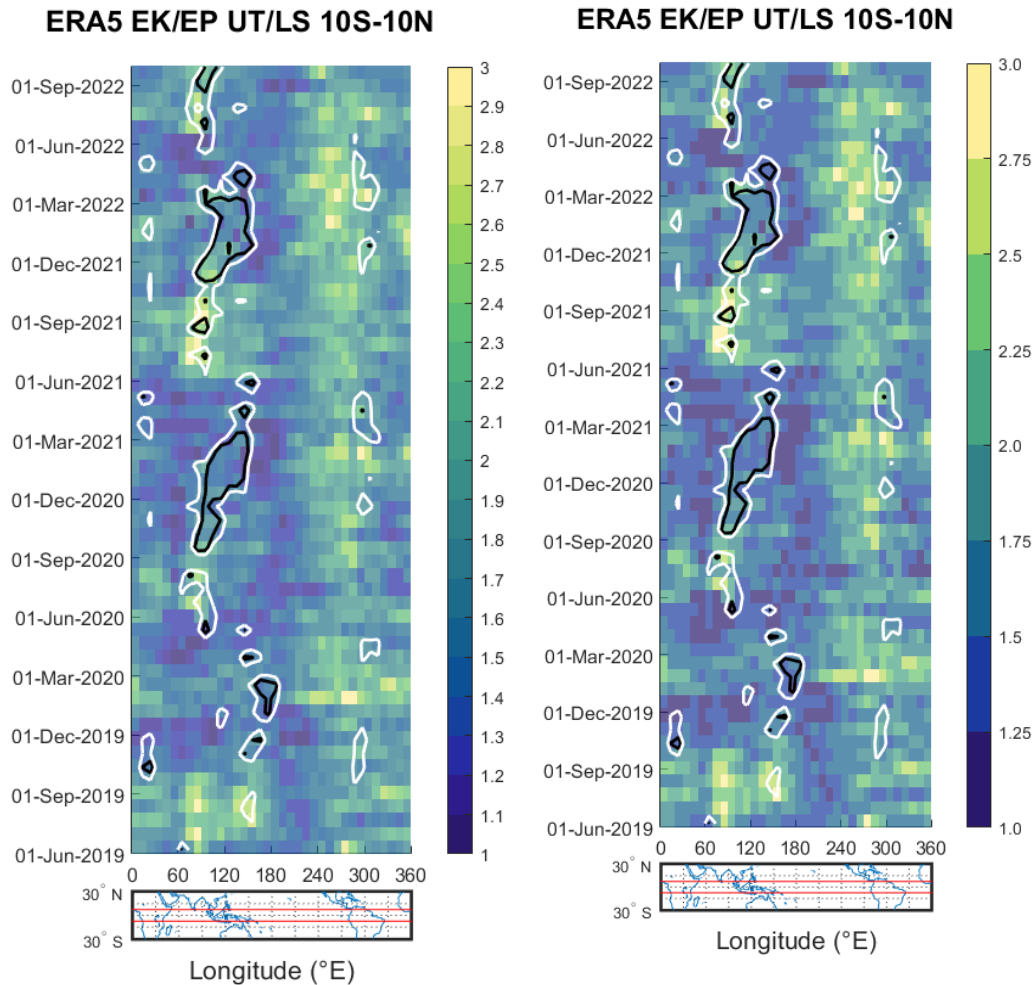
The difference between the two datasets, shown in Fig.3c, quantifies this discrepancy. The plot is overwhelmingly positive, indicating a systematic and significant underestimation of GW kinetic energy by ERA5 throughout the tropics. The regions of greatest underestimation, where the difference exceeds 10 J/kg, align almost perfectly with areas of deep convection, as identified by the low OLR contours. This last element reinforces the conclusion that ERA5's key limitation lies in its representation of convection-driven wave activity. This finding is consistent with the fact that ERA5's non-orographic GWD scheme is not directly coupled to model-diagnosed convection, highlighting the need for improved parameterizations to better capture these sources.

- l.410-411: Either be more specific about what metrics you refer to, or remove this sentence (no info lost if removed)

Sentence removed

- Figure 4: Please improve color scale with a discrete separation of colors every 0.1.

As it is now, it's impossible to tell by eye where exactly the 2 ratio is, or whether it's 1.8 instead.



As shown in these two examples, we decided that a 0.25 discrete separation served the figure better than 0.1, which was still quite hard to distinguish.

- I.423-424: the caption about OLR --> Can be shortened to "white and black contour lines represent 210 and 220 W/m² OLR, respectively."

Updated all captions except for the first one on figure 3.

- I.438: Oscillations --> Oscillation (no plurals with MJO)

Done.

- I.447: dynamically active, you mean convectively?

Yes, we added that precision.

- I.456: each line--> each row

Done.

- I.457: I don't see any white bins

Removed that sentence.

- Figures 5-6-7: same as previous figures, please make a discrete color scale.

Done.

- I.489: "lesser convective areas" --> you mean non-convective?

Perhaps consider adding a contour line with high OLR indicating stable conditions into Fig. 6.

Changed the wording and added the high OLR contour.

- I.506-512: I'd like to see a longer discussion comparing it with Fig. 3 in terms of general values, peaks and sign of the differences

--> Good agreement with ERA5 in E_p , but I see lots of (light red) color in Fig.6c.

--> Poor agreement with ERA5 in Fig.3 --> clear underestimation of E_k in convective regions (red colors), but what about the general blue color (although insignificant?) elsewhere.

The following paragraph replaced the older one, proposing a more detailed discussion on the comparison between both figures, addressing the quantitative discrepancies:

(lines 527-541)

The differences between ERA5 and GNSS-RO data, depicted in Fig. 6c, show a mean absolute difference of 1.96 J/kg. This reflects a reasonable agreement, given that ERA5 assimilates GNSS-RO measurements. While there is a slight positive mean bias of 1.68 J/kg (GNSS-RO > ERA5), which accounts for the prevalence of light red colors in the plot, the differences are scattered and show no large-scale, systematic pattern correlated with convection. This stands in stark contrast to the systematic and large discrepancies observed in the kinetic energy fields.

The E_k differences are not only larger in magnitude, with a standard deviation nearly twice that of E_p (3.16 J/kg vs. 1.82 J/kg) and a maximum underestimation by ERA5 that is almost three times greater (>24 J/kg vs. ~9 J/kg for E_p), but they are also structurally different. The E_k difference plot is dominated by large, cohesive regions of statistically significant positive values (red), indicating a systematic underestimation by ERA5. While some areas do show a negative difference (blue color), these are of small magnitude and, as confirmed by the lack of stippling, are not statistically significant. Most importantly, the peak underestimation of E_k is systematically co-located with the deepest convective regions (inside the low OLR contours), whereas the minor differences in E_p show no such alignment. Taken together, this evidence points to a specific limitation in the reanalysis: the issue is not a general failure to represent wave energy, but a targeted inability of the model's physics and data assimilation system to generate the intense, localized kinetic component of gravity waves originating from strong convection in data-sparse regions.

-I. 535: "including orographic influences": there is no orography at 200 lon

Changed to "the distribution of large land masses"

- I.541: "RO temperature data" --> maybe simply state "RO measurements".

what is actually being assimilated are GNSS-RO bending angles (which contains the temperature information within).

Changed to “GNSS-RO measurements, specifically bending angles which contain temperature information”

- I.558: to me inconsistent implies lots of ups and downs.

--> perhaps decaying performance is more suitable here?

Yes, changed.

- I.564-565: Seasonal cycle or monsoon could be as well.

- I.560-573: Whole MJO discussion is very speculative and not backed by any result of yours. What one can see clearly in Figs. 2 and 3 is the seasonal cycle.

MJO has a timescale of 30-90 days, meaning full positive-negative phase within 3 months at most, moving from the Indian Ocean into the Pacific: sorry but I don't see anything on that timescale in these figures.

We agree that our original discussion overemphasized the role of the MJO, which is not clearly resolved in our analysis, and that the dominant signal in our figures is indeed the seasonal cycle. Following this advice, we have completely removed the speculative discussion about the MJO. We have rewritten the entire paragraph (previously lines 560-573) to focus squarely on the link between the observed gravity wave activity and the seasonal cycle of the major tropical monsoon systems, which is strongly supported by our figures and a comprehensive body of literature. The revised paragraph now reads:

(lines 589-602)

The analysis of ALADIN wind profiling and ECMWF ERA5 reanalysis data, provided in Fig.2 and Fig.3, revealed enhanced GW activity over the Indian Ocean during Boreal Summer, as well as over the western Pacific and maritime continent in Boreal Winter. The migration of this enhanced GW activity from eastern Africa to the Pacific maritime continent follows a clear seasonal cycle, strongly linked to deep convection as shown by the correlation with regional OLR minima. This robust seasonal pattern indicates that the underlying wave sources are organized by planetary-scale phenomena, primarily the major tropical monsoon systems (Wright and Gille, 2011). The structures observed by Aeolus are therefore highly consistent with the kinetic energy signature of gravity waves generated by the powerful thermal and mechanical forcing mechanisms (Beres et al., 2005; Corcos et al., 2025) known to occur within the large, organized convective systems of the Asian, African, and Maritime Continent monsoons (Kang et al., 2017; Liu et al., 2022). Previous satellite climatologies have firmly established these monsoon regions as dominant global hotspots for stratospheric gravity wave activity (Hindley et al., 2020; Wright and Gille, 2011). This suggests that Aeolus is effectively capturing these seasonally-driven, convection-induced GWs that are underrepresented in ERA5. One of the persistent features observed throughout the study was the high-energy gravity wave hotspot over the African continent, which remained consistent across seasons and years. This suggests a continuous mechanism of continental convection driving gravity wave activity in this region.

- I.574-583: this is actually an important result and should belong in an earlier result section and not here.

Expand the discussion in lines 230-235 with the infos from this paragraph.

You may refer to this in the discussion/conclusions in a summarized manner later.

The following text was added to directly explain the results:

(Lines 244-249)

By shifting the analysis layer upward to such levels, we confirm that the geographical patterns of the energy hotspots are remarkably stable (spatial correlation $r > 0.83$), and that the vast majority of the peak energy (~88-91%) persists well into the stratosphere. If the signal were dominated by shallow tropospheric outflow, the energy peaks would have collapsed when the analysis layer was moved above the tropopause. The fact that a strong, structured signal remains confirms that our method is observing vertically propagating gravity waves that have penetrated the lower stratosphere.

And the discussion section element was shortened and integrated at the start of the next paragraph:

Having established that the Aeolus kinetic energy signal is robust and represents vertically propagating stratospheric gravity waves rather than tropospheric artifacts (as confirmed by our sensitivity analysis in Sect. 2.2), we can use external information to arbitrate the cause of the discrepancy with ERA5.

- I.601-607: Newer papers from Žagar on Aeolus and equatorial waves (2021, and especially 2025) would help with this discussion.

The IFS has evolved quite a lot from 2004.

We agree that our discussion needed to be updated to reflect the evolution of the IFS and to incorporate the latest findings from the Aeolus mission. To address this, we have revised the paragraph by adding the newer references to demonstrate that this issue remains relevant today:

(lines 622-632)

These statistical relationships are primarily designed to represent large-scale, quasi-balanced (rotational) flow and have long been known to be less effective at specifying the smaller-scale, divergent component of the wind field to which convectively generated gravity waves belong, especially in the tropics (Žagar et al., 2004). While the Integrated Forecasting System (IFS) has evolved considerably, recent Observing System Experiments (OSEs) using Aeolus data confirm that this challenge persists. These studies provide direct evidence that the assimilation of Aeolus wind profiles systematically enhances the analyzed amplitudes of equatorial waves, particularly in regions of strong vertical wind shear where the model's background state is most uncertain (Žagar et al., 2021, 2025). Consequently, while the assimilation of GNSS-RO constrains the thermodynamic (E_p) aspect of the wave, the system lacks the necessary information and dynamic constraints to generate the corresponding divergent wind perturbations, leading to the observed Ek deficit. This process evidently fails to capture the full spectrum of high-Ek wave modes generated by convection.

- l.612-620: too speculative, was there any remarkable MJO event during your analysis time period? I again suggest to focus the discussion more on seasonal monsoon convection, and existing results on Aeolus capturing equatorial waves (e.g. the previously mentioned Ern and Zagar papers), and the QBO (Banyard).

Also, spending almost an entire paragraph on QBO-MJO modulation is going off-topic.

We thank the reviewer for this very relevant feedback. We agree that our original paragraph on QBO-MJO modulation was too speculative and distracted from the main arguments of the paper. Following this advice, we have completely removed the original paragraph. In its place, we have written a new, more focused paragraph that discusses the known limitations of ERA5 in the context of the QBO, directly incorporating the highly relevant recent literature you suggested:

(lines 637-649)

These challenges are particularly evident in the representation of key tropical phenomena like the Quasi-Biennial Oscillation (QBO), which is driven by the upward propagation and dissipation of a spectrum of atmospheric waves. Recent studies using direct Aeolus wind observations have provided new insights into how reanalyses represent these processes. For instance, Banyard et al. (2023) found that during the 2019/2020 QBO disruption, a period covered by our study, the onset of the disruptive easterly jet was observed by Aeolus five days earlier than in ERA5. This discrepancy was linked to higher Kelvin wave variances and sharper vertical wind shear in the Aeolus data, suggesting that ERA5 may misrepresent the breaking of smaller-scale waves that are crucial for forcing the QBO. Similarly, Ern et al. (2023) confirmed that while the zonal-mean QBO is well-represented in ERA5, local biases exist, particularly in shear zones. From a data assimilation perspective, Žagar et al. (2025) showed that assimilating Aeolus winds produced the largest changes to the analyzed state in the UTLS precisely during the 2019/2020 QBO disruption, highlighting the importance of direct wind observations for reducing uncertainties in these critical shear zones. Together, these findings, derived from the same novel wind dataset used here, support our conclusion that reanalyses can have significant deficiencies in representing the full spectrum of wave activity and its associated kinetic energy in the absence of direct wind assimilation.

- l.632-633: No works that are more recent? It's surprising to me, that the newest reference here is 10y old and that there were no follow-ups (although I'm not an expert in this particular field)

Works from 2021, 2022 and a very recent 2025 paper were added to actualize both sides of the argument with newer studies:

(lines 654-671)

At first glance, using a fixed ratio appears straightforward for converting well-documented E_p (from temperature-based instruments such as GNSS-RO) to E_k . Traditionally, linear GW theory proposes a near-constant ratio of E_k to E_p , often quoted between 5/3 and 2.0 (VanZandt, 1985; Hei et al., 2008). In idealized models of linear wave behavior, the kinetic and potential energies are expected to be comparable, leading to a ratio close to unity. This theoretical relationship has been confirmed observationally. In stable, linear wave

conditions, the energy ratios adhere closely to predictions (Nastrom et al., 2000), a finding supported by a modern case study of individual, freely-propagating waves (Huang et al., 2021).

However, a growing body of evidence challenges this simplification: Empirical work increasingly reveals significant variability in this ratio, indicating non-linear effects in real-world atmospheric conditions (Wing et al., 2025; Baumgarten et al., 2015; Guharay et al., 2010; Tsuda et al., 2004). When the observed energy ratios deviate significantly from this expected range, non-linear processes may be at play. While a large climatological study may find a mean E_k/E_p ratio close to theoretical values (e.g., 1.5 in Zhang et al., 2022), this average can mask significant event-to-event variability. For instance, in situations where wave amplitudes are particularly large, wave-wave interactions, such as those resulting from wave breaking or saturation, could lead to the observed discrepancies. This has been demonstrated in earlier work by Mack and Jay. (1967), who found that under certain conditions, potential energy deviated markedly from kinetic energy, suggesting non-linear effects. Similar findings have been reported by Fritts et al. (2009), who showed that interactions between gravity waves and fine atmospheric structures can result in turbulence, thereby affecting the balance between kinetic and potential energy. A recent study also confirmed that the ratio is not static and can be actively modulated by the background atmospheric state, such as strong wind shear (Wing et al., 2025).

- l.645: "increasingly challenged by observations" --> references please, or say "(see references above / previous paragraph)"

Added "(see references in the previous paragraph)".

- l.652-653: refer to the section where you come up with these numbers

Added "(as detailed in Sect. 2.2 and shown in Appendix C)".

- l.657-665: regarding the hot pixels, wasn't this taken care of in the latest reprocessing that you use?

Even if you state later that the latest baseline improves this, the current phrasing implies it's still a major source of uncertainties -- which is not really the case right?

--> I overall don't like the fit of this sentence for the study's research focus, I suggest to remove even the whole paragraph -- it's mixing many things in a very vague way, without any way forward.

--> Oscillating perturbations misinterpreted as GW signals (Ratysnki et al 2023) can be mentioned in the Data/Methods section to give more nuance on the reliability of the dataset.

The paragraph was removed, there is already a sentence talking about OPs in the original data section.

- l.676: include lower bound of the wavelengths that are band-pass filtered. Also, band-pass is the more common expression (instead of passband)

Done.

- l.687-693: please also mention Aeolus' swath size vs whole globe -- small scales are only sampled very locally every day --> how many equator crossings every 12h, I believe it's in the range of 14-16?

Thank you for the suggestion. We have now added a discussion of Aeolus' narrow observational geometry and its implications for sampling small-scale gravity waves. The revised paragraph explicitly states the ~3 km effective swath width, the ~86 km along-track averaging, and the ~16 orbits per day (~15–16 equator crossings every 12 h). Here's the revised paragraph:

(Lines 710-721)

Looking forward, a critical application for such observations is the constraint of gravity wave momentum fluxes, which are essential for global circulation models. However, deriving momentum flux estimates directly from single-component wind measurements like those from Aeolus presents two co-dependent problems. First, the vertical flux of horizontal momentum (e.g., $\langle u'w' \rangle$) requires simultaneous knowledge of horizontal (u') and vertical (w') wind perturbations. Aeolus supplies only the line-of-sight projection of the horizontal wind and, crucially, no direct information on the vertical wind. In the standard processing w' is simply assumed negligible (Krisch et al., 2022), leaving the key term in the flux equation unconstrained. Second, the satellite's sampling geometry further limits what can be inferred. Aeolus observes with a ~3 km-wide "pencil beam" that is horizontally averaged to about 86 km along track, and its sun-synchronous orbit completes ~16 revolutions per day (roughly 32 equator crossings, or 15–16 every 12 h). Small-scale gravity waves are therefore captured only where the narrow ground tracks happen to intersect them, leaving large spatial and temporal gaps. Together, the absence of direct w' measurements and this sparse, one-dimensional sampling mean that Aeolus winds alone cannot yield global momentum-flux maps without substantial modelling support or complementary observations.

References

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Ern et al (2023), The quasi-biennial oscillation (QBO) and global-scale tropical waves in Aeolus wind observations, radiosonde data, and reanalyses , <https://doi.org/10.5194/acp-23-9549-2023>

Lux et al. (2022), Quality control and error assessment of the Aeolus L2B wind results from the Joint Aeolus Tropical Atlantic Campaign, <https://doi.org/10.5194/amt-15-6467-2022>

Zagar et al. (2025), ESA's Aeolus mission reveals uncertainties in tropical wind and wave-driven circulations, <http://dx.doi.org/10.1029/2025GL114832>