Response to Reviewers

Overview

We thank the reviewers for their helpful comments on our revised manuscript. Both reviewers raise useful points and queries, all of which are addressed in detail individually below. Here, we provide a brief summary of the additional changes made, before we then respond to each reviewer's specific comments.

The key improvements to the paper include, but are not limited to:

- Added emphasis on the role of enhanced lower stratospheric westerlies in aiding the development of the QBO disruption.
- Clarification about the method for obtaining fig. 8 and the middle panels of fig. 9 and 10.
- An additional figure in the supplementary material to justify the removal of instrument noise for figures 9a, b, d and e.

We now respond to the specific comments by each reviewer below.

Report #1 (Reviewer #3)

The content of the paper has been significantly expanded since the initial submission, and the study will be a valuable contribution to ACP. However, I have identified areas where additional clarification is needed, particularly regarding some of the new analyses and corresponding figures introduced in the revised manuscript. Furthermore, certain statements in the text could benefit from more substantial supporting analysis.

We are grateful for the reviewer's support of our study as a valuable contribution to ACP, and are hopeful that the below clarifications and minor additions in response to their comments are sufficient for final publication.

Major comments

1) Currently, it is not entirely clear how Figure 8 and the middle panels of Figures 9 and 10 are obtained. There are also inconsistencies between the text and figures (for instance, it is written that Fig. 8 depicts normalized perturbations but the colorbar indicates units of m/s). See specific comments for further details.

We have modified the manuscript to clarify how the perturbations for figure 8 and the middle panels of figures 9 and 10 are obtained, and we apologise for the confusion about this. We have responded to this issue in more detail after the specific comment mentioned by the reviewer.

2) The authors state in their conclusion that 'the larger values observed by Aeolus suggest that ERA5 does not capture as much breaking and dissipation of Kelvin waves with shorter vertical wavelengths, especially within the TTL. This leads to less westerly momentum being deposited between 18 and 21 km in ERA5, just above the region of greatest Kelvin wave variance as measured by Aeolus.' but this last point is not really supported by the current analysis, which does not include estimates of drag. Would it be possible to estimate momentum deposition by Kelvin waves, e.g. by adapting the approach employed by Alexander and Ortland (2010) to Aeolus observations ? This would strengthen the discussion.

We acknowledge the reviewer's suggestion of including estimates of drag, and we agree that it would be useful to conduct such an analysis to confirm our hypothesis. As quoted by the reviewer, the strong suggestion made by our results is that ERA5 is depositing less westerly momentum at these altitudes because it does not accurately capture the breaking and dissipation of Kelvin waves with shorter vertical wavelengths, especially in the TTL. Given that the purpose of our study was to characterise the QBO disruption using Aeolus, and that a study adapting the approach employed by Alexander and Ortland [2010] would be best done with a strong modelling component, this additional analysis is likely to be beyond the scope of this current study. We do however think this is an interesting piece of future work, and further modelling studies may be beneficial to investigate this drag more directly. We have also added a sentence to the discussion to reflect the reviewer's comment, as it certainly strengthens the manuscript's applications to future work.

3) Figure 5 shows that the delay and underestimation of the QBO disruption in ERA5 compared with Aeolus is associated with weaker Westerlies around 18-19 km in the reanalysis. This is an important result, as recent studies (in particular, Kang et al., 2022), attribute a significant role to enhanced lower stratospheric Westerlies in the development of the disruption. I am under the impression that this point could be emphasized more.

We agree with the reviewer that the delay in the QBO disruption's onset in ERA5 compared with Aeolus is an important result, particularly in light of other recent studies. To emphasise this point more as suggested, we have added: 1 sentence to the abstract; 1 sentence to the results section in the part where we describe figure 5; 2 sentences to the discussion.

Other comments

Line 130: Consider referencing Bley et al. (2022) here, as this paper provides an estimate of Aeolus accuracy in the UTLS by comparing it with Loon long-duration balloon measurements.

We have added a sentence to reference Bley et al. (2022) as suggested by the reviewer.

Lines 156-157: It would be worth clarifying whether you apply a smoothing to the radiosonde data for the point-by-point comparison (or not).

We have applied linear interpolation to fill gaps in the radiosonde data but we have not applied any additional smoothing. We have now clarified this in the text as suggested.

Line 161: Similar question for ERA5. How do you address the discrepancies in vertical resolutions between the data sets?

This is addressed by our creation of the 'synthetic ERA5 HLOS wind' data set, which is geometrically identical to the Aeolus data set, but instead simulates Aeolus orbiting through an ERA5-like atmosphere rather than the real one. The result is that the discrepancy in vertical resolution between Aeolus and ERA5 data is eliminated by our method.

Line 232: When mentioning "fine vertical resolution", it might be helpful to reiterate the ∼1 km resolution for clarity.

We have added "(∼1 km)" to the text here.

Figure 3: Why are there several data points at the same altitude for Aeolus?

There are sometimes several data points at the same altitude for Aeolus because multiple profiles often fall within the 150 km colocation radius, centred on the Singapore radiosonde launch site. The solid lines demarcate the average (mean) wind speed within this 150 km radius circle.

Line 315-320 (also regarding the caption of Figure 4): I am wondering about the number of significant digits you use for the differences. I noticed a change in the values between the initial and current version of the manuscript, which (I imagine) comes from switching from 250 to 150 km collocation radius. This suggests to me that 2 significant digits is 1 too many.

Although the shift in collocation radius from 250 to 150 km produces a slight change in values between the manuscript versions, we do not believe the difference is sufficient to justify limiting the quoted values to 1 significant digit. The mean change is only 0.05 ms^{-1}, ranging from 0.02 to 0.07 ms^{-1}, and it is likely that the standard error on each value (which is the best way to determine how a result should be quoted, as described in Hughes and Hase [2010]), is less than 0.1 ms^{-1}. Given that we have reduced the collocation area by 64%, eliminating a rather large area both south of the equator and north of 2.5°N (latitude naturally being more important than longitude here), that the changes are this small in magnitude gives us confidence in our results. This is in spite of the relatively low reduction in the number of Aeolus profiles used. We also want to maintain consistency in the way we quote the different values here, as well as with other literature.

Figure 5 : Very interesting!

Yes, the changes made to this figure for the revised manuscript reveals very interesting differences between Aeolus and ERA5!

Line 377-378: You could assess a potential regional bias, e.g. by showing altitude-longitude section of the average HLOS wind difference between ERA5 and Aeolus during the disruption (e.g. Jan-March). This is not done in Ern et al. (2023), who restrict themselves to radiosonde locations.

Whilst we agree with the reviewer that potential regional biases during the disruption could play an important role in modulating both the time-lag of its onset in ERA5 compared with Aeolus, as well as the disruption's poor predictability in NWP, we have decided to leave this for a future study. Major comment 2 suggested a deeper analysis of the momentum deposition by Kelvin waves, and so we think a study which combines this more general view of differences in Kelvin wave propagation between models and Aeolus, with a closer analysis of the regional variation in these differences, could be a useful piece of future work. It is possible, for instance, that shorter wavelength Kelvin waves are captured less well by existing observations over the Pacific Ocean, where their propagation is also affected by the Pacific Walker Circulation. There are many other interesting questions of this nature which could be explored, but we feel that the addition of this new material to our particular study here could be detrimental to the overall focus on the 2019/2020 QBO disruption. Further suggestions of this nature are however very much welcome.

Line 460: What do you mean by 'Kelvin wave-filtered'? Is it the same as the time filter introduced around line 390. If yes, you might consider replacing by 'time-filtered'. 'Kelvin wave-filtered' suggests to me that both time and longitude filters are applied together to select only positive phase speeds.

Yes, this is the same time filter introduced at line 390. We agree that this could cause a misunderstanding and have replaced 'Kelvin wave-filtered' with 'time-filtered' for the reason the reviewer has given.

Figure 8: See major comment 1. I am not sure what is shown in panel a). What is this median composite ? From both Fig. 6 and Fig. 7, I would expect much smaller median values over the period (given that we consider maximum instantaneous values around 15 m/s of a field to which a 5-25 day filter has been applied). Also, line 461-462, you write that you are "normalizing by the median RMS of the entire domain". Then the plotted field should dimensionless, but the figure indicates m/s for the unit. This needs to be clarified.

Figure 8a shows two plots on a longitude-altitude cross-section along the equator. (1) In colour, at each longitude and altitude, are the median time-filtered zonal wind perturbations, after scaling (multiplying) by the ratio of (A) the median of the root-mean-square (RMS) time-filtered zonal wind perturbations to (B) the RMS of the median time-filtered zonal wind perturbations (scale-factor = A/B). This is shown to provide an accurate composite representation of the typical true wind speeds and vertical wind shear associated with a Kelvin wave in the real atmosphere during this time period. (2) In contours, is the vertical wind shear which corresponds to plot (1). The reviewer is correct that the median values alone are much smaller (roughly a factor of 10), so this scaling (previously called normalisation incorrectly, as although it brings the data to a "normal" reference point, it does not cause the data to become dimensionless and between 0 and 1) is required to provide true values of the vertical wind shear. To clarify, the median is taken over the time dimension whereas the mean for the RMS is always taken over the domain, across altitude and longitude. Changes have been made to the manuscript to clarify what is shown in these figures.

Line 537: 'not shown': it would be useful to include this figure in the supplement.

Yes, we agree. We have added this figure to the supplementary material to justify the removal of instrument noise for figures 9a, b, d and e.

Line 540 and Figure 9: Do I understand correctly that you are subtracting 0.013m/s in panel a and 0.016 m/s in panel d at each omega and k? This seems unnecessary to me and makes the method description a bit hard to follow. I would recommend showing the 'biased' spectra, starting the colorbar at 0 and revert the colormap (i.e., have white as its first color). With the current colormap, one reads that most of the spectrum saturates at the maximum value (since areas with amplitudes below the threshold are kept white).

Yes, we believe it is important to remove this noise in the observation data set so that the important features of the spectra can be seen and compared between Aeolus and ERA5. We have experimented with some of the suggested changes to the colormap and colorbar range and found that the current figure remains the easiest to interpret, so we have left it as before with most of the spectrum set to be transparent (as opposed to white).

Line 547-549: There seems to be an inconsistency between text and figure: the text specifies that spectra are scaled but, in the title of the colorbar, there is a multiplication symbol (instead of a division). The differences are on the order of a few percent, is that really significant ? What significance test are you using ? If it is a student t-test, an estimate of the standard deviation of the amplitude at each wavenumber/frequency is required, how is it obtained?

We have checked and confirmed that both the text and the figure are correct, but we have made some changes to the text to reduce the risk of any misunderstanding. We are indeed scaling (multiplying) by the mean amplitude of the two data sets at each k and ω, and this is because we are most interested in the differences between Aeolus and ERA5 in the spectral regions which correspond to different types of equatorial wave. We have clarified that we are conducting a Student's t-test, and stippling on the figure shows where p $<$ 0.001, limited to regions where SNR $>$ 1 and scaled differences are greater than 0.1 $\times$ 10^{-3}. As the reviewer says, an estimate of the standard deviation of the amplitude at each wavenumber/frequency is required, and this is obtained from the ($\pm k$, $\pm 0.05\omega$) window of surrounding points (typically 54 values in total), as described in the caption for figure 9. We have used a fairly stringent significance test here, so in the few places where stippling is observed, we have confidence that the null hypothesis can be rejected, with the exception of the regions exhibiting low wave amplitudes and hence lower percent differences as the reviewer suggests (especially in the range $-2 \leq k \leq 2$). We had already indicated this in the text, but have added further clarification.

Line 562: Why not show the spectra up to the altitude range of the disruption?

We found that the data sampling at 20 km and higher was not sufficient for a reliable spectral analysis using the method of Salby [1982] during this time period. A more complex data interpolation routine implemented over a longer time period may yield reliable spectra at these heights, however this is beyond the scope of this study.

Line 594: I imagine that the linear regression is weighted by the amplitudes of the Kelvin waves. This information should be included.

Yes, that is correct, we have added this information to the figure caption.

Line 613: 'vertical wind information' should be replaced with 'horizontal wind vertical profile information'.

Yes, thank you for spotting this error. We have now corrected this as suggested.

Line 636: 'overlying', don't you mean 'underlying'?

We were originally referring correctly to the overlying westerly winds here, however we have now edited the text to replace this with the equatorial waves beneath the easterly jet, since Aeolus doesn't really capture the overlying westerlies very well until the range-bin settings increase in altitude in June 2020. This minor adjustment should now avoid any confusion for the reader.

Line 642-643: I am missing part of the reasoning here. Do you want to imply that those propagating Kelvin waves partly dissipate and limit the magnitude of the Easterlies?

Yes, the Kelvin waves appear to be propagating to and dissipating at a higher altitude in the reanalysis, which causes the easterly jet to be limited in magnitude. We have added a clause to the text to clarify this.

Typos and language:

- Line 57: Replace "complimentary" with "complementary."
  Typo fixed.
- Line 102: Replace "each with thicknesses" with "each with a width" (singular).
  Here, we have replaced "each with thicknesses" with "each with a thickness" to avoid confusion with the lateral width of either the range-bin or laser column.
- Lines 182-183: Adjust "wave, mean-flow interaction" to "wave-mean flow interaction."
  Adjusted as suggested.
- Line 261: Use "Figure 2" (following ACP guidelines, since it is the subject of the sentence).
  Fixed.
- Line 261: Replace "nudges" with "constrains"
  Replaced as suggested.
- Line 515: Change "in the same way as" to "with the same implementation as."
  Changed as suggested.
- Line 627: Replace "showed" with "shown."
  Typo fixed.
- Line 630: This sentence does not contain a finite verb.
  We have replaced "A result that is in agreement with Bley et al. (2022)." with "This result is in agreement with Bley et al. (2022)." to fix this sentence.

Report #2 (Reviewer #2)

The authors significantly improved the manuscript by extending it by a more in-depth analysis of the impact of equatorial waves on the QBO. I have only two small remarks / questions to this version of the manuscript. After these are clarified I recommend the publication in ACP.

We thank the reviewer for their comments and for their recommendation of publication following minor revisions.

Line 487 (of manuscript with tracked changes): Do you observe Doppler shifting in your data or is this sentence more on the general theory of what you would expect to happen?

We have added a clause to make it clear that this sentence is more on the general theory of what we would expect to happen, as the reviewer suggests. Our conclusions assume that any Doppler shifting does not greatly affect the results, however studies such as Yang et al. [2012] did find that their wind power spectra fitted the theoretical dispersion curves better when taking this effect into account. They used ERA-Interim and ERA-40 winds for their analysis, and we would be quite interested to know if a similar analysis, conducted on either model data or a future reanalysis which assimilates Aeolus winds, could provide some interesting new results relating to this.

Lines 609ff (of manuscript with tracked changes): ERA5 has at this altitude range a slightly *higher vertical resolution. The slightly higher vertical wavelengths observed by Aeolus might also be caused by aliasing to longer wavelengths due to the slightly coarser resolution. 6 km vertical wavelength is really at the edge of observability of Aeolus with a vertical sampling of 2 km.*

The reviewer makes an important point about the aliasing of shorter vertical wavelength Kelvin waves in Aeolus data. We are obtaining the vertical wavelength from our measurements of the equivalent depth at each height (3 km-deep bins), which is derived using the Salby [1982] method and therefore analyses only the temporal and longitudinal variations in wind. Using this method addresses some of the aliasing that might occur in Aeolus data for a single profile, or even a single day. It is true that we are near the edge of observability for Aeolus, however most of the ERA5 winds at these altitudes are not constrained well by observations, so much of this finer structure is likely simulated by the reanalysis model, rather than coming from the assimilated observations. Our study therefore cautiously interprets these results, and emphasises consistency with earlier findings while refraining from overemphasising their magnitude due to Aeolus' various observational limitations.

References

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M. L. Salby. Sampling theory for asynoptic satellite observations. part i: Space-time spectra, resolution, and aliasing. *Journal of the Atmospheric Sciences*, 39(11):2577–2600, 1982. doi: 10.1175/1520-0469(1982)039<2577:STFASO>2.0.CO;2.

G-Y. Yang, B. Hoskins, and L. Gray. The influence of the qbo on the propagation of equatorial waves into the stratosphere. *Journal of the atmospheric sciences*, 69(10):2959–2982, 2012. doi: 10.1175/JAS-D-11-0342.1.