EGUsphere, referee comment RC2
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Comment on egusphere-2022-1150

Anonymous Referee #2

Referee comment on "Investigation of links between dynamical scenarios and particularly high impact of Aeolus on numerical weather prediction (NWP) forecasts" by Anne Martin et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1150-RC2, 2022

This is a very nice study which aims to link episodes of enhanced forecast impact from Aeolus to particular dynamical features or changes in circulation pattern. This complements standard overall forecast impact evaluations of Aeolus which report on average impact. As for any feature-based forecast impact evaluations conducted over relatively short periods (3 months in this case), I have some reservations about the robustness of the findings given the relatively low sample size for the cases evaluated. However, the authors present their results in the context of clear positive overall impact and present them primarily as illustrative examples of potential mechanisms, rather than firmly established links. The authors are also sufficiently frank about the sampling limitations of their study and state that longer experimentation would be needed to firm up conclusions. I think the paper can therefore be published after addressing the comments below in a minor revision.

Specific comments:

- Title: As some of the causal relationships remain speculative, I feel a more “neutral” title may be more appropriate. Something along the lines of “Investigation of links between dynamical scenarios and particularly high impact of Aeolus on NWP forecasts”
- Section 2.1: I had expected to read a sentence or two about the biases that have been observed in Aeolus data in this section. They (or at least their corrections) are mentioned later (around line 125), but I feel some mention here would be worthwhile. They are relevant in the context of the bias impact presented later.
- L87-88 (“This combination … than pure 3D-Var could (?).”: I don’t quite understand what is meant with this sentence, in particular what is meant by a “stable analysis” and "incorporating more information from the observations”. I would think the main aspect is that the flow-dependent background error covariance will allow a better weighting of observations and the background.
- Related to the above point: Here of elsewhere the authors might want to address to what extent their results are affected by the use of a 3D-Var-based assimilation

system, rather than, for instance, a 4D-Var system or equivalent. I would expect that a 4D-Var system would be more able to derive wind information from satellite radiances (through the tracing effect). I would expect this to affect to what extent the system without Aeolus data is able to correct some of the error features described in section 4.

- Section 2.2: The reader is referred to Martin et al (2022) for some important details about the experiment configuration. Unfortunately, the Martin et al (2022) reference is incomplete in the reference list, and I was unable to look up these details. I suggest including some more details in the present paper (in particular, a brief outline of what other observations are included in the CTRL and what values are assigned for observation errors for Aeolus).
- L126: A reference regarding the M1-mirror bias correction method should be added.
- Section 2.2.2: A reference for ERA5 should be added. It would also be useful to discuss the quality of ERA5, particularly for the tropical stratospheric wind field, since it is used as verification reference. Given the lack of direct wind observations in this area, might ERA5 data be prone to biases in this regions?
- L156 ("… 120 h are reduced by 2 up to almost 5% on average…"): The "2" seems grammatically out of place. At the same time, 2% appears a more typical number to quote here, given the values in the table.
- Fig. 2 and accompanying discussion: It appears that the improvement in RMSE for the 2nd half of the period shown is primarily due to a reduction in the mean error. This could be spelled out more clearly in the text. I would be curious to know whether there is also a reduction in the standard deviation of the forecast error between the two experiments. Also, since the Aeolus data have been bias-corrected towards the background (L127), I wonder how the change in the mean wind comes about. Do Aeolus departures indicate this bias in the background? Are wind biases in ERA5 sufficiently small for the tropical stratosphere to be certain that this mean change is indeed an improvement?
- Fig. 3: It would be helpful for the reader to mark the typical location of the 30-50 hPa layer in the plot of the range-bin-settings. I suspect it will be largely in the range of 20-26 km, and this information should help the associated discussion. Do the authors think that the primary mechanism for the improvement shown for 30-50 hPa for the "with QBO range bin settings" version is that this setting covers the layer in question, rather than, say, vertical resolution considerations?
- L224 ("… to get a more reliable statement"): I suggest rephrasing to "… to be able to draw firm conclusions".
- L243 ("The largest error reduction coincides with the strongest negative slope in the SST anomaly."): This is true, but similarly large error reductions are also found during the period 15-19 September when the SST anomaly is flat. I am not fully convinced that there is a connection, based on this single case.
- L339/340 ("Selz et al (2022) found … the error growth."): I struggle to follow this sentence.