Replies to Reviewer #3

January 2021

We would like to thank the Reviewer for their positive feedback on our revisions and for their further suggestions for improvement. We provide a detailed reply (in red) to the individual comments (in *italics*) below.

General comments

1. *L 97: The authors focus "on the SH because it is closer to zonal symmetry than the northern". Suggest that the authors firefly comment on this either in that current section or in the discussion section, on how they would expect (based on their expert knowledge) the study/results would differ if the NH would be used.*

Past studies have highlighted that the wintertime North Atlantic jet bears the characteristics of an eddy-driven jet, the Pacific jet those of a merged jet, and the African jet those of a subtropical jet (e.g., Eichelberger and Hartmann, 2007; Li and Wettstein, 2012). Based on intuition, for the North Atlantic sector in winter (DJF), we would expect to obtain results similar to those seen in the southern hemisphere: for most of the time we have two separate jets coexisting (high d and theta). Occasionally, when the jet is displaced southwards like in 2009/2010 (e.g. Madonna *et al.* , 2019; Harnik *et al.* , 2014), the two jets merge (low d and theta). For the NH Pacific, we would expect for most of the time a single merged jet, so low d and theta. At the same time, land–sea contrast and orography influence the structure and tilt of the jet stream (Brayshaw *et al.* 2009), creating zonal asymmetries that are most pronounced in the North Atlantic sector and have no comparable analogues in the SH. The results hypothesised above may be affected by this, although it is not straight-forward to infer exactly how without conducting a full analysis. We have now added a summary of the above to the discussion in Sect. 5 of the main text.

2. *L 102-103: Can the authors briefly comment why this parameter combination causes 'unrealistically regular oscillations' and the other combinations not? The readers might ask; Does leaving this combination out change anything on the overall 'validity' of the approach.*

The simulation the Reviewer is referring to was dominated by a wavenumber 6 disturbance, while other simulations in the merged jet regime were dominated by wavenumber 5. The oscillations that appear in this simulation likely arise from a wave-mean flow interaction mode, which is much weaker in the other merged jet simulations. If added to the analysis, the wavenumber-6 simulation would likely appear as an outlier. We do not believe that such simulation reflects the dominant dynamics observed in the real

atmosphere, and thus argue that its exclusion from the analysis is well-motivated. At the same time, we agree that this was not explained in the previous version of the study. We have now rephrased the relevant passage to: "We removed the simulation with H = 8 km and r = 2 from the analysis, because it was dominated by unrealistically regular oscillations of the eddy amplitude. These oscillations likely arise from an internal mode of wave-mean flow interaction, which is much weaker in the other simulations of the parameter sweep."

3. *L 260: suggest swapping the order from 'JJA months' to 'months JJA'.*

To avoid any misunderstanding, we have now rephrased this to "austral winter (JJA)".

4. *L 299: please specify what 'ongoing work' refers to. Current work by the authors/scientific community in general?*

We agree that this was a somewhat imprecise reference. We have now clarified that we are discussing ongoing work by some of the authors of this study. Unfortunately, these results have not yet been structured into a paper, so we are unable to provide a supporting reference.