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Comment on wcd-2021-4

Anonymous Referee #1

Referee comment on "Acceleration of tropical cyclones as a proxy for extratropical interactions: synoptic-scale patterns and long-term trends" by Anantha Aiyyer and Terrell Wade, Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-4-RC1>, 2021

Review of the manuscript: "Acceleration of Tropical Cyclones As a Proxy For Extratropical Interactions: Synoptic-Scale Patterns and Long-Term Trends", by A. Aiyyer and T. Wade

Summary and overall recommendation:

The proposed paper has two main aims: first, characterizing the large-scale flow patterns behind rapid acceleration and deceleration of tropical cyclones outside the deep Tropics; second, revisiting the issue of the variability and trends in tropical cyclone (TC) motion from the perspective of acceleration and deceleration. The paper is written clearly and the exposition of the results is generally easy to follow. There are, however, a few conceptual misunderstandings in the interpretation of the results, in particular about the role of phase-locking and blocking during TC deceleration/acceleration and, more in general, during extratropical transition (ET). Other important issues concern the choice of subsets for the composite analysis and the significance of the trend analysis. Some careful revisions to make the scope and the results of the paper more precise are due before this research paper can be granted publication in Weather and Climate Dynamics.

Main comments:

- The advantages of employing TC acceleration as a metric are not immediately obvious: the authors could consider elaborating on this aspect to make the motivation of this work stand out more clearly and, more in general, to contextualize its relevance for our understanding of ET and of its impacts. From the title, for instance, acceleration is meant to be used as a "proxy" for interactions between TCs and the extratropical circulation, but what is actually gained with respect to a similar analysis performed on, e.g., TC translation speed, by comparing subsets of rapidly moving and stagnating TCs

on the 30°N-40°N latitude band? The same critique could be extended to the second part: what is the benefit of using the acceleration framework with respect to the simple translation speed? Wouldn't it be just a more convoluted way to re-obtain the results of Kossin (2018) and Lanzante (2019), while being still heavily affected by the limited length of the data record?

- The authors should carefully consider their use of the concept of phase-locking, which appears to be different from the one established in the literature by Hoskins et al. (1985) and later employed, among others, by Riboldi et al. (2019) [R19]. In that context, phase-locking (or "phasing") represented the optimal flow configuration for enhanced and sustained baroclinic growth of an extratropical cyclone and the correspondent amplification of a downstream Rossby wave packet. It occurs when an upper-level positive (potential) vorticity anomaly (i.e., a trough) is located a quarter of wavelength upstream with respect to a low-level positive temperature anomaly, leading to 1) sustained tropospheric ascent (by vorticity and temperature advection) in the region of the cyclone and 2) mutual intensification of the two anomalies via the anomalous flow field induced by the anomalies themselves. Phase-locking is inherently three-dimensional, as it consists of an interplay between features at upper- and lower-levels. There are, however, a few points in the manuscript where a more "two-dimensional" concept of phase-locking is considered and this makes the comparison with previous literature problematic. For instance, lines 229-230, "The phasing of the tropical cyclone and the extratropical wavepacket as led to the formation of a cyclone-anticyclone vortex dipole" (that are, however, one to the north of the other); lines 265-267, "we have viewed this as a phase-lock between the ridge and the tropical cyclone" (this is confusing and conceptually incorrect, as phase-locking occurs at best between an upper-level ridge and a low-level anticyclone during anticyclogenesis), or lines 417-418. Also at lines 426-430 the mechanism that holds dipole blocking stationary is described, rather than the phase-locking dynamics during ET.
- More in general, it seems to me that the outlined results relate only marginally to R19, despite it being probably the closest analogue in the literature. For instance, the DECEL subset by R19 features enhanced downstream flow amplification and atmospheric blocking activity, following a classic ET pathway of rapid TC poleward motion ahead of a stagnating upper-level trough. That subset would then correspond to the rapidly accelerating TCs of the current manuscript; however, the discussion of the results relates R19's DECEL subset with the subset of rapidly decelerating TCs (lines 225-231, 419-424, 436-439). It is also not always clear whether the analogy is drawn with Fig. 10a or Fig. 10b in R19. Another questionable point is the parallel drawn with Fig. 10 of R19, as no vortex dipole was observed or discussed by R19 (lines 260-265, 419-424; see also the next comment).
- For rapidly decelerating TCs, the authors often say that the TC and the anticyclone become "phased" in a configuration of atmospheric blocking (a dipole block; e.g., lines 409-418). This occurrence does not seem realistic, as the horizontal scale and the dynamical characteristics of the large-scale blocking anticyclone are completely different from the ones of the tropical cyclone, and the latter is "enslaved" to follow the large-scale flow induced by the former. Related to the previous main point 3), R19 described how blocking occurs at the end of the Pacific storm track a few days after ET completion (see their Fig. 3) and did not mention blocking occurring at the same longitude of the transitioning TC, or with the transitioning TC being part of it. A direct impact of the TCs "injecting" low-PV air in the block could occur and therefore inflate the ridge (as speculated in lines 216-217), but this needs to be proved. A simpler interpretation of the composites in Fig. 5 or 7 would involve a pre-existent slow-moving anticyclone, maybe an atmospheric block, that decelerates the north-eastward progression of the TC because of 1) the presence of easterlies on the southern side of the block and 2) (more speculatively) the presence of an "inverse beta-effect", due to that large-scale anticyclone locally reversing the planetary vorticity gradient and effectively "pushing" the TC away from the anticyclone (cf. the selective absorption mechanism described by Yamazaki and Itoh 2009). The authors should check, using an

appropriate diagnostic, whether the presence of blocking follows or precedes TC deceleration and therefore modify their interpretation of the results and the discussion section (e.g., lines 423-430) of the manuscript.

- There is evidence of a poleward trend in jet position and extratropical wave activity due to global warming, but there is also evidence of a poleward trend in TC genesis and track that can compensate this, as the region favorable to sustain TCs expands northward. This is one reason why it is speculated that ET storms reaching Europe will increase in frequency as global warming progresses (e.g., Haarsma, R. 2021, <https://doi.org/10.1029/2020GL091483> and references therein). How would the authors comment on that?

Specific comments:

Line 24: even though it may seem reasonable to suppose it, are there references discussing the impact of wind shear on tropical cyclone motion? A possible one might be Jones et al. (2000, <https://doi.org/10.1002/qj.49712657008>).

Line 36: isn't also Bieli et al. (2019) a more recent reference to justify this high percentage of transitioning TCs in the North Atlantic?

Lines 37-39: this is the only place in the literature review where TC acceleration is cited, it would also be a good place to motivate why it is worth investigating it (see also main comment 1).

Lines 43-44: the work by R19 is definitely relevant, but another relevant reference not included in the literature review might be the recent work by Brannan and Chagnon (2020, <https://doi.org/10.1175/MWR-D-19-0216.1>) who also tried to investigate phasing during ET and focused on the North Atlantic.

Line 163: composites are built from TCs between 1980 and 2016 (line 144), but here is said that anomalies are calculated with respect to the 1980-2015 seasonal cycle, as for bootstrapping (line 172). Earlier (line 97) it was said that the considered period for composites would have been 1981-2016. Why these differences?

Lines 166-170: storm-relative ensembles are built using data between 1980 and 2016 (line 144) and are based on data drawn from 196 and 168 unique storms (line 169). From a rapid check on Wikipedia (https://en.wikipedia.org/wiki/Atlantic_hurricane_season), "only" 555 tropical systems occurred over the Atlantic. This means that $196+168=364$ tropical systems (65.6%, almost 2 out of 3) would then be either rapidly decelerating or rapidly accelerating storms. Aren't these numbers very high? This percentage seems far from the top and bottom 10% that should be selected to build the composites. Does this total need to be split between curvature and tangential acceleration? Please explain.

Lines 171-174: As the same TC can sit in the 30-40°N latitudinal band for several, consecutive 6-hourly time steps (lines 148-149), the composites are likely built by averaging together consecutive time steps with very similar large-scale flow configuration. For instance, 196 (168) accelerating (decelerating) TCs correspond to 352 time steps in each subset, so each TCs contributes to the composites with 1.8 (2.1) consecutive time steps (~12 hours). This effect of serial correlation needs to be accounted for during bootstrapping, otherwise this might lead to a significance test that is too "easy" to pass. Instead of drawing 352 random time steps, an appropriate combination of dates should be selected so that a substantial fraction consists of couplets of consecutive 6-hourly time steps.

Line 172: are TCs in the period of study selected only between July and October? If this is not the case, then why is the bootstrapping performed only by sampling dates in these months? A more appropriate sampling should take into account the climatological distributions of composite elements by selecting a random date in a time interval centered around the time of each rapid acceleration/deceleration and by attributing it to a random year, as done, e.g., in R19.

Lines 211-217: it would be great if the points discussed in this paragraph were also backed up by some more quantitative analysis. Besides showing in the composites the strengthening jet streak (e.g., with zonal wind anomalies), no metric of downstream impact is employed in the study and it is not clear whether the two subsets have significantly different downstream impact on the flow evolution. The meridional flow index (Archambault et al. 2013), the eddy kinetic energy framework (Quinting and Jones 2016) or Rossby wave packet amplitude (R19) can be possible choices.

Lines 232-253: the composites for strong curvature deceleration are very similar to the ones of tangential acceleration. How would the results change by considering positive and negative values of curvature acceleration according to the local concavity of the track? If the circle associated with the radius of curvature is to the right (left) of the track, a positive (negative) value can be given to curvature acceleration. Would it make sense? Furthermore, the upper and lower decile of curvature acceleration are remarkably similar (line 168, 48 and 32km/hr day⁻¹) despite the large variability in the size of the curvature circles of Figure 1 and 32km/hr day⁻¹ is the same value of the upper decile of tangential acceleration: maybe it is worth double-checking if the values written in the manuscript are correct.

Lines 246-257: the wrapping of the anticyclonic anomaly around the TC during recurvature has not (to my knowledge) been discussed in previous literature. Couldn't the significantly weaker ridge be due simply to blurring of the composite with increasing lead time? It would be helpful if the authors could elaborate on this aspect a bit more.

Lines 351-353: This approach to trend estimation is likely affected by the presence of serial correlation in the data. Was the independence of the acceleration values for each

quantile verified? How would the results change if trends of quantiles were computed for each year?

Lines 387-395: the interpretation of the trends would suggest that the interaction TCs-midlatitude storm track is occurring less often. How does this result relate with trends in the occurrence of ET, in this or other studies?

Lines 405-406: what is precisely meant in this context by "the impact of the phasing"? See also major point 2.

Lines 440-445: this paragraph is rather general, how exactly do the results of this study highlight/confirm the presence of bifurcation points during ET? The authors could consider removing this paragraph, as the Discussion part is already rather long.

Lines 445-458: parts of this paragraph are a repetition of the results in Section 6-7 and could be merged with them. This would emphasize the real topic of this discussion paragraph, the question "Is rapid tangential acceleration a sign of imminent extratropical transition?" The following lines provide additional data, but do not give a clear answer to this question, that is left (unsatisfactorily) pending. A more direct answer about whether it is possible to employ acceleration as a proxy of ET could be helpful and might be emphasized in the paper (see also major point 1).

Lines 469-470: the problem of "strength vs frequency" is very important for the interpretation of the results of the second, trend-related part of the manuscript. Without guidance on this aspect, the relevance of the outlined results is difficult to evaluate. The authors can provide some (at least partial) answers to this issue using the data in their possess and I encourage them to try to do so. Trends in strength can be evaluated by considering the strongest/weakest acceleration in each season, trends in frequency by checking the number of TCs underdoing ET in each season or in the number of rapidly decelerating/accelerating TCs ($\tau < 0.1$, $\tau > 0.9$). The authors might likely have additional, better ideas.

Technical/style suggestions

Line 25: the AMS Glossary of Meteorology refers to "storm track" rather than "stormtrack" (https://glossary.ametsoc.org/wiki/Storm_track), please choose the more usual formulation (unless you have a strong argument to use "stormtrack").

Lines 51-52: the sentence "While we begin..." can be omitted at this stage.

Line 62: "to" natural factors, but "natural factors" is actually not very precise. Maybe just "attributed these discrete changes to regional climate variability as well..."

Lines 75-76: what do the authors mean with "still" classified as tropical? Does it just mean that each storm must be classified as "TS" at least once in their life cycle to be considered?

Lines 92-93: just to be clear, only the time steps labeled as ET are omitted, right?

Line 130: this "3-day threshold" was not introduced early.

Lines 134-141: As many relevant data are in Table 1, Fig. 1 is not directly referenced in the discussion. Please consider to reference it, or omit it otherwise.

Lines 166-170: this paragraph can be merged with the bullet point ending on line 155, as it is its natural continuation.

Line 210: what is the characteristic signal of a cold front?

Lines 211-217: in which sense a poleward moving tropical cyclone may "either interact with an existing wave packet or [...] perturb the extratropical flow"? The two options are not mutually exclusive. In terms of initiation of Rossby waves by TCs, Riboldi et al. (2018) is likely a relevant reference.

Line 261 – PV was already introduced earlier.

Line 278: sections 6 and 7 should be moved, in my opinion, in the rather short section 4 to characterize the evolution of tangential and curvature acceleration during ET. This would help introduce the acceleration framework for the rest of the study.

Line 309: Theil-Sen

Lines 459-467: this paragraph seems a repetition of the results in the previous section, rather than a discussion item. It could be removed or merged with the description of the

results in the previous section, or drastically shortened and attached to the following discussion paragraph.

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