

Decadal variability and trends in extratropical Rossby wave packet amplitude, phase, and phase speed (DOI: 10.5194/wcd-2022-28)

Reply to referee comments on the revised manuscript

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Once more I would like to thank the two referees for kindly reviewing the revised version of the manuscript. I appreciate their time and I am glad that they found this version improved. Below are point-by-point replies (in black) to their comments (in blue) as well as a few additional changes I deemed necessary. Where applicable, line numbers and figure labels in my replies correspond to the marked-up version of the revised manuscript.

Referee #1 comments

I appreciate the detailed answers to the raised points, which will remain public for the potentially interested readers, and I am overall very satisfied with the revised version of the paper.

I just have a few, minor points for further consideration of the author before publication:

1. Reply to major point 4: A sentence could be added confirming that results of the trend analysis (Fig. 8) do not change if the angular velocity is used instead of the linear velocity.

Reply: This is now mentioned (Lines 331–333)

2. Reply to major point 5 and 7: From the provided answer, it seems that atmospheric blocking would follow indeed the expected high E/low c_p relationship. However, I still find puzzling that such a relationship does not appear in the analysis leading to Fig. 9 (and in FW20): is wave breaking leading to a decay in E but not necessarily of c_p ? I realize that this curiosity likely deserves a separate study, unless the author has additional comments on the above point. However, parts of the reply or references to relevant results by FW20 could be added to further explain this “surprising” result. A similar consideration holds for the thorough reply to major point 7, maybe a part of it can be added to the main manuscript to further contextualize the lack of covariance.

Reply: Cases of blocking are only a small subset of all days considered in this analysis and the fact that E and c_p are not generally anticorrelated (as indicated by the shape of the climatological-mean E - c_p spectra) is not too surprising. For example, there can be low-amplitude RWP objects (these are still features with an amplitude of above $15ms^{-1}$) with low, average or high phase speed. The reason is that these two RWP properties do not just depend on each other, but on other factors as well. The absence of covariance in E and c_p trends of various regions and seasons is arguably a manifestation of that. I added comments along these lines in the main text and hopefully the issue is now clearer (Lines 377–381).

3. Reply to major point 6: I do not immediately see where the issue has been discussed as the line range is unclear (the paper ends at line 470, maybe a typo?).

Reply: This is not a typo. Line numbers in my replies refer to the marked-up version of the

manuscript, such that the old and new text is seen at once.

4. [Line 19: verb is missing, should it read as “This is a manifestation”?](#)

Reply: I added a verb as suggested (Line 20).

5. [Line 180: shouldn't it read “seasonal means”?](#)

Reply: Both plural and singular forms are fine here. As it stands, it denotes the standard deviation of a variable (the field's seasonal mean in this case). If the variable is thought of as a set of data points, then “seasonal means” should be used.

6. [Line 219: “might not be well constrained”, as it is only a \(reasonable and interesting\) speculation?](#)

Reply: The speculative nature of this statement was implied by the verb “suggests” in the beginning of the sentence. Nevertheless, I followed the referee's suggestion to make this clearer (Line 223).

Referee #2 comments

Summary

As requested, I reviewed the revised manuscript along with the author responses. I found that the manuscript has been improved on several aspects, while the author also clarified raised comments and as to why to refrain from some proposed revisions. At the same time, I also feel that the improvements have been done partially, and that several of my comments need to be reconsidered along with additional changes in the manuscript. After implementing these changes, or clearly motivating why these changes are not desired, I recommend the manuscript to be accepted for publication. Below I outline these open points in a few general and minor comments.

General comments

1. **Motivation of the study, and the interpretation and implications of the results**

The revised manuscript improved by including a statement on the implications of the work in the abstract. Still, I feel that, after another careful read of the entire revised manuscript, that the main message of the article remains somewhat hidden. Here I do not suggest extending the material of the manuscript in other directions as perhaps unintentionally suggested in my earlier review. After re-reading the manuscript, I still wonder, what is the key message of this work that readers should take away? Perhaps, that decadal variability and trends of RWP characteristics vary substantially across regions and seasons? This may be important information in context of regional circulation changes in a warming climate and weather extremes (as indeed addressed as motivation in the introduction). I would like to suggest including the main message of the work in the abstract, for example, by replacing the phrase “; a manifestation of the pronounced in some areas and season” (lines 19-20) — which I find rather unclear — by such a key message. Also, I think the key message should be articulated in the “Summary and concluding remarks”.

In addition to this general comment, I also think that one of the key points of section 3.1 — the positive (negative) trend in E over the N Pacific in DJF (JJA), and the narrowing distribution and reduced E over the N Atlantic in DJF and JJA, respectively, (based on Fig. 5) deserves to be mentioned in the conclusions. If the author agrees, perhaps a sentence on this subject can be added at line 415, between the sentence ending with “... underestimate E.” and starting with “Focusing on the 1979–2019 ...”

Reply: Apart from listing the key outcomes of the individual analyses it is indeed worth concluding with a couple of sentences that extract a single take-home message. The referee is right about the key message in this case. The final sentence of the abstract has now changed to such a

statement (Lines 21–24), rather than the generic comment of the prior version. The key message of the study is now also articulated at the end of the summary in section 4 (Lines 463–465). In addition, the outcomes of Fig. 5 are now included in the conclusions as per the suggestion of the referee (Lines 442–444).

2. Section 3.3

Okay, thanks for the clarification and mentioned text revisions.

3. Section 3.4

Thank you for the clarification on the analysis using two 20-year periods. About the (compound) extremes in other seasons, it is great these figures are added in the Supplement. However, in my opinion, the results for the other seasons should be briefly described in ± 1 paragraph in section 3.4, and should also include a reason on why the author decided to elaborate on DJF, and why the other seasons were less interesting. Currently, as a reader I feel left behind with questions as to why DJF is chosen, and wonder how the analysis looks like for the other seasons. In my opinion, a scientific article shouldn't just add figures in a supplement without describing those in the text and leaving it up to the reader to interpret these figures.

Reply: Section 3.4 has been updated to address these issues. The analysis is restricted to Northern Hemisphere DJF, since one of the two goals of this subsection (see Lines 385–387) is just to emphasize that temporal variations in the aforementioned trends may exist. The pronounced interannual-to-decadal variability over N Pacific in DJF provides a good archetype that serves this purpose without tiring the reader too much. Nevertheless, it is worth adding the corresponding analysis of the other seasons in the Supplement for reference and, indeed, worth commenting on them in the main text. I have now added a paragraph that briefly discusses their main points (Lines 421–431).

4. Decadal variability or trends? Section titles

After re-reading the manuscript, I still felt sometimes somewhat confused whether the manuscript — as well as specific sections — address decadal variability or trends. In my opinion there is some inconsistency in the manuscript text:

- please, write “aspects of decadal variability and trends” in line 62 as the manuscript clearly addresses both;
- decadal variability and trends of the RWP in line 75 as subsection 3.1 addresses both;

Along the same lines, several section headers/titles do not adequately describe the context of the sections

- section 3.1; please, consider writing “Decadal variability and trends ...” as trends are an important theme of this subsection (line 190);
- section 3.2; perhaps, consider “Spatial distribution of decadal ...” (line 249);
- section 3.3 investigates “trends” and not the “variability” as it seems to me; please, consider writing “trends in joined Rossby wave packets amplitude and phase speed” (line 323)

Reply: I have followed the specific referee's suggestions in order to minimize this inconsistency and better reflect the sections' contents. A small deviation is that Section 3.3 is now titled: “Trends in the Rossby wave packet amplitude and phase speed joint distribution”.

Minor comments

1. Lines 4 and 204. To the author's reply on the comments using the phrase “may creep behind”; I understand perfectly what the author means. However, I do believe another phrasing is easily possible, for example, by simply saying “... to unveil past trends and interannual/decadal variability in the probability distribution of Rossby wave packet (RWP) amplitude and phase speed

(cp).” (Lines 3–4) and “... aim to highlight decadal variability against “noise” from interannual variability” (Lines 204) or something along those lines. I do not mean to impose these specific suggestions but would like to encourage the author to consider rephrasing.

Reply: The two instances of “creep” have now been replaced (Lines 4 and 207)

2. Lines 6-7. Please, consider simplifying the writing, for example, by saying “... where two historical reanalyses systematically underestimate E compared to three modern-era reanalyses.”

Reply: Changed as suggested (Lines 6–8)

3. Line 30. What would the sentence lose by removing the word “anyway”?

Reply: The word “anyway” here emphasizes the fact that any forcing on the circulation due to global warming (as well as any effect the circulation has on the temperature field) may lead to variability and trends on top of the ones already generated owing to internal natural variability.

4. Caption of Fig. 7. Please, clarify in the caption whether the solid and dashed black contours depict positive or negative v .

Reply: Since contour labels are perhaps not clear, I have added this information in the captions of Figs. 7 and 8.

5. Lines 286, 300, 304, etc. Please, consider rephrasing “frequency of occurrence of ...” by “occurrence frequencies of ...”, here and elsewhere, which would read better.

Reply: These instances have changed as suggested.

6. Line 415. Please, consider writing “... but less in JJA where the historical reanalyses systematically ...”.

Reply: This has now changed (Line 440).

7. Line 418. Please, remove the phrase “The decadal variability of mean”, and consider starting the sentence as is with “RWP properties ...” since this paragraph — summarizing section 3.2 — discusses trends and not the variability.

Reply: This sentence has been reformulated to account for this (Lines 445–447).

8. Line 429. In my opinion, it seems more accurate to write “... are associated with varying shifts in the E–cp domain between seasons and regions” and to replace “a lack of” by “the absence of”.

Reply: This is true. I have changed that as suggested (Lines 456–458).

Additional changes

1. A sentence is added to explain the meaning of the seasonal-mean phase index value with an example (Lines 282–284).
2. I changed “multi-decadal” to “decadal” in order to have a consistent terminology for the trend analyses in the text (Line 210).
3. I changed “most” to “much” (Line 394).
4. I moved the reference to Figs. S6–S8 from Line 423 to 410.
5. Line 494 has been simplified.