Weather and Climate Dynamics Discussions

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Interactive
comment

Interactive comment on "**Atmospheric blocking types: Frequencies and transitions**" *by* **Carola Detring et al.**

Anonymous Referee #1

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1   General comment

This manuscript presents a statistical analysis of blocking in part of the Northern Hemi-sphere with a focus on the North-Atlantic European region. Using a set of reanalysis data, the authors investigate blocking frequencies and their trends during the period 1990-2019. The study applies a novel method to assess blocking based on the detection of centers of vorticity; this method allows one to distinguish two different types of blocking and to consider the transitions between them.

The paper is a welcome contribution to the discussion, since blocking and related trends are important topics in our science, and in the end it would be desirable that the results will be published. By the use of their specific methodology, the authors are

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[Discussion paper](#)

able to quantify a few novel aspects which would be hard to address using other methods. At the same time there is a number of issues which I think should be sorted out before the manuscript can be published.

2 Major issues

Statistical significance

In several of the figures I was missing a quantification of the statistical uncertainty (e.g., Fig. 6, Fig. 9). In my eyes all results must be tested with regard to their statistical significance.

In some other plots the authors provide a range of statistical uncertainty, but I could not find out how this was determined and what assumptions were underlying this estimate. I think that the authors need to explicitly describe (in the methods section) how statistical uncertainty was determined.

In the main section (section 4) the authors discuss the results irrespective of whether they are significant or not. In several places they seem to draw firm conclusions from results which are (as the authors say themselves) not statistically significant. It think that this is not good scientific practice. Rather, only those results that are statistically significant can be considered as "results" and should be discussed and used (for instance) to test hypotheses etc. Marginal statistical significance occurs in the present case when breaking down the results to individual months or seasons in section 4 (e.g., Fig. 16). In particular, in the summary section the authors should only refer to those results which are statistically significant.

<u>Better motivation</u>

It would be desirable if the authors can provide a better or more explicit motivation for their work and, especially, for the specific method that they chose to use. What is the advantage of their method in comparison with the many other methods that have been used in the past? What specific questions can one address that previous authors were not able to address? Why are those questions important? One possible avenue for improvement into that direction would be to formulate an interesting hypothesis and test this hypothesis with the analysis.

Lacking a more explicit motivation, the reader is somewhat left in the limbo as to what one is supposed to learn. One can always invent a new method an apply that method to reanalysis data in order to produce "results". But without further comment it would not be clear to what extent these "new" results are important or relevant.

To be sure, I believe that the authors are able to provide such an improved motivation. In fact, some material in this direction is scattered throughout the text. I just urge the authors to collect this information and illuminate it in a more explicit fashion.

<u>What are the true results?</u>

This issue is related to the previous one. In the discussion section (which is partly just a short version of section 4) the authors mention a few caveats and sensitivities, but the reader is not told whether and to what extent these have an impact on the results. In other words, which of the results are true results and which are only consequences of the specific method that was applied? For instance, transition probabilities depend on the temporal resolution of the underlying data, so the specific value of the probability cannot possibly be a "true result". Similarly, the discussion provides the statement that different methods yield different numbers. My question would be: which of the results survive a change in the method? More broadly speaking: what is this paper's

unique contribution to the topic? The devil's advocate would argue: "Well, you are using a novel method to investigate a problem that has been studied often times before; your results differ to some extent from previous results and many of your results are statistically insignificant". I am sure the authors have a good reply to such a provocative statement.

Again, I think that there is a unique contribution to the topic from this paper, I only say that this must be worked out more clearly.

3 Minor issues

- Quite a number of minor issues are added as comments to the pdf of the manuscript.

- Sections 5 and 6 are partly redundant. For instance, large parts of section 5 repeat what has been said in section 4. What's more important: the summary section should not simply be a shortened version of the results section; rather, the reader expects a summary (plus discussion) on a higher level of abstraction.

- In some parts of the text the quality of the English could and should be improved.

Please also note the supplement to this comment:
https://wcd.copernicus.org/preprints/wcd-2020-62/wcd-2020-62-RC1-supplement.pdf

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Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-62, 2020.