Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-20-RC1, 2020

Interactive comment on "Impacts of the North Atlantic Oscillation on Winter Precipitations and Storm Track Variability in Southeast Canada and Northeast US" *by* Julien Chartrand and Francesco Salvatore Rocco Pausata

Anonymous Referee #1

Received and published: 12 June 2020

In the manuscript "Impacts of the North Atlantic Oscillation on Winter Precipitations and Storm Track Variability in Southeast Canada and Northeast US" the authors present a composite analysis of snowfall, total precipitation and cyclone track densities. They focus their discussion on the northeasternmost part of the American continent. The manuscript is easy to follow and the Figures are generally clear.

I do however have considerable doubts about the novelty of the findings in this article. The authors cite quite a few studies in the introduction and throughout the manuscript that considered similar diagnostics with a similar scientific question for a similar region.

Not surprisingly, throughout the manuscript, the authors then describe their findings as consistent with what has been pointed out before. That is in my eyes not enough to warrant a new publication.

I am nevertheless recommending major revisions in the faith that the authors will be able to derive genuinely novel results from a similar set of results to the one presented in the current manuscript. It might however require a shift or broadening of the scope of the manuscript. For example the authors could explicitly consider extreme precipitation and snowfall rather than cold season means, or relate precipitation and snowfall to weather regimes (such as Greenland blocking) rather than the NAO. Whatever the authors' choice, the motivation and purpose of the study must become much more clear, in particular in the introduction and conclusion.

Specific major issues

L75. The preceding paragraphs list quite a number of previous studies and their explanations for the observed impact on northeastern US snowfall. In the light of these, I don't think the authors can claim that there have only been few studies on the topic, and neither that they in general are all unclear or implicit in their reasoning. In other words, it's not become clear to me which gap in the understanding the authors intend to fill.

Sec 2.2: What is the motivation for developing yet another cyclone detection algorithm? There are already many variants published, some of them also applied to ERA5 and other comparatively high-resolution datasets. Specifically, if the results from this algorithm compare are well with those from other methods (L139-141), then what is the point of adding this complexity? I would urge the authors to use one of the more established methods unless there is a very good reason not to. In that case this reason will need to be convincingly laid out in the manuscript.

If the authors decide to keep their own cyclone detection and tracking algorithm, this algorithm should be properly evaluated; to be able to evaluate the climatologies, I would

require a larger region than the one shown in Figure 3, and also a comparison of individual cyclone tracks with those obtained from other algorithms. To make inter-comparisons easier, it would further be good if the authors used the same units for their cyclone/cyclogenesis densities as in Neu et al. (2013), percentages of cyclone occurrence per time step and (1000 km)ˆ2.

L333-335: What exactly are these new results? Throughout the manuscript, findings have been pointed out as consistent with previous studies. This manuscript might still be redundant, if it does not add anything new, even if only few studies explicitly consider the region of southeastern Canada.

Specific minor issues

L90. Singular reads awkward, may be better: Positive values ... are .

L126/7: How can a spatial filter filter out stationary (in time, I would assume) minima?

L129: What is the "previous/current/next position assumption"?

L134: Please provide a complete list of criteria for the cyclone detection and tracking. Otherwise your results will not be reproducible.

Fig 4: The purple contour is hardly visible in some of the composites/correlation maps. This makes some of the Figure panels a bit misleading, as most of the displayed signal is not significant. One way to solve the issue could be to use filled contours only for significant parts of the map, and otherwise only contour lines. Then the significant parts would stand out much more clearly.

L171: The precipitation over the Great Lakes seems only marginally positively correlated with the NAO, just exceeding the zero contour.

L199: Typo: Winter snowfall. Also, may be you can relate that finding to Fig 1c, showing that the majority of precipitation comes as snow in southern Canada, and sec 3.1 already showed little impact of the NAO on total precipitation. With that in mind, the

result here seems quite obvious.

Figs. 5-6: Why are these a separate Figures? Logically, both would seem to fit quite well as additional panels into Fig 4.

Figs. 7-8: Which parts of these Figures are significant, if any? I appreciate that cyclone densities and in particular cyclogenesis densities tend to be very noisy, so I would not be surprised if no part of the signal reaches statistical significance. That is fine, but should be explicitly noted.

Sec 3.5: At the same time, the description of these Figures in sec. 3.5 should reflect the significance of the results. Some formulations such as "considerable increase" (L238) or "strongly favored" (L244) will likely need to be toned down. This also applies to the conclusion.

L245: What is "the" upper-level trough? The analysis is based on daily data, so there will be hundreds of troughs moving through the region in 40 winter seasons. Further, what is this conclusion based on? Even taking into account Fig 9 (which at this point has not been introduced yet), the conclusion seems to be too strong based on what is presented. The ridge-trough structure over the CONUS seems only slightly more pronounced during NAO- than during NAO+. Finally, more generally, composite analyses can at best hint at where there might be causal relations. Here I don't think there's an a-priori reason to believe that the 500 hPa is causing surface cyclogenesis and not vice-versa.

L245, point 2: Somewhere around here the manuscript transitions from a discussion of the cyclone composites to a more open discussion of all results. It might be useful to indicate that by a (sub)section.

L248: How do you deduce where there is a jet exit and a jet streak?

Fig 10: The values seem to exceed the color scale by far, in particular for the NAO+ composite.

L276: Why refer to Fig 11 instead of Fig 7? Figure 11 has not been introduced and described yet.

L279-281: How do you see storm track variability and the variability in cyclone paths in Fig 7a?

L284. I was not aware of this name before. If there is a reason for this name that you can explain in half a sentence, you might consider adding that here.

L290-291: What is the "Northern Rockies storm track"? This term seems a bit self-contradictory as a storm track would cover a large area such as an ocean basin.

L293-297: Opposing effects for which quantity are being balanced? What result is the discussion of extreme and light precipitation based on?

L299: Why capital "Neutral"?

L298-301: This seems to be the introduction to Fig 11 that I have been missing before. It however remains a bit unclear what I am to take away from Fig 11 in addition to what is already apparent from Fig 7. Further, how does this area of negative correlation conform the results of the study? This seems a very general statement which might not be equally valid for the variety of results and hypothesis presented before.

L306: Here as well as in the discussion part of sec 3.5: Do the authors equate the cyclone track density and the storm track? In my opinion, that would be an justifiable assumption for the purposes of this study, but it should be explicitly mentioned.

L338-339: The paper has not considered extreme precipitation at all, so this final conclusion appears to be quite a stretch.

L341-344: If the authors keep their own cyclone tracking algorithm, this algorithm, or its results for the ERA5 reanalysis, should be made available.

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