

General comments

The authors are using a causal effect network to forecast Atlantic tropical cyclone activity (July-October) as presented by accumulated cyclone energy (ACE). They developed two models, using ERA5 and JRA-55 data, for the months of March and May. Analysing the deterministic and probabilistic skill of their models, they find that it is competitive with other seasonal forecasts currently available.

The technique used here identifies predictors that have already been recognized in previous studies. So in that sense, the manuscript doesn't lead to new insights into the drivers of Atlantic cyclone activity. However, the objective of the paper is more to showcase this new method and the fact that it recovers the known drivers lends credibility to the results. I would encourage the authors to apply this technique for basins which have received less attention and for which the current forecast systems have more difficulty (e.g. Australian basin).

As I mention in my comments below, the authors identify regions of mlsp in the southern hemisphere in March as robust predictors of vertical wind shear during the hurricane season. Given the limited amount of observations that go into constructing the reanalyses over that region, I was wondering whether the authors thought this link was real or an artifact of the lack of observations. The authors should provide a comment to that effect in the manuscript.

Finally, the text is well written and easy to follow. I recommend it for publication after the minor points below have been addressed.

Specific comments

Line 18: "Earlier seasonal hurricane forecasting provides a multi-month lead time to implement more effective disaster risk reduction measures."

I'm not aware of any organization or government using seasonal forecasts for disaster risk reduction. Are the authors aware of any? If not, I would recommend removing this sentence from the abstract.

Line 26: "Preparedness for the secondary impacts can however be improved if reliable forecasts for the potential risks of the upcoming hurricane season are available (Martinez 2018)."

Does this refer to seasonal forecast? If so, it might be worth adding a sentence explaining how these forecasts are used in that context.

Line 156: "The color of the nodes indicates the strength of the auto dependence,"

auto dependence has not been defined. Can the authors explain what it means? And how is the strength of the link defined?

Line 182: “our cross-validated forecast seems competitive with operational forecasts”.

Could the authors be more precise? Which operational forecasts are they referring to?

Line 199: “deficit to predict some of the most active seasons might be due to missing relevant predictors”

There is also a stochastic component to TC formation. Two different years with similar large-scale fields conditions would/could lead to a different numbers of cyclones.

Line 231: “As robust precursors, a high-pressure system over the southern Indian Ocean and a low-pressure system eastward of New Zealand are identified in nearly all training sets”

Is this a true feature or possibly a feature of the reanalysis, which have very little observation over the southern ocean?

Line 262: “Overall these precursors seem less robust in JRA55 and thereby the forecast skill is also slightly reduced”

The Spearman correlation is higher using JRA-55 in March actually.

Technical corrections

Line 21: “Tropical cyclones (TCs) are among the most damaging weather events in many tropical and subtropical regions.”

This statement should be referenced.

Line 28: Klotzbach (2019) should be Klotzbach et al. 2019

Klotzbach, P. J., E. S. Blake, J. Camp, L.-P. Caron, J. Chan, N. Kang, Y. Kuleshov, S.-M. Lee, H. Murakami, M. Saunders, Y. Takaya, F. Vitart, and R. Zhan, 2019: Seasonal tropical cyclone forecasting. *Tropical Cyclone Research and Review*, 8, 134-149, doi: 10.6057/2019TCRR03.03.

Line 30: "A whole variety of forecasting methods are applied ranging from purely statistical forecasts to forecasts based on regional climate model simulations and hybrid approaches."

I'm not familiar with the methodology of every group, but nowadays global climate models are used instead of regional climate models.

Line 32: "Their skill depends on their ability to represent TC genesis and development and their capacity to forecast the large-scale circulation over the Atlantic main development region (MDR)."

As well as their ability to adequately represent the interaction between the two.

Line 35: "With increasing spatial resolution their representation of TCs improves."

I would add a reference here.

Line 61: "official WMO agencies"

Line 64: "We use the monthly reanalysis data provided on a regular 1-degree grid."

Aren't the ERA5 data at 35 km resolution?

Line 88: "As such, we cannot exclude potential common drivers on longer, e.g. annual time scales"

Do you mean multi-annual or decadal time scales?

Line 99: "A statistical model is built"

Line 126: "we will still refer to our cross-validated predictions as "forecasts""

I would recommend using hindcast, to avoid confusion.

Line 199: hypothise -> hypothesize

Line 202: “as a predictor”

Line 219: “(BSS) is indicated in the lower right corner of each panel.”

Line 294: “causal effect network rather helps to identify “the least spuriously link”

Line 297: “The detected causal links might not be stationary over time”

Nonstationarity in the climate influence on TC activity has been pointed out by:

Fink AH, Schrage JM, Kotthaus S (2010) On the potential causes of the nonstationary correlations between West African precipitation and Atlantic Hurricane activity. J Clim 23(20):5437–5456

Caron, L-P, M Boudreault and C Bruyère (2015) Changes in large-scale controls of Atlantic tropical cyclone activity with the phases of the Atlantic Multidecadal Oscillation. Climate Dynamics, 44, 1801-1821. doi:10.1007/s00382-014-2186-5.

Figure 1: I would like to thank the authors for taking the time to produce this figure. It helped a lot in understanding the methodology.