Response to Anonymous Referee #2

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).

> *We thank the reviewer for this useful suggestion. We think that for the present manuscript it makes sense to keep the focus on the Atlantic basin, exactly because the involved mechanisms are relatively well documented. Thus, our study should serve as a proof of concept of the method and we intend to apply it to other basins in a follow up study (see line 358-359). We have modified the framing of the manuscript accordingly.*

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.

> *We share the concern raised by the reviewer and we are aware of the possibility of detecting artifacts of the reanalysis as precursors for our model. In the revised manuscript we therefore discuss a potential mechanism that could explain the influence of the identified MSLP dipole in March on VWS in the hurricane season (see Fig. 10 and lines 268-281).*
>
> *We speculate that a high-pressure anomaly in the southern Indian Ocean and a low-pressure anomaly in the western South Pacific could weaken the trade winds in the western Pacific which is favorable for El Nino formation (which in turn is known to have an enhancing influence on VWS in the Atlantic).*
> *We test this hypothesis by constructing a causal effect network that shows the links between SSTs in the El Nino3.4 region, trade winds in the western Pacific and the strength of the identified MSLP dipole. This analysis supports our hypothesis. Note, however, that more research is needed to confirm the robustness of this linkage, something we also clearly state in the paper.*
>
> *Furthermore, we extended the sensitivity tests adding a trend analysis for the identified precursors (see Fig. S3, line 293-296) and an application of a forecast model trained on the period of 1980-2018 to an earlier test period (1958-1978) using the JRA55 reanalysis (see Fig. S8, line 308-319 and the response to reviewer 1).*

Given the limited available datasets, we cannot fully rule out the eventuality of detecting artifacts as precursors. However, our sensitivity tests suggest that the identified MSLP precursors have a physical link to VWS in during the hurricane season.

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

We thank the reviewer for this overall positive feedback.

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.

With this statement we wanted to refer to long term planning of governments and financial institutions. As we don’t have a specific reference outlining how seasonal forecasts are used in this context we removed the sentence.

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.

We thank the reviewer for this comment. Martinez 2018 indeed refers to forecasts on shorter time scales. We now reference (Murphy et al. 2001) *that discusses the use of seasonal forecasts more generally. We think that for secondary impacts the statements of this study are also applicable to seasonal hurricane forecasts.*

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?

We thank the reviewer for this comment. We used the term “auto-dependence” to differentiate it from “auto-correlation”. Thus, it refers to how much the past of a process influences the next time-step. Both auto-dependence and link strength are calculated using partial correlations as described in Runge et al. (2019).
We only use the link strength to identify robust precursors. The final forecast model is based on a simple regression which is why we do not think that in depth discussion of the interpretation of link strengths is necessary.
In the revised manuscript we shortly explain the meaning of auto-dependence and link strength in the caption of Fig. 3 (line 162-165).

Line 182: "our cross-validated forecast seems competitive with operational forecasts". Could the authors be more precise? Which operational forecasts are they referring to?

We were referring to Fig. 1 from Klotzbach et al. 2019 in which the skill of seasonal April forecasts of ACE provided by CSU, TSR and NOAA is presented. We added the abbreviations of these institutes to the manuscript and specifically mention Fig. 1 in the citation (line 191-192).

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.

We thank the reviewer for this comment. We added the following sentence to the manuscript (line 212-213):
"Furthermore, it has to be noted that there is some stochastic component to TC formation which systematically limits the skill of our empirical forecast model that is based on favorable conditions for TC formation."

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?

We fully understand the concern of the reviewer. Please find our answer to this question below general remarks of the reviewer.

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.

We thank the reviewer for pointing this out. We think, however, that there has been some confusion. The Spearman correlation in March is 0.22 for JRA55 and 0.27 for ERA5.

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.

We included a reference to the MunichRe natural hazard database (Munich Re 2020)*: https://natcatservice.munichre.com/*

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.

Done

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.

We thank the reviewer for correcting this statement. We changed “regional” to “global”.

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.

We fully agree with the reviewer and added the sentence (line 37).

Line 35: “With increasing spatial resolution their representation of TCs improves.” I would add a reference here.

We added the following reference (Roberts et al. 2020)*:*
1. Roberts, M. J. et al. Impact of model resolution on tropical cyclone simulation using the HighResMIP-PRIMAVERA multimodel ensemble. J. Clim. 33, 2557–2583 (2020).

Line 61: “official WMO agencies”

Done

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

Aren’t the ERA5 data at 35 km resolution?

ERA5 is indeed available on a higher resolution. We are using the 1-degree grid (which is also provided on the website). We hope that removing the word “provided” clarifies this.

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?

We meant multi-annual up to multi-decadal time scales. Based on a comment of reviewer 1, we rewrote this paragraph also specifying these time scales (line 89-95).

Line 99: “A statistical model is built”

Thanks

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

I would recommend using hindcast, to avoid confusion.

We agree with the reviewer and write about “hindcasts” throughout the revised manuscript.

Line 199: hypothise -> hypothesize

Done

Line 202: “as a predictor”

Done

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

Done

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

Done

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.

We thank the reviewer for suggesting these references (that we now included in line 93, 354). Following the suggestion of reviewer 1, we also included a test using the earlier period (1958-1978) of JRA55 (see Fig. S8). Since the data quality of the reanalysis before the satellite era (before 1979) seems questionable in the southern hemispheric oceans (Tennant 2004) *we still cannot fully investigate potential non-stationarities in our precursors. See line 308-319. Nevertheless, we have added a sentence discussing this possibility.*

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.

We would like to thank the author for this nice comment. We are very glad to hear that this figure is helpful.

References

Munich Re (2020) Natural catastrophe know-how for risk management and research. https://natcatservice.munichre.com. Accessed 19 May 2020

Murphy SJ, Washington R, Downing TE, et al (2001) Seasonal forecasting for climate hazards: Prospects and responses. Nat Hazards 23:171–196. doi: 10.1023/A:1011160904414

Roberts MJ, Camp J, Seddon J, et al (2020) Impact of model resolution on tropical cyclone simulation using the HighResMIP-PRIMAVERA multimodel ensemble. J Clim 33:2557–2583. doi: 10.1175/JCLI-D-19-0639.1

Runge J, Nowack P, Kretschmer M, et al (2019) Detecting and quantifying causal associations in large nonlinear time series datasets. Sci Adv 5:eaau4996. doi: 10.1126/sciadv.aau4996

Tennant W (2004) Considerations when using pre-1979 NCEP/NCAR reanalyses in the southern hemisphere. Geophys Res Lett 31:n/a-n/a. doi: 10.1029/2004GL019751