

Black second set of reviewer comments

Blue previous responses

Green current responses

Responses to author responses on major comments:

1) At the very least, the title should add in "Model Predictability..."

The abstract is clear that the analysis is "in climate model forecasts," and predictability studies are often based on model behavior. So, we think the title is appropriate.

We do not find the reviewer's argument for adding "model" to the title entirely compelling. If editor sees a problem with the title, we are willing to change the title.

2) The authors did not address this concern. They should examine the relationship of the physical pathway and/or composited synoptic weather features to understand this modulation and pathway. Without it, there is no physical basis for the predictability relationships and it would be rather useless from an operational perspective.

We tried to address this point in our responses to the first review and made changes in the text that we thought were appropriate. It is unclear what parts of our response were inadequate. It is unclear to us what specific analysis the reviewer is suggesting now: "They should examine the relationship of the physical pathway and/or composited synoptic weather features to understand this modulation and pathway." Also the last point about the "operational perspective" is unclear to us since we make no claims that our findings are ready for use in operational forecasting. We think that our composites analyses are sufficient to support our findings within the context of physics-based climate models which is the setting of our analysis.

3) OK, but AO is not the dominant mode of variability for severe weather for the months examined in this study. The MJO is a much larger factor and is not addressed.

We are glad that the reviewer accepts our response. We do not claim anywhere that the AO is the dominant mode of variability for severe weather in the months considered in this study.

As we noted in our first response, we do not see a direct connection between the monthly analysis here and MJO signals which do not persist that long. In particular, the monthly data here could not credibly be used to analyze the MJO signal. If there is a connection that we have overlooked we would be happy to hear it and add an appropriate reference.

4) The author responses to comment 4 are adequate. A google search points directly to the Canadian tornado data: <https://open.canada.ca/data/en/dataset/65658050-7a80-4da3-9a09-da137c203a34>

5) The author responses to comment 5 are adequate.

6) The response re: TEI data is not adequate for an open source journal in my opinion. Yes, the CFSv2 data is available via the URL, but the calculated TEI data are not. The TEI, AO, and ENSO derived data should be provided in a repository to ensure reproducibility.

We used only variables provided directly in the CFSv2 output. The formula for TEI is given in the manuscript and is a direct calculation. Nino 3.4 is a box average of SST, and the AO index is the first PC of Z1000.

As the reviewer notes, the CFSv2 data is available. TEI is directly computed from that CFSv2 data in one line using the formula given in the manuscript.

$$\text{TEI} = \exp(-14.01 + 1.36 \cdot \log(\text{cPrpc}) + 1.89 \cdot \log(\text{abs}(\text{SRH}))) \times \text{days per month}$$

Addl. comments:

The others mention, "We are happy to add other appropriate references." My original suggestion was that the paper initially read like a literature review of only the authors' works. Adding relevant citations doesn't cost anything and would enhance the breadth of the manuscript for readers that are unfamiliar with these areas of research.

... suggestions for the inclusion of additional literature are more constructive when accompanied by the relevant citations.

In the first response, we gave a detailed and lengthy explanation for the choice of citations in the Discussion and repeatedly asked for additional references that we might have overlooked. We don't understand why the reviewer questions the choice of citations but does not suggest additional or alternative citations. The WCD obligations for referees state that "Any statement that an observation, derivation, or argument had been previously reported should be accompanied by the relevant citation."

I disagree with the comment about a three-year average. 15+ years should be used in a climatological context.

Our original submission included preliminary report numbers for 2021, which were the data then available. Actual report numbers were provided by NOAA/SPC on 11 July 2022 for 2021. We are now able to follow the reviewer's suggestion and compare 2021 numbers with those of the period 2006–2020, and we have revised the text accordingly.