

Geosci. Model Dev. Discuss., referee comment RC2  
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## Comment on gmd-2021-131

Anonymous Referee #2

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Referee comment on "Systematic global evaluation of accuracy of seasonal climate forecasts for monthly precipitation of JMA/MRI-CPS2 by comparing with a statistical system using climate indices" by Yuji Masutomi et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-131-RC2>, 2021

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This study sets out to compare global precipitation forecasts generated by a dynamical SCF and a climate-index based statistical forecasting system. It is found the dynamical forecasts performed better in the first month and the statistical forecasts performed better for longer lead times, with some nuances over land/ocean and around the equator. It is a potentially interesting study and the paper is presented well. However, I believe the study suffers a major flaw that the authors will need to rectify, and some additional work that pushes the revisions beyond major. Here are my major and minor comments:

### Major comments

- The statistical forecasting methodology adopts the predictor with the highest correlation after model fitting (Figure 1 and lines 82-88 ). This means the selected climate indices are based on peeking during the evaluation period and selected to maximise the performance over the evaluation period. With 18 climate indices, the risk of artificially inflating the performance of the statistical system is high. I suggest that the selected climate index for each year ought to vary based on total exclusion of the data for that year through all steps up to and including the calculation of the correlation.
- Seasonal forecasting is highly uncertain, and the deterministic forecasts of the type produced here may be of limited value to end users. I suggest that the authors consider a means to produce uncertainty estimates or "ensembles" and include some more typical SCF analysis and verification results. I believe that is important to analyse the underlying skill in the ensemble mean as the authors have done, however, adopting it as the only verification approach side-steps a major advantage of the dynamical forecasts in having ensembles.

Minor comments:

- I suggest that the motivation for studying precipitation should be established earlier in the introduction, and a comment that statistical forecasts are suited to forecasting a small number of variables.
- The process of selecting a different climate index for each cell could lead to very spatially inconsistent forecasts. I suggest this is addressed in the revised discussion.
- It is one intention of this study to highlight that dynamical models don't always capture the slow dynamics and teleconnections. However, the paper is rather brief and doesn't attempt to explain why particular climate indices are important, or even the frequency at which they are selected. I suggest in the revision some further attention is given to highlighting the most important indices.
- Mean square error and skill scores are referred to in the introduction, however they are not used in the paper. I suggest making the introduction and methods more consistent.
- It sounds to me like the climate indices wouldn't be available in time to make real time forecasts due the week delay (e.g. section 4.3). I suggest addressing this issue in the revision.