Dear Dr. Neale

I have carefully reviewed their response and am deeply disappointed. Not only did their reply fail to address the key points, but more importantly, the authors appear to be responding to non-existent arguments (as noted in my comments below). I believe this study is methodologically flawed and conceptually confusing. Therefore, I am convinced that this work should not be published.

Below is my response to their replies. The red text represents their original comments, the bold black text is my previous feedback, and the blue text contains my current responses.

We thank you for taking your time and giving your comments, which are useful for improving the manuscript. Here are point wise clarifications.

1. The article only explains how signal and noise variance are defined and calculated. Since variance itself is not the actual component, it is unclear how the signal and noise are extracted from the data. The concept and defintion are totally different between the variance and the variable itself.

Reply: There are numerous paper on how signal and noise components are extracted from model data and some of them are cited here (e.g. Kang and Shukla, 2006; Scaife et al., 2014; Saha et al., 2016a; Scaife and Smith, 2018; Weisheimer et al., 2018 and many more). While inter-ensemble spread is considered as noise/internal component, the ensemble mean is the signal/external component (equation 1 and 2 respectively in our manuscript). How signal and noise are extracted from data is clearly mentioned in lines 104-108 of the manuscript, section 2.3.1.

The articles cited by the authors only discuss signal variance and noise variance. It is problematic to treat the ensemble mean directly as the signal/external component. As a measure of variability, the signal should not be constrained by sign—how does one interpret a “positive signal” versus a “negative signal”? Therefore, it is more appropriate to use the square of the ensemble mean to represent the signal.

In the author’s statements in lines 104–109 as below, I could not find a clear definition of either the signal or the noise.

respectively. Here, predictable and unpredictable components are termed external/signal and internal/noise components, respectively. The ratio of external to internal variance is known as the signal-to-noise ratio (SNR). If x is the precipitation field of the model, i is the year of the model integration (total year 'N'), and j is the number of ensemble simulations (total ensemble n = 52), then internal variance following Rowell et al. (1995), can be expressed as

2. The article tries to discuss and analyze the paradox, but the purpose of using Nino3.4 to predict precipitation remains unclear. What is the intention behind comparing it with dynamic models? Is it to demonstrate whether the actual or potential forecast skill of dynamic models is higher or lower, reasonable or unreasonable? The objective is not clearly stated. Moreover, can using Nino3.4 to predict precipitation effectively achieve these goals? Would the forecast skill be reliable? Was the forecast skill mentioned in the article derived from training or test data? Similarly, were other modes affecting precipitation in the Indian region, such as IOD, considered?

Reply: The idea is to asses prediction skill of not only predictants (i.e. ISMR, PACR), but also the fidelity in simulating global predictors (e.g. ENSO) and their teleconnections. Figure 9 shows multiple correlations involving major global predictors (Niño3.4, IOD, PDO, AMO) and sub-seasonal components.

Your response does not address my question. Such a simple linear regression approach is unreliable and insufficient to explain any core issues discussed in this paper.

3. Rowell (1995) never defined signal variance and noise variance using ANOVA. While they did mention ANOVA, it was only used for statistical testing. The authors should revisit Rowell (1995) to better understand the content. ANOVA has exactly defintion in statistics, which should be followed to avoide unnecessary confusion.

Reply: Please look into page no 699 of Rowell et al. (1995).
https://rmets.onlinelibrary.wiley.com/doi/epdf/10.1002/qj.49712152311

,which mention "The approach we use to estimate the components of variance closely follows an 'analysis of variance' methodology ..."

I could not find the answers provided by the authors in Rowell (page 699) as below. It should be noted that ANOVA has a rigorous statistical definition. The authors, however, only performed variance partitioning, not ANOVA.

RAINFALL VARIABILITY OVER NORTH AFRICA 699

The next stage of research will be to explore the physical mechanisms which link the SST patterns to seasonal rainfall variability. Circulation changes over north Africa will be examined in a later publication, and some global-scale circulation patterns associated with Sahelian rainfall anomalies are presented by Ward *et al.* (1994).

Given that SST patterns are often predictable at least a few months in advance, this offers hope for the production of skilful forecasts of seasonal JAS rainfall anomalies averaged over the Sahel, Soudan and Guinea Coast. Indeed, such forecasts have now been issued by the UK Meteorological Office for the Sahel region since 1986, and for the Soudan and Guinea Coast regions since 1992, on an experimental basis (see Ward *et al.* (1993) for details). In order that such forecasts achieve maximum utility, further research is required on the variations of rainfall–SST relationships *within* the large regions used here and *within* the July to September season.

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4. I do not understand the meaning of the statement: "The use of the orthogonality assumption is a methodological simplification to partition variance across time scales; it does not imply the absence of physical co-variability." Do physical and mathematical co-variability have different interpretations? In my opinion, if two quantities are physically related, they cannot be assumed to be orthogonal in mathematics. Additionally, I do not comprehend the authors' claim that "sub-seasonal components are the building blocks of the seasonal mean." Following this logic, all time scales would be sources of error, since hourly components are the building blocks of the daily mean, and daily components are the building blocks of the weekly mean, and so on.

Reply: The argument why we are using assumption of orthogonality and not the actual one, lies on the fact that it is challenging (if not impossible) in a non-linear system to separate individual components.

It seems no basis to argue the "challenging to separate" as a justification for such an assumption. This is the most critical weakness of the study: on the one hand, it attempts to examine the effect of A on B using linear statistical analysis, while on the other hand, it assumes that A and B are orthogonal, implying that their covariance (or correlation coefficient) is zero.

Sub-seasonal components of the monsoon particularly have clear preferred band. Some of the band are more vigorous in terms of their spatial scale, strength than the others. In terms of their contribution to the mean and variability/predictability also varies. While MISOs have very large spatial structure and strong sub-seasonal variability, their contribution to year-to-year monsoon rainfall variability is minimum (weak negative correlation). So, clearly, we are not talking here about hourly/daily events but some known and prominent sub-seasonal variability/bands, which shape the seasonal monsoon rainfall of a year. Here are literatures, cited in support of our arguments (Saha et al., 2019; Borah et al., 2020). Some important papers in the similar lines but not cited here are.

I am drawing this inference based on the authors' own argument. You may choose to ignore or omit other scales of the atmospheric process, but I cannot overlook them. Isn't that ?

5. So I have to feel sorry to decline this work again. The topic is interesting that is the reason why I agreed with reviewing it. Unfortunately I do not learn more from this work. To my understanding, the paradox should be from the "defintion" of potential predictability. The ratio of signal to noise may not well represent the potential predictability. If authors wish to work this problem, I suggest them to seek other measures to quantify the potential predictabilty.

Reply: We wish, if you could have read the full manuscript. The main content of the manuscript is the following:

i) Perfect model framework is used to estimate potential predictability of seasonal anomaly, which often shows paradoxical behaviour. 'Analysis of variance' framework is used for calculating 'signal' and 'noise' components using 52-ensemble member re-forecasts.

ii) Here we argue that 'perfect model framework' is not adequate, as the error growth is not from only initial condition errors but also from other sources, like physics, numerical scheme etc. We demonstrated that sub-seasonal component, which is part of the physics, adds error (biased contribution) in the seasonal forecast anomaly (i.e. Figure 7). However, 'perfect model framework' assumes, ensemble spread solely attributed to initial condition error. Consequently, true limit of predictability is not known. So, here our argument matches with your point of view that the method of estimating PPL based on perfect model framework is inadequate. We have already mentioned it in lines 337-344, in the last para of section 3.3

The authors appear to lack a clear understanding of the PPL issue. PPL is fundamentally a product of the "perfect model" framework. Once model errors are taken into account, it ceases to be a PPL problem. Therefore, the very premise of this study is conceptually inconsistent.

iii) Finally we propose a method for estimating PPL, which is free from paradox (section 3.4). Therefore, we believe the rationale provided for rejection does not fully capture the merits of the manuscript