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Comment on wcd-2021-58

Anonymous Referee #2

Referee comment on "Stationary wave biases and their effect on upward troposphere-stratosphere coupling in sub-seasonal prediction models" by Chen Schwartz et al.,
Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-58-RC2>, 2021

Stationary Waves and Upward Troposphere-Stratosphere Coupling in S2S Models

C. Schwartz et al.

The authors analyze the northern hemisphere stationary wave field in subseasonal forecasts from eleven subseasonal forecast groups, considering how model biases in the stationary wave field evolve as a function of the forecast lead time. They find that all models develop some biases by about week 3 of the forecasts in both the troposphere and stratosphere, with some biases arising earlier in the integration. There is some tendency for models with lower resolution in the stratosphere to have larger biases in both the stratosphere and troposphere. Furthermore, these models tend to show a larger bias in the stratospheric wave 1 field whereas models with a higher resolution in the stratosphere show a larger bias in the wave 2 field. Some further evidence is presented linking the tropospheric biases to biases in tropical convection.

Identifying and correcting these biases would seem to be a promising way forward to improving S2S forecasts: the mean stationary wave field should be a relatively predictable component of the circulation and the biases identified here are linked to some extent to errors in the mean state. The results are thus noteworthy and of definite interest to the readership of WCD; in particular connection between resolution and wavenumber of bias in the stratosphere is curious.

However, the manuscript feels very rushed and the analysis, while interesting, also feels somewhat unsatisfyingly incomplete. There is certainly much more to understand about the character of these biases and their origins than is demonstrated here. And while the

analysis is certainly limited by the output available, I have a few specific concerns about the analysis and interpretation that I feel need to be addressed in order for the manuscript to be published. Beyond that I have many questions and suggestions for ways to deepen the analysis. I don't want to suggest that they all be pursued, and the present results are certainly of note, but I do feel that the paper needs a bit more depth to warrant publication.

General concerns

1) Use and choice of small regions for bias characterization

The discussion around l125 suggests that the largest biases arise near the peaks and troughs of the observed stationary wave. In comparing Fig. 2a and b don't see this at all. In particular, I worry that focusing the discussion on these quite narrow (10 degree by 10 degree) regions can give quite an incomplete view of the nature of the biases across the S2S models. I worry that Figs. 3, 6, and 9 may be quite sensitive to these choices. At a minimum there should be some demonstration that the inferred connections between biases are not sensitive to these choices, and this should be in the manuscript, not just in the response to reviewers. It would also be very helpful to see maps of intermodel correlations in some cases (more on this below). I also wondered if the analysis might be more powerful if the focus was on amplitude and phase of the leading wavenumber components of the anomalies.

2) Connection to tropical convection

It is certainly very reasonable to hypothesize that these biases could be related to biases in tropical convection. But I again find the evidence presented to be pretty weak: I am not at all convinced that the first place a modeling group should turn to to correct these errors is the tropical mean convection. In part the correlations are relatively weak. Moreover, this is again based on correlations of very small regions. One way to make this connection more convincing may be to show inter-model correlation maps of omega versus geopotential height biases. This would indicate whether the biases have a teleconnection pattern.

Another question I had on reading this text was the time scale on which the tropical convection biases arise. The stationary wave field biases take a few weeks to develop. Is this the same for the tropical convection? If so, how do we know that the stationary wave field might not be impacting tropical convection? If not, what sets the timescale for the extratropics to respond, and can one see evidence for this?

3) Connection between stratospheric bias and stratospheric resolution

This is a simple request (hopefully), but it takes a lot of effort to determine which symbol in a given plot corresponds to which model, and in particular, which symbol corresponds to a high resolution vs low resolution model. It would help to have a different kind of symbol for models in these categories; in particular this seems more useful than distinguishing model versions from individual models.

A closely related question: Is the wave two component of low-resolution models in better agreement with observations than those of high-resolution models, or is it just that the wave one biases dominate in these cases?

4) Importance of outliers

In many of the inter-model correlation plots there are one or two models that are to some extent outliers and in some cases seem to be determining the overall correlation (at a quick glance: Fig 6d,e,g; 9b). Some discussion should be included about the sensitivity of these correlations to such outliers.

Further questions/comments

1) The authors choose to stratify forecasts by model version in some cases as a result of updates to the forecast model over the course of the S2S project. Is there any evidence that these biases depend on model version and not just on sampling errors due to the different time periods? My impression from some single model studies was that the difference was fairly small (I could not easily find a reference for this). In any case, if this is clear it should be presented to justify the extra stratification; if not I would think it better not to stratify the results in this way (?)

2) Figure 7 is quite interesting in that it suggests some connection between the stationary wave biases and the zonal mean state. One point of clarification - are the heat fluxes from the stationary component alone?

This is important in that it provides a connection between these biases and other mean-state biases that could be of strong importance for accurately capturing the impact of the stratosphere on forecast skill, for instance. There are some interesting relationships - for instance, the heat flux forecasts of JMA seem to be about right, whereas the zonal mean wind speeds seem to systematically decay. Also, heat flux biases in the CMA forecasts are larger than those in the ISAC model, but the zonal mean state of the latter seems to diverge more quickly.

Can the authors comment on the relative role of dynamical and radiative processes in

determining the mean bias?

3) Can the authors comment in the consequences of these biases? Do they correlate with forecast skill in any way?