

Interactive comment on “A Double ITCZ Phenomenology of Wind Errors in the Equatorial Atlantic in Seasonal Forecasts with ECMWF Models” by Jonathan K. P. et al.

Anonymous Referee #1

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General comments

The study investigates the double-ITCZ problem in the Atlantic basin with an emphasis on wind errors. The investigation uses the hindcasts by two coupled models and an SST-forced GCM. The analysis starts with an overview of model biases and then focuses on the double-ITCZ problem during the boreal spring. After comparing model biases among the hindcasts, the study further highlights the transition of wind biases. The discussion is followed by an attempt to link the double-ITCZ problem to other biases in the hindcasts.

Overall, the study is clearly organized and provides adequate information that helps

C1

interpret the analysis. However, the discussion of some results is incomplete and does not fully consider all the facts. These issues make some statements appear assertive or over speculative. I encourage the authors to consider the following comments and revise the manuscript accordingly.

Specific Comments

1. The attribution the double-ITCZ problem to the cross-equatorial flow is highly speculative (Line 19-21 on Page 1). Most parts of the study focus on zonal wind biases. In contrast, the cross-equatorial flow was only briefly mentioned in Section 5. Furthermore, the evidence there does not clearly suggest whether the bias of the cross-equatorial flow is a cause or a symptom, even though causality was indicated in the abstract. The ambiguity becomes more troubling if one notices that some alternative possibilities were not fully accounted for. Two apparent examples are as follows.

a) SST biases. The study discusses the potential impact of the SST biases at the equator but not those near 10N (Fig. 4). The SST biases, which develop with the easterly wind bias, may also affect the low-level wind and precipitation (e.g., Lindzen and Nigam 1987). Do they indicate any potentially important model errors? Would these biases contribute to the double-ITCZ problem (c.f. Line 5-9 on Page 12)? Why?

b) Land impact. The argument against the land impact on precipitation (Starting from Line 16 on Page 16) omitted the apparent bias along the equatorial coast between 35W and 50W (Fig. 7). While the observation has the heaviest coastal precipitation over land, the model simulations tend to have more intense precipitation offshore. Would these factors contribute to excessive oceanic precipitation in the southern hemisphere and worsen the double-ITCZ problem?

Please consider clarifying these issues and use caution when making statements.

- Lindzen, R.S. and Nigam, S., 1987. On the role of sea surface temperature gradients in forcing low-level winds and convergence in the tropics. *Journal of the Atmospheric*

C2

Sciences, 44(17), pp.2418-2436.

2. Table 1 is helpful. But what does “Time Step” represent? The value (45 min) appears unusually large for atmospheric models with about 1-degree resolution. The table may also include the analysis periods.

3. Fig. 2a is not exactly a latitude-time plot. Please consider updating the caption.

4. Line 22 on Page 12: Could the initial easterly wind biases be related to the shock of initialization (e.g., Mulholland et al 2015; Pohlmann et al. 2017)? The problem appears common when model components are initialized separately and contain some imbalance.

- Mulholland, D.P., Laloyaux, P., Haines, K. and Balmaseda, M.A., 2015. Origin and impact of initialization shocks in coupled atmosphere–ocean forecasts. *Monthly Weather Review*, 143(11), pp.4631-4644. - Pohlmann, H., Kröger, J., Greatbatch, R.J. and Müller, W.A., 2017. Initialization shock in decadal hindcasts due to errors in wind stress over the tropical Pacific. *Climate Dynamics*, 49(7-8), pp.2685-2693.

5. Fig. 7 suggests that the S4A has severe problems in representing the zonal distribution of precipitation, even though its double-ITCZ problem is less severe. This may warrant a comment.

6. Line 16 on Page 19: It would be helpful to briefly review the mechanism. The physical reasoning that connects the boundary layer depth and the cross-equator flow is not clear here.

7. Line 8-11 on Page 16: Is there a figure that supports the statement on the equatorial waves?

8. In the last section, please consider commenting on if the findings are likely model-specific and how the findings would benefit the broader community.

Technical comments

C3

1. Line 27 on Page 19: A missing article word in “draw firm conclusion”?

2. Many sentences appear excessively long. While they are grammatically correct, longer sentences are generally harder to follow when compared to shorter ones.

3. The title of Section 3.3 (“Focussing on the Transition Period”) is not particularly informative.

4. Should the acronym (ITCZ) be explicitly defined in the title and/or the abstract?

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C4