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Dr. Paul Young
Editor,
Atmospheric Chemistry and Physics

February 1, 2020

re: manuscript number: acp-2019-625 Title: "Description and Evaluation of the Specified-Dynamics Experiment in the Chemistry-Climate Model Initiative (CCMI)"

Dear Dr. Young,

We thank you for your consideration of our responses to the reviews of our manuscript. In addition to the technical edits that you have indicated that we correct (please see the next page), we have also incorporated the feedback from the new reviewer in the most recently updated draft. As in the previous round of revisions we have provided two versions of the revised manuscript, one of which includes the corrections highlighted in red. We hope that the manuscript is now acceptable for publication in ACP. I confirm that my coauthors, David Plummer, Darryn W. Waugh, Huang Yang, Patrick Jockel, Douglas E. Kinnison, Beatrice Josse, Virginie Marecal, Makoto Deushi, Nathan Abraham, Alexander Archibald, Martyn Chipperfield, Sandip Dhomse, Wuhu Feng and Slimane Bekki concur with the submission of our manuscript in its revised form. The revised version of the manuscript has been resubmitted electronically.

Yours sincerely,

A handwritten signature in black ink, appearing to read 'Clara Orbe', with a stylized initial 'C' and 'O'.

Dr. Clara Orbe

Response to Technical Comments from Editor

We thank the Editor for providing the list of technical comments which have now been corrected in the most updated version of the manuscript.

Our response to the comments raised are as follows:

1. *"P5, L22: Should the "e.g." be "i.e.", or removed? In any case, please least all models that this applies to."*

Good point – we have replaced "e.g." with "i.e." as the stated list of models does include all models to which this applies. Please see the revised text.

2. *"P18, L28: her -> here"*

Thank you for catching this typo. This has been fixed.

3. *"P20, L15: Should HADLEY be Hadley (as in the Centre)? If so, please provide a reference as well."*

Our apologies for this confusing reference. We should have specified that HadISST SSTs and SICs are used. Please see the revised text (note that HadISST is referenced earlier on in the manuscript so that a new reference is not needed).

4. *"P20, L15: begin -> being"*

Thank you for catching this typo. This has been fixed.

Response to Reviewer 4

We thank the referee for his/her suggested edits to the manuscript. Incorporation of this feedback into the new revised manuscript has improved the clarity and readability of the study.

Our response to the comments raised are as follows:

1. *“60S-60N averaging: if your motivation is to focus the analyses on the tropics and mid-latitudes then why not point this out to the reader (I may not be the only one who gets confused about the wording of that sentence, which seems to only refer to problems near the poles – quite a different argument”*

We thank for the reviewer for her/his comment. We have rephrased this passage by making explicit reference to the fact – as requested by the reviewer – that we are only interested in latitudes equatorward of 60°S and 60°N. The new passage in the most updated draft now reads as follows:

“In order to focus our analysis on the tropics and midlatitudes we restrict our analysis of temporal variability to spatial averages performed over latitudes between 60°S and 60°N, with the exception of the vertical velocities v^* and w^* . For the latter variables, which change sign from positive to negative in the subtropics in both the troposphere and stratosphere, we perform averages over 30°S and 30°N. Our exclusion of latitudes outside the range 60°S-60°N is in order to avoid emphasizing the poles, where differences among the simulations may reflect large sensitivities to a few grid points and/or numerical instabilities.”

2. *“interannual variability: you could remark that while not specifically analyzing for ENSO, QBO, these sources of variability may likely still dominant the signal (true?)”*

We think that this point (i.e. mentioning the explicit contribution of ENSO and QBO to interannual variations in both dynamical and transport quantities) is already clear in the text. Please see line 16 on page 6:

“As such, our assessment of interannual variability, which evaluates only the degree of correlation between timeseries, differs from previous studies (Chrysanthou et al. (2019)), in which timeseries were further decomposed in terms of different modes of interannual variability (i.e. the El-Nino Southern Oscillation, the Quasi-Biennial Oscillation, etc.).”

Therefore, no changes to the text have been made as it is not clear to us why this point needs to be made again in the manuscript. Doing so would be redundant.

3. *“MERRA: which one, MERRA-1 or MERRA-2? Please clarify in paper”*

Well, there is no official “MERRA-1” per the notation suggested by the reviewer. The products are, simply, MERRA and MERRA-2. Although we feel that we have been clear that we are using

the first version of MERRA via the reference to Reinecker et al. (2011) we have, per the reviewer's request, made this still clearer through the following changes to that section:

"...both constrained with MERRA meteorological fields (Reinecker et al. (2011)) (not MERRA-2 (Gelaro et al. (2017)))."

Note that the new Gelaro et al. (2017) citation has also been included in the references.