

***Interactive comment on* “Description and Evaluation of the Specified-Dynamics Experiment in the Chemistry-Climate Model Initiative (CCMI)”** **by Clara Orbe et al.**

Anonymous Referee #1

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The study by Orbe et al evaluates the performance of the so-called "specified dynamics" simulations performed within the CCMI1 project in terms of tropospheric and stratospheric large-scale circulation as well as stratospheric transport tracers. Further, the results are contrasted with the free-running simulations with the same set of models. The study shows that while zonal winds and temperature climatologies are generally well constrained by "nudging", the meridional and vertical component of the circulation are not. While all variables are constrained in terms of their interannual variability and (less so) seasonal cycle phasing, the seasonal cycle amplitude is less constrained. Another important result is that the SD simulations are no longer dynamically consistent, which can be expected due to the addition of the non-physical nudging term. Over-

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all, the study is an important contribution to the CCMI evaluation papers, and more so an overdue clarification on the effects of nudging in CCMs. Specifically, it clarifies that "nudging" of the large-scale flow does not necessarily lead to a reduction in the spread even in the large-scale flow itself, and therefore even less so in transport and tracers. The paper can be improved in terms of the presentation and writing, but will be an important contribution after thoroughly revising the text. Furthermore, I have a few suggestions that could emphasize the points of the paper even more.

General comments:

1. Trends: There is no mentioning of long-term trends in the paper. It is found that interannual variability in the SD simulations is rather consistent, and thus it is concluded correctly that the SD simulations are most justified to be used for studies of interannual variability (page 17, lines 6ff). I strongly recommend to add a word of caution here that the agreement of interannual variability very likely will not transfer to long-term trends. Even more so, I suggest to the authors to add a small section that specifically analyses the trends; while this might seem like opening another "can of worms", in my opinion this could be done in a rather short way: If you just calculate the trends of the time-series presented in 7 and 8 and present them in one additional Figure, you clarify how well the trend is constrained (or not). Of course comparison to the FR simulations (and underlying reanalysis were appropriate) would be very valuable here as well. On a similar note, as stated by the authors the time-series were not de-trended before the interannual corrections were calculated, which might obscure the rating of the interannual variability. I recommend to detrend the timeseries for the correlations of interannual variability, and regard the trends separately as suggested above. As long-term trends are the subject of many studies (and you even mention "past trends" in the 2nd sentence of the abstract), and often the naive idea that nudged simulations should get the observed trend right is still around, I think this would be a very important addition to the paper.

2. Reanalysis data of TEM circulation: Is there a particular reason why the anal-

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ysis of v^* and w^* is not contrasted to the respective data from the reanalysis? While I understand that calculating those diagnostics is a large effort, they are available from the "SRIP" dataset (see <https://www.earth-syst-sci-data.net/10/1925/2018/> and <https://catalogue.ceda.ac.uk/uuid/b241a7f536a244749662360bd7839312>). If you choose not to include the data, please give a short explanations for the reasons in the paper.

Specific comment:

Abstract: The abstract emphasizes the differences in the SD simulations strongly, but does not mention that the interannual variability is indeed constrained in the simulations. While I agree that the text should clearly state the "warning" to users of SD simulations, it would be fair to also mention the positive outcomes of this study. On the other hand, I think it would be good to mention the results on the dynamical inconsistency in the Abstract, as I think this is a major result to keep in mind when working with SD simulations: the dynamics are not only not well constrained, but they are actually internally consistent (which makes sense as a non-physical term is added to the budgets). This important finding should also be mentioned in the conclusions.

page 3, line 26: I find the way you introduce the possible reasons for the SD differences a little confusing. Why not list all three points first, and then mention that in the following with "implementation differences" you refer to the named two points (and give a reason why you group them - presumably because it is hard to quantify their relative roles?)

page 5, line 3-4: Could the transformation of w to ω contribute to model spread in this variable, as probably the density is approximately calculated using the given zonal mean monthly mean temperatures? What is the reasoning behind not using w ?

page 5, line 22: why is the interpolation for "tropospheric variables" performed? Because the CCM1 model levels are too coarse in the troposphere?

page 6, line 12: before mentioning ensemble member, define that the ensemble here is

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the multi-model suit of SD experiments (earlier, it was mentioned that there are multiple ensembles of the REF-C1 simulations in the conventional meaning of "ensemble", so this is somewhat confusing).

page 6, line 24, and entire section 3: In general, I find your way of using "e.g." when listing models a little confusing - this implies that you only list examples, when indeed you do list all the models with certain properties. So please just remove the "e.g." (see also page 7, line 23 and 26). Similarly, when mentioning that "few" models have a certain property, please specify the number. For example in Section 3.3 you say "most" models use HadISST, while "some" use other forcing. While I agree you do not need to list all details here, please be a bit more specific, otherwise this information is more confusing than helpful.

page 9, line 21: the listed differences occur quite close to the equator, where the variable changes its sign, does it? As those result from small shifts in the distribution, it might not be entirely fair to list those as range or difference - rather refer for example to difference in maximum values?

page 9, line 30ff: Good to see you applied the recalculation to check on the possible impact of this issue. According to the Figure in the supplement, the magnitude of differences appears to be larger to me in the recalculated w^* values? Therefore I would recommend to reformulate that statement, in that the differences are at least as high as the ones shown in Fig. 2 (or in other words, that the differences cannot be explained by differences in the calculation method of w^* , as the recalculation even emphasizes the differences.)

page 10, line 1-2: Because they are clustered by reanalysis product, correct? As you do not present the values from the reanalysis this is indirectly inferred.

page 10, line 5: I find the references to Table 3 mentioned here (and elsewhere) not very helpful. In particular in this paragraph, there is no explanation of why the respective potential reasons are thought to explain the differences. In general, I think Table

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3 could be removed, and rather the potential reasons should be mentioned/discussed where appropriate.

page 11, line 1: Isn't the spread in U850 almost as large?

page 11, line 3ff: I think here you mix up the phase and the amplitude: the spread in V300 τ_{\max} is present for all three RA (largest for MERRA, but still 3-4 months for the other RA). If you refer to the amplitude rather than phase here, this would make more sense (then the text needs to be rephrased).

Fig. 4: Please specify more clearly what the individual "dots" represent for the phase: are those the individual models? (and only few are seen in the phase plot because they lay on top of each other?). In the caption it says "show the spread...", which does not clarify what is shown.

page 11, line 17: Could the effect of small annual means amplify the differences? If one model has zero mean, its annual cycle would be infinity...?

page 12, line 16: Do you really mean "positive correlations" here, or "high"? Negative correlations would be very surprising.

page 13, line 9: could you mention the value for N here to give the reader a feeling of the ensemble size (without having to go to Table 1 and count).

Fig. 9: I really like this Figure and think it provides valuable information!

page 14, top: I also like the Figures 4 and 5 of the supplement (but agree it is too much to show them in the paper). Some additional interesting features are to be identified, e.g. for ω , it turns out that the RMS at higher latitudes is stronger for FR than for SD. Also for w^* , this Fig highlights that the RMS appears to be generally higher in the SD simulations, a result that I'm not surprised by. Mentioning of those interesting features could be worthwhile.

page 14, line 12: I'd summarize the findings in that SCA is (much) better constrained

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for T und U for the SD runs, and similar for V, but the spread in the SCA is even larger in the SD runs for omega.

page 14, line 16: "poorly (equally)" - confusing formulation, please clarify

page 15, top: Again, one interesting result that could be mentioned here is that the correlations for w^* are lower in the SD simulations in the tropics compared to FR, a result that is not as clear from Fig. 11 (where the spread is similar or lower in SD).

page 17, line 35: Do you need to make the assumption that advection scheme biases are small? I think you can rate it under "underlying free-running model" biases. So even if the circulation would be constrained perfectly (which it is not, see TEM diagnostics), one would expect the advection schemes to induced differences, but those should be smaller than in the FR simulation - but they are about as high (see Fig. 9), i.e. spread in the circulation itself must contribute.

page 18, top: I recommend to include somewhere in the paper the important result of dynamical inconsistency. Also, you can refer to Chrysantou et al. for the dynamical inconsistency of the TEM budget (the "downward control" calculation works less well in the SD runs).

Table 1: A few questions to be clarified: Are the models with non-constant nudging timescales those that give a range, and the others have constant nudging timescales? Why include information on models that are not used?

Technical:

Abstract, line 2, and later: the word "online" GCM is not well defined. I would advise not to use it in the Abstract, and if used later on it needs to be defined.

page 3, line 22: I suggest to remove "it is important to note" here, but rather just state that in addition to your paper, there is a study that focuses on the stratospheric circulation (also, the next sentence also starts with "it is important to note")

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page 3, line 30: two times "overall" in one sentence.

page 3, bottom: sections should be labels 1,2,3,... as in the text

page 4, line 17: rather introduce the model names than institutions? It is not really scientifically relevant whether the contributions are from the same institution, but whether it is the same model that contributes with different set-ups (and as I understand it, the NASA contributions are two different models). Furthermore, also other models contribute more than one set-up (e.g. EMAC L47 and L90).

page 6, line 30: add "information" after "for more"

page 9, line 11 and general: mbar or hPa? There is a mixture, and as this an European journal you might just want to use hPa.

page 11, line 3: please put for example V300 in brackets after "the latter", otherwise hard to follow for the reader.

page 11, line 9: again, please put e.g. meridional winds in brackets after "the former"

page 15, line 5: remove "e.g." (?)

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-625>, 2019.

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