Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-238-RC1, 2019

Geoscientific Model Development Discussions

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Interactive comment on "A Comprehensive Assessment of Tropical Stratospheric Upwelling in Specified Dynamics CESM1.2.2 (WACCM)" *by* Nicholas A. Davis et al.

Anonymous Referee #1

Received and published: 28 October 2019

Overall Comments: In this study the authors explore a broad range of specified-dynamics (SD) simulations in which WACCM is nudged to MERRA-2 meteorological fields in an attempt to quantify and understand the extent to which such SD simulations can reproduce upwelling trends in the underlying reanalysis. Given its implications for, among others, recent investigations into lower stratospheric ozone trends this study is very relevant. It is also an admirable attempt to understand in detail the mechanics of nudging, delving nicely into the momentum balance of upwelling and discrepancies that may arise in these balances among different nudged runs. In this sense the study does stand out as an attempt to address with more rigor than is standard the ways in which nudging can produce non-intuitive trends/variability/etc. For these reasons I

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recommend that this paper be accepted with minor revisions. However, there are still several key points that must be addressed. As these are more directed at the delivery and presentation of the results, and not related to fundamental problems that I have with the paper, I have not recommended "major revisions." Nonetheless, they need to be addressed.

My major comments are as follows:

Main Point 1: Throughout the authors argue that it is most desirable to preserve WACCM's free-running climatology (e.g. see discussion at the top of page 8, and various other places). Since this is a not a standard goal of nudging this needs to be better justified. In particular, I find the justification in lines 88-92 unsatisfying. Why is it bad that WACCM-SD reproduce the tropopause (or, more generally, temperature) structure of MERRA-2? Even if that impacts transport isn't that the point? What I would understand is if the authors argued that doing so creates dynamical inconsistencies in the circulation (assuming either the nudging tendency is large enough that it implies spurious vertical velocity analogous to the situation presented in Weaver et al. (1997)). Is this what the authors mean? Given that the use of nudging to climatological means is a central component of this work (and not conventional) I think this needs to be much better explained.

Weaver, Clark J., Anne R. Douglass, and Richard B. Rood. "Thermodynamic balance of three-dimensional stratospheric winds derived from a data assimilation procedure." Journal of the atmospheric sciences 50, no. 17 (1993): 2987-2993.

Main Point 2: I think the authors need to be much more cautious in generalizing the result that zonal mean temperature nudging should not be applied. As the conclusions read (especially point 1 spanning lines 400-404) the authors seem to suggest that this is a general result. However, given that the zonal mean temperature nudging trends are failing to reproduce MERRA-2 through their effects on eddies (via discrepancies in the meridional heat fluxes) I'm highly suspect that other nudging frameworks using

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different models (with different balances of resolved vs. parameterized momentum forcing of upwelling) will automatically corroborate these findings. In short, I think the authors need to state clearly how this conclusion depends very specifically on the particular way in which the momentum forcing in WACCM is driving w* and how that may depend on horizontal/vertical resolution and other factors. Of course I notice that line 408-409 seems to direct these questions to future work but this is a bit unsatisfying. If the authors do not wish to do any test simulations (at higher horizontal resolution, for example) they should at minimum be very clear that these results are not likely to be generalizable to other nudging frameworks.

Minor Comments:

Abstract, Ln. 22: I'm a bit confused why the goal is to "preserve WACCM's (free-running) climatology". The whole point of nudging is to draw the free-running model towards the reanalysis in as dynamically consistent a way as possible so I'm not sure why one would want to preserve a (biased) free-running climatological state. I'm sure there's a clear motivation for this but I couldn't identify one in the text (neither here nor in the sections later). See Major Comment 1.

Abstract Ln. 22: "climatological winds" -> Is that also just zonal or meridional too? Ln. 40: What does "the quality of the meteorological data" mean? Please specify.

Ln. 71: Does nudging occur everywhere?

Line 88: This wouldn't happen, though, if one were to nudge "hard" to T (using, for example, a relaxation timescale of a few days, not 50 days). I'm not sure I really understand the point here. Sure, it would change WACCM's tropopause (and other fields) but why is that necessarily a bad thing? Clearly, this would not be good if it were done in such a way that violated dynamical balance but that is more likely to be an artifact of the nudging machinery. What is fundamentally wrong about nudging to the full time-varying reanalysis field?

Line 99: You write that three-hourly MERRA-2 input is used in Line 70 but six-hourly here. Which is it? If six-hourly why was the decision made to coarsen the resolution temporally?

Line 104: Again, can you please justify what you mean by "climatological anomaly nudging scheme is in theory..."? If the nudging was perfect (i.e. converged to assimilation) then it's not obvious to me that there's any fundamental problem with nudging to the full time-varying field.

Line 121: I am assuming other more standard tests have been done (i.e. vertical profile of nudging? changes in nudging timescale?). If so, it should be clarified that these have been done and they have not produced any satisfying simulation in which w* reproduces w* in the underlying reanalysis (here MERRA-2).

Line 168: How did you calculate this from MERRA-2 (as shown in future figures?)? Where did you get all of the components (specifically the subgrid-scale wave momentum forcing)? And which product did you use? You indicated the third hourly fields initially but were six-hourly used here?

Lines 184-193: Again, I am confused. Don't you want to reproduce MERRA-2? See earlier comments. Figure 2 caption: The hatching definition is strange. Per the colorbar definition white contours in all panels should indicate regions where there is upwelling 100% of the time (i.e. fraction of 1). Why doesn't all hatching align with white?

Line 198: Is this frequency calculated daily/monthly/etc. Does the temporal sampling used to evaluate this measure matter?

Line 200: Are you taking w* directly from MERRA-2 or calculating offline in a consistent fashion as for the WACCM simulations? This relates to my earlier question about MERRA-2 mass flux estimates. How exactly are all measures derived from the MERRA-2 output?

Line 201: What if you just compare climatological annual mean w* between WACCM and MERRA-2? That's more standard – does that show the same sort of difference (i.e. w* smaller in MERRA-2)? I find this "split" in upwelling frequency in MERRA-2 curious only because it doesn't appear to manifest in the climatology of w* (see Figure 10-3 in Bosilovich et al. (2015)). Note that in MERRA this region of anomalous downwelling was present but it was corrected in MERRA-2. This seems to be at odds with what the current study is showing. Can the authors explain this discrepancy? The easiest thing to do would be just to plot the climatology and see if you can reproduce the aformentioned figure...

Bosilovich, M. G. (2015). MERRA-2: Initial evaluation of the climate. National Aeronautics and Space Administration, Goddard Space Flight Center. Available at https://gmao.gsfc.nasa.gov/pubs/docs/Bosilovich803.pdf

Lines 248:251: So this is a really important conclusion – the lack of any convergence of the trends to MERRA-2 in Figure 4 is striking (and frustrating!). This is a merely a comment that I like this figure. Line 280: Indeed. Hence, why is this the primary goal of the paper? Again, more justification needed. See earlier comments.

Line 313: Given the larger role played by the (parameterized) GWD in contributing to upwelling trends in WACCM does this imply that your conclusions will depend largely on horizontal and vertical resolution? One would think that as more of the waves contributing to w* are resolved then the disparities with MERRA-2 (in terms of the physical mechanisms forcing the trends) will get smaller. Have you looked at SD simulations at different horizontal resolutions?

Line 357: You can see that enhanced wave propagation clearly in the AMIP run but not so clearly in the AMIPQBO run (no evidence in NH extratropics)...please check.

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