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Geoscientific Model Development Discussions

Interactive comment on "Quantifying and attributing time step sensitivities in present-day climate simulations conducted with EAMv1" *by* Hui Wan et al.

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Referee comment: This paper addresses the important and until-now under studied impact of changing time-stepping and physics-dynamics coupling methodologies within numerical models of the atmosphere. In this work, the Energy Exascale Earth System Model is used to investigate the impact of a factor of 6 shortening in the time step for 10-year duration simulation of the present (year 2000) climate. The shortening of the time step reduces errors associated with truncation errors in time and also allows a tighter coupling of the physical processes which are parameterized separately within the model (such as dynamics, clouds, convection, and radiation). These changes have

a small to moderate impact on the overall radiative balance of the model resulting in a warming of the lower troposphere (+0.5K), cooling of the upper troposphere/lower stratosphere (- 0.5K), a decrease in low level relative humidity (1%, locally up to 10%) and a reduction of low level clouds, particluarly in subtropical stratocumulus & shallow-cumulus regions. These changes were determined to result from local thermodynamics changes rather than changed to the circulation.

The authors then further investigate which part of the coupling is responsible for these changes through a series of experiments where only a subset of the model's parameterizations use a shortened timestep. The conclusion of these experiments is that differences come from the calculation of the cloud marcophysics+microphysics, but are not caused by sensitivities within the cloud microphysics+microphysics calculations; rather they are caused by changes to the input values to the cloud macrophysics+microphysics parameterization, which result from changing time step length in other parts of the model physics (e.g. larger temperature increments due to longer radiation or dynamics time steps). In an attempt to study this effect, they briefly introduce the "dribble" method of substepping the cloud microphysics with a shorter time step and providing partial updates of the model prognostic variables to each substep based on their change since the last time step.

I find the study to be well constructed, the paper well written and the figures generally clear and justifying inclusion in the paper. I find the results convincing and have no significant concerns regarding the method nor the scientific conclusions of the paper. However, I have a few suggestions for making the paper clearer and improving the clarity of the messages within it. In my opinion the paper is already worthy of publication and my suggestions are intended to improve the clarity of some parts of the paper.

Author response: We greatly appreciate Dr. Barrett's very positive assessment and insightful review. Our responses to his suggestions are detailed below.

Referee comment: 1) I suggest including quantification of the changes to temperature,

RH and clouds in both the abstract and conclusions so that the scale of the time step sensitivity is easily findable.

Author response: The suggested changes are included in the revised manuscript.

Referee comment: 2) I do not see the value of including information about EAMv0 model output in the paper. The authors should think about whether it is really important to include the description of this model version and the changes in biases between v0 and v1. I didn't find that this information added (or detracted) from my understanding of the time step sensitivity.

Author response: We agree the v0 results do not provide much help in understanding time step sensitivity in v1, as the parameterizations and the vertical resolution in the two model versions are both substantially different. We chose to, and still would like to, keep the v0 results in the manuscript because this little part is intended for a slightly different audience, namely colleagues who have been developing, tuning, and evaluating EAM with a focus on its fidelity in reproducing the observed climate but who might have not thought much about time step sensitivity before reading our manuscript. The v0 results presented here are intended to provide these colleagues with a quantitative assessment of the relative magnitude of the time step sensitivity compared with the changes in model biases when EAM was evolved from v0 to v1.

Referee comment: 3) I found the schematic figure 2 incredibly valuable - showing which sets of simulations I should compare to see what effect. However, the text-based descriptions of the different time-stepping (coupling) strategies I found confusing and made my own notes/sketches when reading the paper. Only at the end of reading the paper did I find the schematics in figures A1 & A2. In this case the figures are much easier to compare than long sections of text and I would therefore suggest adding more explicit references to the explanatory figures at the beginning of each block of text, rather than at the end. (a specific example: more the reference to Figure A2b from line 322 to line 314 (perhaps moving the whole sentence)).

Author response: We are glad to hear Dr. Barrett's appreciation of the value of the schematics, and we agree that these are more straightforward to read than the text. Following Dr. Barrett's suggestion and example, we have revised the manuscript and moved references to the schematics to the beginning of each block of text description. We also moved schematics that were originally in the Appendix (i.e., Figures A1 and A2) to the main body of the revised manuscript so that they are placed closer to the text descriptions of the corresponding simulations.

Referee comment: 4) The sections 4.3.2 (dribble method) and 4.3.3 (shorter tau in convective param.) are confusing to me. The method is introduced, the results are very briefly summarized and a summary sentence is reached. However, I do not follow which figures should be compared to reach the same conclusions as the authors. Furthermore, the authors do not include any information from these two subsections in their conclusions and therefore I think the meaning of these sections is lost. Either these sections (and the conclusions) should be expanded to include the logic and comparison of the relevant figures, or could be omitted from the paper completely without losing impact (as they are currently difficult to understand - at least by me). You already mention that an additional paper is being prepared about the dribble method, so perhaps that would be the place for a more expansive discussion.

Author response: Sections 4.3.2 and 4.3.3 are indeed the most non-straightforward parts of the manuscript.

We imagine colleagues in the CESM and E3SM communities whose work has been influenced by the paper of Williamson (2013) – "The effect of time steps and timeâĂŘscales on parametrization suites" (DOI: 10.1002/qj.1992) – would find the results in section 4.3.3 intriguing, while researchers who do not work with these two models might not feel a strong motivation to investigate the impact of the convection scheme's built-in time scale "tau". Having considered comments on Section 4.3.3 from both referees, we choose to move most of the contents in that section to the appendix, leaving only a brief discussion pointing out that the ratio of $\Delta t/\tau$ alone cannot explain the time step

sensitivity of convective activities in our model.

We prefer to leave the short discussion on the “dribbling” experiment in the revised manuscript because comparing v1_Dribble with the simulations v1_CPL+DeepCu_Shorter and v1_CTRL discussed earlier in the manuscript reveals the previously unknown results that the frequency of coupling between the stratiform cloud subcycles and the rest of EAMv1 is the primary reason for cloud-related sensitivities in the subtropical marine stratocumulus regions while the step sizes used for deep convection and/or its coupling with other processes have significant impact in the trade cumulus regions and along the equator (shown by Figure 13 in the original manuscript). These helps to answer the “what step sizes caused what changes” question and hence address the attribution theme of this study, although the question “why does the model behave this way” still needs to be answered by follow-up papers. Nevertheless, to address Dr. Barrett’s comment, we have made revisions in Section 4.3.2 and the related figures and captions to better guide the readers through our reasoning.

Referee comment: 5) The authors state that the cause of the time step sensitivity coming from outside, rather than within, the subcycled physics is “counter-intuitive" (abstract and line 279). However, this is exactly the mechanism for time step sensitivity that I reported in Barrett et al. (2019) from cloud-resolving model simulations (with time step ranges from 1-15 seconds). I agree that it was initially counter intuitive, but is related to the requirement for the cloud parameterizations to react to a large push away from equilibrium by condensation (resulting from cooling imposed by the dynamics and radiation tendencies). I therefore suggest:

5.1) Adding a sentence that this is potentially caused by condensation/cloud formation in the model and the sensitivity does not get removed even with time steps of only a few seconds (which are not achievable in climate simulations), strengthening the argument for the importance of finding a good numerical solution to this problem (perhaps dribbling...)

Author response: We removed the word “counter-intuitive" in the abstract and at line 279 of the original manuscript, as whether a model developer or user would find those results counter-intuitive would probably depend strongly on their prior research experience.

Regarding the observed strong sensitivity of simulated clouds to step sizes outside the subcycles, our recent follow-up work has suggested that the model sensitivities over the mid-latitude storm tracks indeed are closely related to the strong condensation the referee pointed out, while the sensitivities in the subtropical marine stratocumulus regions are more closely related to the positive feedback between cloud liquid amount and cloud-top radiative cooling. We will describe these in detail in the next manuscript.

Referee comment: 5.2) Including information in the model description about which part of the model diagnoses the cloud fraction and condensation (i.e. whether this occurs inside or outside of the microphysics+macrophysics and how it is treated within the subcycling setups)

Author response: A clarification is added to the model description part (Section 2.1) that CLUBB diagnoses cloud fraction and effectively does the large-scale condensation calculation using the predicted sub-grid probability distribution of heat, water, and vertical velocity, meaning that the condensation and cloud fraction calculations are done at intervals of Δt_{macmic}= 5 min.

Referee comment:
Typos found:
Line 90: this *is* tied to
Line 440: The mechanisms *under* → *behind*

Author response: Both have been corrected. Thanks for pointing them out.

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