Responses to Reviewer #2

This manuscript presents results of the coupling of the CAM5 physics parameterization to a single column and simplified 3D dynamical model (SGRIST and GRISTCAM5phys, respectively). The main result of the paper is that both SGRIST and GRIST-CAM5phys closely match the output of the CAM5 single column model (SCAM) and CAM5 with finite volume dycore (CAM5-FV). Generally minor differences in simulation outcomes are noted and are attributed to differences in numerics (e.g., time integration method and physics-dynamics coupling) without much in-depth exploration. While the paper presents minimal novel scientific results, it does show technical advances in keeping with many of the authors' other recent publications and demonstrates the feasibility of the SGRIST and GRIST-CAM5phys model configurations under idealized conditions. I believe the paper would be easier to digest if some of the previous results simply cited (i.e., details of the ptend coupling strategies) were described here, even if briefly. Overall, I have a few general comments and numerous minor/typographical comments, thus I recommend the authors attend to these concerns before the paper is accepted. The suggested revisions are uniformly minor in scope. In the remainder of the review, references to specific passages are accompanied by a line number (e.g., L134 refers to line 134 of the manuscript).

Response: We are grateful to the reviewer for the detailed and constructive comments and suggestion (in blue text). Substantial revision has been made based on the comments, especially the SCM simulation section. Another two SCM test cases, the GATE III and CGILS, are added to further demonstrate the usefulness of SGRIST1.0 in physics-dynamics coupling, and to evaluate the CAM5 physics package in different model frameworks. The LES output is added as reference to evaluate the SCM simulations. For the 3D model, the ptend coupling strategies are described briefly as follows (and in the revised manuscript):

Ptend_f2_sudden is a pure operator splitting approach, which is also used in CAM5-FV. The tendencies of all prognostic variables from the physical parameterizations are multiplied by the model time steps to suddenly update the atmospheric state before the next step of dynamical integration. Ptend_f1_f1 is also based on operator splitting, but incrementally adds the physics tendencies to dycore and tracer transport after each step of their sub-cycled integration, respectively. Within one model time step, the physics tendencies are applied as a constant source term in the dynamical (dycore+tracer) sub-steps. The subtle difference between the ptend_f1_f1 and the dribbling strategy (se_ftype0, Thatcher and Jablonowski, 2016; Zhang et al., 2018) used in CAM5-SE is that ptend_f1_f1 treats dycore and passive tracer transport as two separate components, while the dribbling approach treats them as a whole.

General comments

- Why have you chosen CAM5 physics to start with? It has been demonstrated quite clearly over the last 5 years or so that CAM6 physics with a unified turbulence and macrophysics scheme (CLUBB) produces superior results to the disjoint combination of separate moist turbulence, shallow convection and macrophysics schemes. This is not necessarily an indictment of your approach; I simply want to understand the rationale.

Response: We agree with this Reviewer that CAM6 physics may be superior than CAM5. There are two reasons to start with CAM5 physics: (i) for climate modeling, our earlier experience is more closely related with CAM5, which has many known model simulations to be compared with, and known behaviors that can be verified; so this is a good starting point; (ii) CAM5 physics suite is widely used and modified, which allow some development efforts involved in a domestic project to be transferred to this suite (e.g., Qin et al., 2018; Chu and Lin, 2021; Zhao et al., 2021).

We also wish to mention that GRIST (both the SCM and 3D GCM) support different full-physics packages (see Zhou et al. 2020 for description). Using CAM5 physics is a good example to demonstrate such a flexibility, but it does not exclude any possibility to add new packages in future (e.g., CAM6).

- I'm not convinced that you actually explore any of the "additional uncertainties" (L65) introduced by the physics package. You appear to just plug it in and go – there's no discussion of how the physics package was changed for this implementation, or any assessment of parameter uncertainty, etc. In fact, the goal of this paper seems to be to reproduce as closely as possible the CAM5 solutions. So what "uncertainties" are you addressing?

Response: We have added some discussion in the manuscript to demonstrate some possible uncertainty in the current GRIST-

CAM5-physics suite. There are two major uncertainties: (i) uncertainty of implementation; (ii) uncertainty of using a physics suite across different modeling systems because it has not been tuned in the new system.

Yes. As a technical work to demonstrate the usefulness of a SCM in transferring a full-physics suite to a new modeling system, the first goal is indeed to produce closely comparable simulations to the best that we can. This is especially important for CAM5-physics because the suite is sophisticated and has complicated internal data flow. The parameterization forcing is in most parts inherited from CAM5, however, some structural modifications are made to couple with GRIST. As with many other physics-transfer work across different modeling systems (e.g., Giorgetta et al. 2018, ECHAM physics to ICON-A), no proof can be given that the implementation is free of errors, bugs or inconsistency. The comparison of SCM test can confirm that if there is still remaining inconsistency between GRIST's CAM5-physics and that in CAM5, the resultant consequence is limited.

- Results shown in Figures 2b-c, 4 and 5 compare SGRIST and SCAM with no external reference to gauge whether SGRIST represents improvement, degradation or persistence compared with SCAM. Reference LES solutions should be available for all the SCM cases, and I recommend the authors include such output so that statements such as that given in L212-214 ("… are in good agreement with the [LES] in Park and Bretherton [2009]") can be directly evaluated.

Response: We have added the LES solution for DYCOMS-RF02 in Figure 4 of the revised manuscript. The LES data is from the DHARMA model (Ackerman et al., 2009; available at: http://gcss-dime.giss.nasa.gov). The maximum cloud fraction and cloud liquid amount for SGRIST1.0 is in good agreement with the LES, but the cloud is slightly thicker. The rain mixing ratio with *dt* = 600 and 1200 s also agrees well with the LES, which is less than that in SCAM at the corresponding time steps.

We are not able to get the LES data for BOMEX. However, the SCM solutions can be compared with the LES shown in Siebesma et al. (2003) and Park and Bretherton (2009). Figure R1 (also given as supplement) shows the comparison between SGRIST1.0 and LES for the BOMEX case. The cloud fraction and cloud liquid amount for the LES are cited from the Figure 3 and 6 in Siebesma et al. (2003), and the cumulus cloud fraction and updraft mass flux are cited from the Figure 2 in Park and Bretherton (2009). The maximum (cumulus) cloud fraction and updraft mass flux with each time step for SGRIST1.0 are higher than the LES. The maximum cloud liquid amount with *dt* = 600 s is closer to the LES than *dt* = 1200 and 2400 s simulations.

For the tropical convection cases, the time average cloud fraction derived from the observation (Xie et al., 2010) is added in Figure 1 b for evaluation of the TWP-ICE case; and the SCM simulations for GATE III are compared with the Giga-LES (Kharioutdinov et al., 2009). The Giga-LES is a 24-h simulation with 100 m horizontal resolution. Figure R2 (also given as supplement) shows the time average cloud fraction for the GATE III case from the SCMs and the Giga-LES respectively. SCAM and SGRIST1.0 produce more low cloud than Giga-LES. The maximum high cloud fraction for SGRIST1.0 is close to the Giga-LES, which is about 0.1 less than that for SCAM. For the TWP-ICE case, both SCAM and SGRIST1.0 show overestimation of middle and upper-level cloud in comparison with the IOP data.

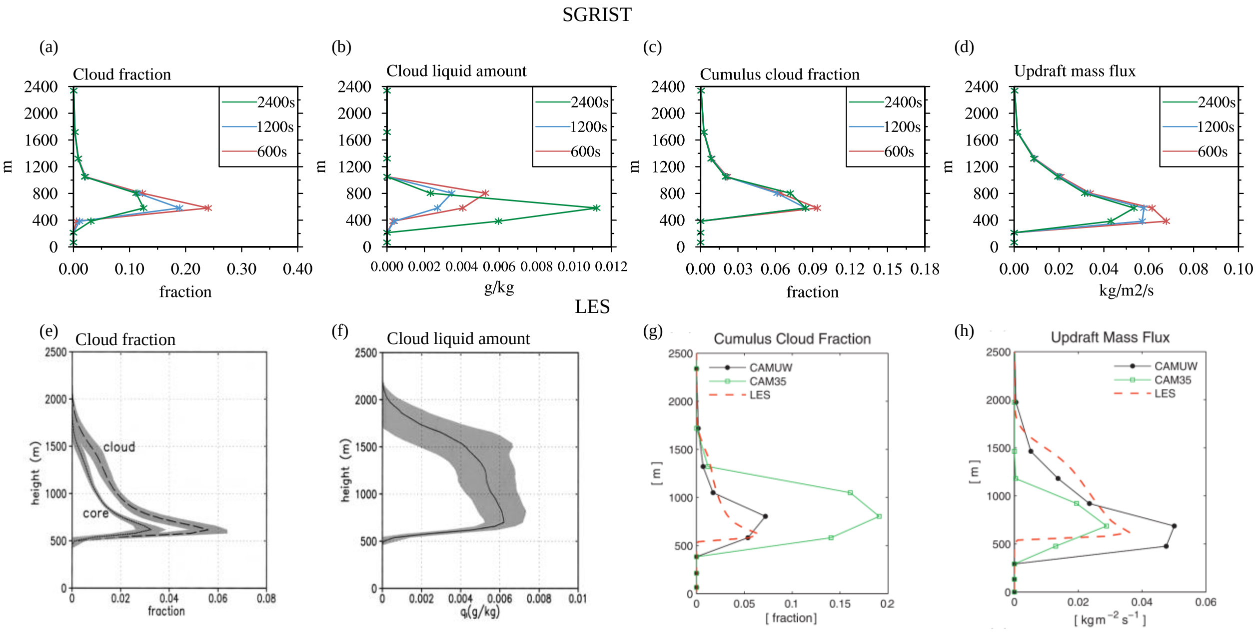

Figure R1. Comparison of cloud fraction, cloud liquid amount, g kg^{-1}, cumulus cloud fraction, and updraft mass flux, kg m^{-2} s^{-1}, for the BOMEX test case between (a-d) SGRIST1.0 and (e-h) the LES. (a-d) are the same as Figure 3 (e-h) in the revised manuscript. The LES figures are cited from Siebesma et al. (2003) and Park and Bretherton (2009). The lines in (e) and (f) show the ensemble mean of 10 LES models, and the band is a width of twice the standard deviation of the participating models. The red dashed line in (g) and (h) is the LES result of the System for Atmospheric Modeling (version 6.5).

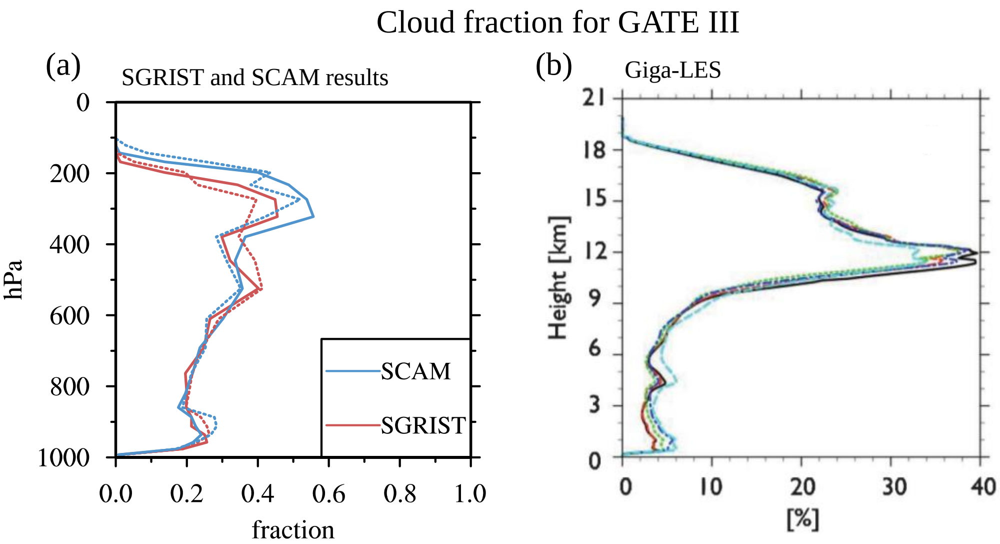

Figure R2. Time average cloud fraction for the GATE III case from (a) the SGRIST1.0 and SCAM simulations, and (b) the Giga-LES result (black solid line). The solid and dotted lines in (a) show the dt = 1200 and 2400 s simulations respectively. The Giga-LES is a 24-h run, and the last 12-hour simulation is used for analysis. (a) is the same as Figure 1 (d) in the revised manuscript, and (b) is cited from Figure 11 in Khairoutdinov et al. (2009).

- Similarly, a general statement (L241-242) is made that "micro- and macrophysics variables are sensitive to the profile of humidity, which varies with the time integration method in the dynamical component." It would be helpful to add SCAM results to Figure 3 to evaluate this. Do you mean that the grid-mean humidity profile is different in SGRIST and SCAM output? Or is this an issue that arises "internally" during a timestep? If this is indeed a consequence of intra-timestep evolution, a more in-depth analysis to explain how these differences arise is desirable. As it is currently written, this point regarding differences in time integration reads like an unjustified conjecture.

Response: We have revised the statement about the sensitivity of micro- and macrophysics to time step in the manuscript. We add the water vapor budget for the SCM cases to show the sensitivity of stratiform cloud scheme to time steps in SGRIST and SCAM (Figures 2 and 5 in the revised manuscript). The large-scale condensation (evaporation) plays an important role in the TWP-ICE and DYCOMS-RF02 cases, and the sensitivity of water vapor budget for SCAM to time step is discernable. SGRIST1.0 shows less sensitivity to time step. As we know, the horizontal advection term and the vertical velocity for a single column model are derived from the IOP data, which are the same in SCAM and SGRIST1.0. The difference between the SGRIST and SCAM dynamical cores is primarily attributable to the time integration method. SGRIST1.0 uses the third-order Runge-Kutta time integration scheme instead of the leapfrog scheme that used in SCAM. Therefore, we speculate that the sensitivity of stratiform cloud scheme to time step is highly coupled to the time integration of the dynamical core.

However, we have not figured out the specific mechanism that is involved in this sensitivity. Wan et al. (2020) used a simplified microphysics parameterization to show that the process coupling and closure assumption are two causes of the time step convergence problem. They provided a good reference to approach this issue, which merits more experiments to study the coupling of dynamical core and microphysics parameterization in the future.

- Please describe the CAM5-FV configuration (grid spacing, timesteps, etc.) used for APE simulations.

Response: Description of the CAM5-FV configuration is added in the revised manuscript. The time steps for CAM5-FV is set as 1800 s, and the horizontal resolution is 1°. The effect of prognostic aerosols is not considered. Other configurations and physical parameterizations for CAM5-FV are consistent with GRIST-CAM5phys.

Specific comments

L213: Cumulus cloud fraction is too high by at least a factor of 2, which I wouldn't call "good" agreement. But it's hard to tell without an LES plotted in the figure (see general comment above)

Response: Thanks for your reminder. We revise the statement as: The maximum (cumulus) cloud fraction and updraft mass flux with each time step for the SCMs are higher than that in the large-eddy simulation (LES) (compared with Figures 3 and 6 in Siebesma et al. (2003) and Figure 2 in Park and Bretherton (2009)).

L221-224: Did you change how MG is sub stepped?

Response: No, we didn't. The MG is the same as in CAM5.

L230: Did you use RRTMG normally during the 15 h run (i.e., with shortwave radiation on)? Or did you use the simplified longwave parameterization suggested by Ackerman et al. (2009)?

Response: The RRTMG, with both shortwave and longwave radiation on, is used in all the SCM test cases.

L319: Add units to SST (equation 4). I assume this is in degrees C.

Response: Thanks for your reminder. The unit of SST is added.

Figure 11: Add y axis labels (at least to panels a and d)

Response: We have added a y-axis title to this figure.

Typographical comments

L58: "complexity" instead of "complex"; L60-61: Not sure what is meant by "well-expected" – I suggest removing "well"; L65: "intricate" doesn't make sense here.; L184: "despite that the dynamical …" instead of "despite the dynamical"; L264: weather modeling is not "factual" – it is still a simulation. Perhaps "operational" would better capture your meaning?; L312: "with the CAM5-SE dribbling …" instead of "with the CAM5-SE with the dribbling"; L334: "participating" instead of "participated".

Response: Thanks. We have modified the grammar mistakes mentioned above.

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