Response to Anonymous Reviewer #1

We thank Reviewer 1 for the valuable suggestions. The reviewer's comments are copied below in an italic font. Responses are in normal font.

Minor comments

1. *At several places in the description of the test case, the assumption is made that physics-dynamics coupling takes place at constant pressure. While this is standard for hydrostatic models, this is not necessarily the case for nonhydrostatic models. The distinctions needed depending of the type of physics-dynamics coupling should be mentioned explicitly where appropriate, e.g. on p. 8271 / Eq. (2) and p. 8274 / discussion after Eq. (13).*

   The reviewer rightly points out that the assumption of a constant moist air pressure within the physics package comes from the hydrostatic modeling paradigm that traditionally uses pressure-based vertical coordinates. Non-hydrostatic models are most often built upon a height-based vertical coordinate. Mass-based vertical coordinates (based on hydrostatic pressure) have also become prevalent for non-hydrostatic models. Height-based vertical coordinates have a built-in assumption that the volume, and thereby the density, stays constant within the physical parameterization suite.

   We added additional information to Section 2.6 about the physics-dynamics coupling in nonhydrostatic models. and point to three new references (Thurre and Laprise (1996), Thurre (1998), and Malardel (2011)). These references explain that the isobaric physics-dynamics coupling shown in this paper represents a anelastic approximation for nonhydrostatic dynamical cores. In particular, Malardel (2011) showed with a nonhydrostatic and hydrostatic version of ECMWF's model IFS that the anelastic (isobaric) physics-dynamics coupling in the nonhydrostatic simulations leads to almost identical results in comparison to the hydrostatic IFS simulations at large (hydrostatic) scales.

   Unapproximated forms of the physics-dynamics coupling for nonhydrostatic models are model-dependent, and would require a rather lengthy addition to this manuscript when trying to capture all possible cases (which we do not do). For example, the coupling philosophies need to be different depending on the choice of the thermodynamic equation in nonhydrostatic models. Models that use the thermodynamic equation in potential temperature form are actually coupled in an identical way for both isobaric (constant pressure) and isochoric (constant volume) assumptions. This has been verified with nonhydrostatic model developers from NCAR (Drs. William Skamarock and Joseph Klemp, personal communication) and is briefly mentioned in Sect. 2.6 now. However, if nonhydrostatic models use the thermodynamic equation in the form c_vT the physics-dynamics coupling becomes more complicated and has led to debates in the literature. For example, the nonhydrostatic model COSMO of the German Weather Service (with c_vT formulation) has traditionally used an isobaric formulation for the physics saturation adjustment (SA) as documented by the newly added Petrik et al. (2011) reference (their Eq. A2, $\partial T/\partial t_{phys} = Q_h/(\rho c_p)$ where Q_h is the latent heat release). Petrik et al. (2011) (their Eq. 23) then showed that this approximation on the right hand side should be replaced with $(Q_h + Q_m)/(\rho c_v)$ where Q_m indicates the mass redistribution of water species.

   In the revised manuscript, we now mention the anelastic approximation, comment on the coupling technique for potential temperature based dynamical cores and point to the Petrik et al. (2011) paper as an additional resource for other model formulations.

2. *Eqns. (7), (9) and (13): Shouldn't the sensible heat flux be proportional to $\theta_s - \theta_a$ rather than $T_s - T_a$?*

   Unfortunately, the formulations for the sensible heat flux at the surface are quite diverse in the literature and in GCMs. The reviewer rightly points out that alternative formulations with $\theta_s - \theta_a$ instead of $T_s - T_a$ have also been used where the subscript s denotes the surface and the subscript a the lowest model level. Formulations with $\theta_s - \theta_a$ follow a turbulence approach. This implies that sensible heat fluxes are positive and thereby upward for unstable environmental conditions with $\theta_s > \theta_a$, the sensible heat flux is zero for adiabatic conditions with $\theta_s = \theta_a$, and it becomes negative (downward) for stable conditions with $\theta_s < \theta_a$ despite the decreases in temperature with height in the latter two cases. While we agree that the mixing in the turbulent boundary layer should depend on the stability characteristics, we do not apply this approach at the surface. Basic physical principles demand that heat is transported from a warmer spot to a colder spot, which is captured in the sensible heat formulation with $T_s - T_a$. As an example, a warmer surface layer will lead to $T_s - T_a > 0$ and thereby positive, upward pointing sensible heat fluxes from the surface into the atmosphere. Such temperature-based formulations have e.g. been documented in textbooks like Thomas Warner's "Numerical Weather and Climate Prediction" (Eq. 5.14) or David Stensrud's "Parameterization Schemes"(Eq. 2.43).

3. *p. 8275, 2nd para: As many nonhydrostatic models employ a height-based coordinate, a comment would be desirable on how sensitive the results are to the profile function in Eq. (14). Would one have to convert the vertical profile function from linear in z to linear in σ?*

   We did not have access to a nonhydrostatic model with height-based coordinates to test the sensitivity ourselves. However, since pressure and height are related in an exponential fashion we argue that the differences between a linear-in-z and a linear-in-σ dependence are rather big. We therefore recommend reconstructing the $\sigma = p/p_s$ variable in nonhydrostatic models by using the pressure p at the given model level and the surface pressure p_s. In case the surface pressure is not readily available in a non-hydrostatic model an extrapolated value of the surface pressure based on the information at the lowest model level should be computed. We note that our simulations used the pressure-based η-coordinate and a linear-in-η profile for Eq. (14), which behaves the same as the linear-in-σ profile.

4. *Eq. (18): At the poles, T_{eq} is more than 40 K colder than the SST. I see that the relaxation coefficient k_T is rather small at the poles, but I still wonder if this yields reasonable heat fluxes.*

   The heat fluxes at the pole are quite reasonable. The test case has an average sensible heat flux of about 10 W/m^2 at the poles, compared to about 14 W/m^2 in aquaplanet simulations.

5. *p. 8279, 1st para of section 3: Similar to comment #3, it would be important to know how sensitive the results are to the setup of the vertical model levels. In models with a height-based coordinate system, the layer setup cited here cannot be exactly replicated.*

   When developing the test case, we experimented with different level spacings, e.g. a doubling of the vertical resolution (from 30 to 59 vertical levels while keeping the model top at 2 hPa) which halved the vertical grid spacing. However, we kept the height position of the lowest model level the same which is about 60-65 m (depending on the temperature and therefore location). This height position of the lowest model level enters the physical parameterizations, e.g. via the computation of the surface fluxes. Therefore, we recommend in the manuscript to also select a similar lowest level position in other models in order to make the results comparable. In general, the general circulation of the atmosphere changes very little when changing the vertical grid spacing above the lowest model level, but is more sensitive to the position of the lowest layer. It is not necessary (and even not possible) to exactly replicate our layer setup in non-hydrostatic models, but the position of the lowest model level should be set around 60-65 m.

6. *p. 8295, top: Does the "se_ftype = 0" option apply to all physics forcing terms, including latent heat release from the saturation adjustment? I ask this question because at convection-resolving scales, applying the latent heat release term as a gradual forcing in the dynamical core tends to severely (and detrimentally) affect convective dynamics.*

This is an interesting comment. In the current "se_ftype = 0" formulation the total tendency from all physics processes is passed to the CAM-SE dynamical core that applies it gradually during its sub-cycled time steps. Therefore, the current formulation includes the forcing by the latent heat release from the saturation adjustment. We have not tested CAM at convection-resolving scales since all dynamical cores are currently hydrostatic and inadequate for such simulations. At these small scales, we envision that the desired coupling frequency between the dynamical core and physical parameterizations is very short so that it might not be necessary to select "se_ftype = 0".

7. *p. 8297, 2nd para: Do the CAM developers have a hypothesis about the reason for the strong circular gravity wave structures in the SE dycore? Their large spatial extent over several thousands of kilometers raises the questions why they propagate with nearly no damping over such large distances, and if they propagate at a physically reasonable phase speed. Gravity waves with a vertical wavelength of twice the tropopause height ($\sim$ 30 km) should have a propagation speed of about 50 m/s, implying that there would have to be a stationary on-off forcing over many hours in order to excite circular waves of the spatial extent observed here.*

   The circular gravity waves in CAM-SE are of rather large scale, and are hardly affected by the diffusion processes in the CAM-SE dynamical core (which act most strongly at the grid scale). For example, we tested simulations with an increased explicitly-applied diffusion. The increased explicit diffusion changed the gravity wave noise pattern somewhat but did not eliminate it. The circular gravity waves are triggered by large latent heat releases from grid-point-scale storms along the equator (provided the physics time step is long). These grid-scale storms are rather long-lived and can exist at one location for several hours before dissipating. We argue that this time is sufficient to create this large-scale gravity wave response over several thousand kilometers.

8. *p. 8299, 3rd para: Very good point!*

   Thanks for highlighting this.

9. *p. 8303, 2nd para, discussion of Fig. 14: Do the different physics time steps (600 vs. 1800 s) play a role for the precipitation intensity spectra?*

   Our assessment is that the impact of the physics time steps is minor. EUL is run with the shorter physics time step (600 s) than the other three dynamical cores since it is the default setting. However, we initially also used a sub-cycled EUL dynamical core with the longer physics time step of 1800 s, and found that the precipitation intensity spectra are very similar with a slight increase in the occurrence of extreme (> 450 mm/day) precipitation rates.

Editorial comments

1. *p. 8270, bottom: R_v should be 461.5 J/(kg K), not 462.5*

   Agreed, this was a typo (thanks for catching it) and has been corrected to $R_v = 461.5$ J kg^{-1} K^{-1}

2. *p. 8276 / Eq. (17): Model developers usually associate p_{top} with the model top pressure. I would prefer p_{pbl} or something like that.*

   Agreed, p_{top} changed to p_{pbl}

3. *p. 8279, ln. 4/5: The subject seems to be missing in this sentence.*

   Corrected to “It is also the default setting for CAM5-FV”

4. *p. 8289, ln. 26: In Table 2, it says 2.10 rather than 2.11 mm/day for MITC.*

   Thanks for pointing this out. 2.10 mm/day is correct, the text has been corrected to match the table.

5. *p. 8230, ln. 12: “Instantaneous precipitation rates” refers to one physics time step?*

   Yes, the “Instantaneous precipitation rates” refer to the rate at the end of one physics time step, without any averaging.

6. *p. 8291, ln. 21: c_p already denotes the specific heat capacity at constant pressure. Please use e.g. c_{ph} for the phase speed.*

   Agreed, c_p changed to c_{ph}

7. *p. 8300, ln. 6: The term “hemispherically averaged” is a bit misleading. I was first thinking of “averaged over the northern and southern hemisphere, respectively”; only after looking at Fig. 12, I understood what it means. Perhaps this could be formulated a bit more clearly.*

   The description has been rewritten to be clearer. The text now reads:
   The general characteristics of the time-mean, zonal-mean precipitation rates of all four MITC dynamical cores are shown in Fig. 12. The forcing terms and zonal results are symmetric about the equator, therefore the two hemispheres have been averaged together to reduce sampling variability. The precipitation rates are similar, especially for the precipitation rates in the midlatitudes and polar regions (not shown)...