Response to Anonymous Reviewer #2

We thank Reviewer 2 for the valuable suggestions. The reviewer's comments are copied below in an italic font. Responses are in normal font.

1. *The references are a bit "Williamson heavy". It would be helpful to also include the following references, where the first discusses aquaplanet wave spectra with and without deep convection (lack of Rossby and MJO modes) and with physics scaled for different planets, the second introduces the small planet framework, and the third describes Kelvin waves in the stratosphere.*

   Semane, N. and P. Bechtold, 2015a: Convection and waves on small Earth and deep Atmosphere. Tellus A 2015, 67, 25151, http://dx.doi.org/10.3402/tellusa.v67.25151

   Wedi, N. P., and P. K. Smolarkiewicz, 2009: A framework for testing global nonhydrostatic models, QJRMS, 135, 469-484

   Lott, F. et al, Kelvin and Rossby-gravity wave packets in the lower stratosphere of some high-top CMIP5 models, 2014, J. Geophys. Res. DOI: 10.1002/2013JD020797

   We removed two of the references to papers by Williamson. As suggested we added the Wedi and Smolarkiewicz (2009) reference as well as a new reference by Kurowski et al. (2015) to the introduction since they have a clear connection to Held-Suarez-type simulations and idealized test cases. In addition, we added the suggested Semane and Bechtold (2015) reference to section 4.4. However, the Lott et al. reference is not a good match for this manuscript, see also point 11 below.

2. *p16, l3-4: You rightly state that the "order of the physics processes matters". What happens if one computes forcing, ie (radiation + surface fluxes) first and then condensation, also if u add convection it should be called last if possible*

   We have not tested the specific order of the physics processes that the reviewer mentioned, and are therefore unable to exactly comment on the consequences of the changed order. We expect changes, but the general circulation should still be rather similar to the one presented in the manuscript. This raises an even bigger question. E.g. the reviewer suggests that convection should be called last. In NCAR's CAM5 physics package deep and shallow convection is deliberately called before the microphysics scheme (which includes large-scale condensation). There seem to be very different modeling paradigms in the GCM modeling community. We would very much like to understand these better in the future. Unfortunately, very few modeling centers document the order of the physics routines and the coupling strategies in their publications.

3. *p20, l16-17: please remove sentence "our aquaplanet ... Bulk aerosol Model ... symmetric".*

   We keep this sentence since this piece of information enables others to exactly replicate our model experiment. CAM offers various aerosol treatments (bulk aerosol module, modal aerosol module, fully prognostic aerosols), and without this information our model results would not be repeatable.

4. *p21, l12-13: change "aquaplanet simulations are a more suitable comparison than observations" -> "... are an attractive alternative comparison tool to observations"*

   Sentence has been changed.

5. *p22, l2-3: "... we do not focus on these systematic stratospheric differences" ???? why do you then plot T-spectra at 100 hPa in Fig. 7 is stratospheric Ts are not realistic??*

   The stratospheric general circulation in the moist Held-Suarez simulation is different from observations or aqua-planet experiment since the atmosphere is relaxed towards an isothermal reference state in the stratosphere. This is unrealistic and e.g. suppresses the polar stratospheric jets. However, when comparing the tropical wavenumber-frequency spectra (Fig. 7) of the temperature at 100 hPa (which lies at the tropopause level in the tropical atmosphere) we do not compare the general circulation of the atmosphere, but the tropical waves that originate in the troposphere and travel upward. These waves are comparable since the tropospheric circulations are comparable.

6. *p31-33: I find section 5.1 and Figs 8,9 largely redundant and out of place. Please remove. 5.2 is a natural start for section 5. However if you want to keep the result for se+ftype=1 in Figure 9 then add it as (d) in Figure 10 or alternatively you can mention it (without Figure) in 6.8*

   We disagree with the suggestion to delete section 5.1. This section discusses one of our major findings that the physics-dynamics coupling technique can trigger spurious gravity wave noise. Figure 9 shows proof that grid-scale storms are the source of the gravity wave patterns in CAM-SE and highlights the impact of the coupling algorithm. We consider this figure a key figure of the paper. Figure 8 is important since it proves that dry dynamical cores are insensitive to the coupling choice, and that the moist idealized test is able to replicate the behavior of more complex aqua-planet simulations (and is therefore a test case that is relevant for modelers). Merging section 5.1 (with the focus on the model CAM-SE alone) and section 5.2 (with the focus on a model intercomparison) would make it unnecessarily difficult to disentangle the scientific messages.

7. *p 35, first paragraph: please shorten and account for removal of Figs 8,9*

   As described above we disagree with the suggestion to merge sections 5.1 and 5.2 and the removal of Figs. 8 and 9 (which contain a key message of the manuscript.

8. *p38: Please remove either Fig. 12 a or Fig. 12 b, redundant these two results are equivalent and shorten/adapt the text accordingly*

   Agreed, Fig. 12b has been removed and the caption adjusted accordingly. All text referencing Fig. 12b has been removed. All references to Fig. 12a have been changed to Fig. 12. The adjusted text now reads:
   These dynamical connections are displayed in Figs. 12 and 13. In particular, Fig. 12 demonstrates that the equatorial precipitation rate is strongest in SLD, followed by EUL, FV, and SE.

9. *p39, l8-9: remove "Similarly, the SLD (Fig. 12b)"*

   Removed, text now reads:
   This is displayed by the narrower regions of equatorial precipitation, or equivalently the steeper slopes in the precipitation rate curves, in SLD and EUL. Additionally, SLD and EUL exhibit stronger low-level downdrafts between $\pm 12^\circ$ to $\pm 5^\circ$, which reside in the descending branch of the Hadley circulation (Fig. 13c and d).

10. *p42 5.2.4: in discussion of equatorial waves include results of reference Semane and Bechtold above (lack of Rossby, MJO)*

We cite the Semane and Bechtold (2015) paper now in section 4.4 where it is a very useful reference for the discussion of aqua-planet simulations (and their equatorial waves) with and without deep convection schemes. However, the Semane and Bechtold (2015) reference is not such a good match for the suggested section 5.2.4. The Semane and Bechtold (2015) paper investigates full-physics aquaplanet simulations with and without a convection scheme and furthermore asks whether convection-resolving simulations can be computed rather cheaply in model configurations with a reduced-size radius of the earth and rescaled diabatic forcings (from a full-complexity physics package). The article is thereby very different than the topic of our section 5.2.4. This rather short section intercompares the wavenumber-frequency spectra of four dynamical cores in the simplified "moist Held-Suarez" mode that misses many of the complexities of full-physics configurations.

11. *p46 6.6: Discussion on QBO etc, please include reference to Lott et al*

Unfortunately, the Lott et al. reference is not a good match for section 6.6 which contains a rather brief discussion of how idealized Held-Suarez-type experiments can be used for studies of QBO-like oscillations. The Lott et al. (2014) paper does not have a direct connection to idealized QBO or simplified GCM studies, but contains a model intercomparison of Kelvin and Rossby-gravity waves in nine full-complexity CMIP5 models with high model tops. Since the reference is more general it would not contribute to the discussion of idealized test cases.