

## ***Interactive comment on “A moist aquaplanet variant of the Held–Suarez test for atmospheric model dynamical cores” by D. R. Thatcher and C. Jablonowski***

### **Anonymous Referee #2**

Received and published: 29 October 2015

The manuscript describes a moist evrsion of the HELD-Suarez test for global models including simplified physics. The manuscript - though somewhat too lengthy (see below) - is generally well written and the method and results are well presented. The discussion is essentially faultless but I strongly recommend to shorten the manuscript and to include a few clarifications and references. Overall my recommendation is for "minor revisions".

Specific points:

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

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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, 2015: 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 non-hydrostatic 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

-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

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

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

-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 ??

-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

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

-p38: Please remove either Fig. 12 a or Fig. 12 b, redundant these two results are

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equivalent and shorten/adapt the text accordingly

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

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

-p46 6.6: Discussion on QBO etc, please include reference to Lott et al

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Interactive comment on Geosci. Model Dev. Discuss., 8, 8263, 2015.

**GMDD**

8, C2724–C2726, 2015

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