Interactive comment on "Radiative-Convective Equilibrium Model Intercomparison Project" by Allison A. Wing et al.

I. Held (Referee)

isaac.held@noaa.gov

I am very supportive of the proposed RCE intercomparison project. The paper is generally well-written, and my comments primarily relate to the specifics of the proposed project.

Major issues:

I continue to be puzzled that the papers on RCE simulations with CRMs hardly ever discuss the sensitivity of the results to boundary layer formulations. In fact, most of these simulations have no boundary layer closure at all except for the Monin-Obukhov (M-O) surface flux framework. M-O-is, in fact, a boundary layer formulation, and if that is all that one has in the model, one is (arbitrarily it seems to me) assuming that a

closure is needed between the surface and the lowest model layer but not between the first model layer and the second model layer. The specification of a minimum wind speed in the bulk surface flux formula can also be thought of as a boundary layer turbulence parameterization of sorts, and the fact that you are careful to include this in your specification is implying that you don't trust the resolved scales to provide the right gustiness. Running at 3km resolution without a boundary layer above the lowest model level strikes me as extreme (not that I haven't done it myself). I would really like to see this topic directly discussed by the authors. This is a "grey zone" that seems to get less attention than the problem of transitioning from parameterized "convection" to explicit "convection" and one that I find very confusing.

Along the same lines, it would be nice if the text more explicitly encouraged much higher resolution simulations than 1km in the 100km domain, closer to LES resolution for the planetary boundary layer.

I would strongly encourage you to reconsider and ask groups to run with a standard microphysical mechanism in addition to their model's microphysics. Otherwise, there is a good chance that the diversity of simulations will be dominated by the diversity of microphysical assumptions. (For example, we know that in RCEs assumptions about ice fall-speeds will exert a strong control on the cirrus climate.) I realize that this would be work for the authors, who have to be confident that this standard microphysical mechanism worked reasonably well in a couple of the models that they are familiar with. The proposal here is to leave this issue to a second round, but I think this is postponing the inevitable. (For example, you suggest that models with more elaborate microphysics, with explicit activation of CCNs perhaps, be adjusted to create simulations that produce the same droplet number concentrations as recommended for models that specify these droplet numbers, but what if droplet numbers change significantly with warming in these models?)

The utility of single column models, in my view, is dependent on their "effective stability": the single column equilibrium, whether time-independent or dependent, with no

resolved flow is always a solution of a fully doubly periodic box or global RCE model that uses the same column physics (in fact, this is a good test that a global model has been successfully homogenized) . But it is invariably spatially unstable. To the extent that the time average of the RCE state in the 100km box bears little resemblance to the single column equilibrium, the single column results are going to be hard to relate to the rest of the project.. I would try to encourage any contributions of single column RCE simulations to also provide full doubly-periodic 100km box simulations for comparison, or at least global RCE simulations if these are easier to perform.

For the same reason, I don't particularly care for initializing global models with single column equilibria. Among other issues, what if these equilibria are time-dependent? (This is not hypothetical – it may be the rule rather than the exception with cloud-radiative interactions present). Do you initialize all the grid point with the same phase?

I would not specify number of grid points to be used. I would just specify the physical size of the domain and then encourage simulations with 1km resolution in the small domain and 3km in the bowling alley domain, while also encouraging any other resolutions that the groups are interested in exploring.

The recommended upper boundary seems quite low to me, making it awkward to look at the vertically propagating gravity waves in the stratosphere excited by the convection. This is a nice opportunity to look at the robustness of the simulation of those waves, especially in the bowling alley case,. On a related point, I would be a little worried that the bowling alley configuration will in some cases allow the generation of QBO-like oscillations, as in the two-dimensional limit. This could cause some large unexpected differences between simulations. It is worth mentioning at least.

You seem to be initializing stratospheric water with extremely small, but non-zero, mixing ratios. But won't stratospheric water take forever to equilibrate in RCE simulations, where the only thing that can mix vapor up is breaking gravity waves? This vapor has some effect on sensitivity of radiative fluxes to warming. Kuang and Bretherton, JAS,

2004 have circumvented this problem by specifying a small vertical motion (a Brewer-Dobson upwelling extending through the troposphere and stratosphere), which allows the tropopause cold-trap to operate. As in nature This also makes it easier to study the tropical tropopause layer, where convection and upwelling can both be important . In any case, this issue of equilibrating stratospheric water should be discussed. (How do you get water vapor into the stratosphere in a single column model? Do you intend that these models include sub-grid closures for mixing by convectively generated waves?)

Minor comments:

p-3, l-2. Held, Zhou, Wyman, 2007 also compared the cloud feedbacks in RCE against those in the tropics of a global model with the same column physics.

p.-5, l-20. Sorry, but false precision is a pet peeve of mine. You specify the mean surface pressure to 6 significant figures, but the mass of the atmosphere is not conserved, it increase with warming due to increase in vapor. Some global models correctly incorporate the mass source/sink of vapor, and these models will just ignore the value that you specify, they need to specify the dry mass. As another example, in the list of constants you are careful to specify the value of the latent heat of vaporization , but you don't specify its temperature dependence – or, equivalently, the heat capacity of the condensate. Are you expecting the latter to be set to zero? You specify the (mean) radius of the Earth with precision and then suggest that models just use smaller radii, a good idea since the radius in the non-rotting case is totally irrelevant until it approaches the depth of the model domain. Meanwhile differences in assumptions about surface smoothness or ice fall-velocities or many other things that are literally orders of magnitude more important are passed over. The table of geophysical constants is pointless in my opinion; just ask modelers to use their standard Earth values

I would not ask the modeling group to provide your preferred measures of aggregation. Other measures might turn out to be more interesting. These should be relatively easy to generate in a uniform way after the fact from the data archive. You should keep the

post-processing requested from the groups to a minimum. (Some groups may not be interested in the dynamics of aggregation.)

As for the energy budget, I am not sure why you want this in advective rather than flux form (I may be missing something.). A data request could include the fluxes of energy at the boundaries of the grid cells – at least in CRMs and grid-point GCMs. These are more likely to be precisely defined by the underlying numerical scheme than the advective form.

I can't see anything in Fig. 6

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