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Interactive comment on "Improving subtropical boundary layer cloudiness in the 2011 NCEP GFS" *by* J. K. Fletcher et al.

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"general comments"

This study implements a set of parameterization changes to the GFS model's boundary layer cloud schemes. The goal is, ultimately, to improve the representation of clouds in the GFS and the CFS. The changes that are made to the physics make sense, and seem to take steps to improve the physical consistency and veracity of the GFS parameterizations. That said, the changes are also relatively small 'tweaks' to the model rather than development of new parameterizations. This is justified as a way to improve the operational model in a timely manner, and also makes sense as an approach to deal with 'low-hanging fruit.' The approach, as outlined in the text, appears to be a fairly typical one to parameterization development (at least in climate models). Starting with the single column model and then moving to global simulations. Most of the re-

sults are in the SCM sections, which focus on non-precipitating shallow convection and nocturnal stratocumulus cases. Issues of compensating errors becomes quite clear in the experiments, even with only the SCM in these idealized cases. Results seem overall promising in the SCM experiments. The results in the global model seem more mixed, but the current results suggest that continuing in this direction will wind up improving the GFS and CFS. The presentation is straight-forward and easy to follow, the results are shown clearly and there are no major problems with the experiments or results that I can detect. There are a few issues that should be addressed, either with some additional discussion or possibly by doing some additional analysis, as described below.

"specific comments"

1. Is there some justification for running the SCM with half the time step of the global simulations? Similarly, if the time step is being reduced, why isn't the frequency of radiation calls commensurately changed?

2. The BOMEX case excludes cloud-radiation interaction, which seems like an important simplification. Have more realistic shallow convection cases been run (e.g., RICO)? Is anything being missed with the simpler setup?

3. In the discussion of Figure 3, the text says that the changes are a big improvement to the condensate profile. That seems justified, but isn't it still a bit concerning that the cloud water is still restricted to two levels with several completely condensate-free levels in between? Shouldn't those levels be moist enough to maintain the detrained condensate from the shallow convection scheme? Between the changes in entrainment and updraught velocity, I expected to see condensate through the cloud layer.

4. The discrepancy between the cloud fractions used in the microphysics and radiation schemes is quite shocking. I was surprised that this was not emphasized even more as a glaring issue that should be corrected (even to the temporary detriment of forecast skill if need be).

5. Something is wrong about Figure 5's top panel. Either some of the experiments are not visible or ar the wrong color. Or two cases are being shown, but not described.

6. For the global simulations, there are some seemingly odd choices made. The first one, skipping the uncoupled configuration, is explained in the text: apparently GFS isn't set up to run "AMIP" experiments. Perhaps the other details are similarly explained? First, the choice to start in 1948 instead of either later (better comparison to obs) or earlier ("preindustrial"). Second, initializing from NCEP NCAR reanalysis seems like a poor choice; aren't there severe biases in the vertical distribution of water vapor that could impact boundary layer clouds? Third, it isn't clear why long runs were chosen rather than short hindcasts (as in the CAPT approach). Though some forecasts are examined at the end, it seems like a natural choice for working with the GFS model.

7. There's not much indication about whether the global simulations are producing a reasonable climate in terms of large-scale circulation, seasonal patterns, etc. The runs are too short to do a whole lot, of course, but there is the potential for ascribing biases in the cloud fields to the parameterization changes directly, but there could be feedbacks with the dynamics that lead to remote cloud responses. One possible way to deal with this could be to include Taylor diagrams showing a few important fields for the experiments. The changes in the cloud fraction and SWCRE could also be summarized by a Taylor diagram.

8. It is difficult to make much of the results in Section 7. Including some analysis of forecasts seems like a good idea, but here we get the changes to the parameterizations as well as resolution and possibly large-scale circulation. Disentangling these effects is impossible without getting a sense for the role of resolution and error growth during the forecasts (toward model climatology). Some discussion of these issues should be included, assuming additional runs can not be performed to directly address the issues. The point of the section seems to be to show that improving the parameterizations does not immediately improve the forecasts. This is an important point to make, but exposes these details about resolution, initialization, and model climatology that have mostly

been avoided up to this point.

9. Also related to results of Section 7, and to all the global results, there was not much mention of tuning procedures for the model. Having altered the parameterizations and seen that these changes interact within the cloud schemes, and also obtaining big changes in the TOA radiation in the global simulations, it seems likely that the model needs to be re-tuned. Is there a standard GFS tuning procedure, or well-known tuning parameters (besides the ones being changed in the updated parameterizations)? Even though the horizontal wind forecasts degrade with the parameterization changes, isn't it likely that could be negated with other adjustable parameter changes?

Interactive comment on Geosci. Model Dev. Discuss., 7, 2249, 2014.