

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

J. K. Fletcher et al.

jennifer.fletcher@monash.edu

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The authors thank Reviewer 2 for thoughtful questions and 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?

We reduced the time step of the SCM so that the GFS could submit a simulation to an intercomparison project and never switched it back. It had no noticeable effect on the SCM simulations. Unfortunately, the GFS is set up such that a one-hour time step on radiation is the minimum permitted.

2. The BOMEX case excludes cloud-radiation interaction, which seems like an im-

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portant simplification. Have more realistic shallow convection cases been run (e.g., RICO)? Is anything being missed with the simpler setup?

We did simulate the RICO case, and found that the GFS SCM was actually slightly closer to LES than in BOMEX. We don't show these results because of the difficulty in constraining the forcing in the RICO case. We agree it would be quite interesting to see if changes in radiative cooling feed back on the shallow cumulus development; however we will have to reserve that for future work.

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.

Our recommended parameter change is the `VvelNewEntr`, in which the grid scale condensate is only visibly greater than zero at cloud base, with no grid scale condensation at cloud top. This is likely due to the relative humidity profile within the conditionally unstable cloud layer. We have tested, but do not show, simulations where we implement a modified version of the de Rooy and Siebesma detrainment scheme; this increases shallow cumulus detrainment at levels below cloud top and leads to a profile a bit closer to what you're describing. However, this change has not been tested in the global model and so we do not include it in this paper.

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).

We have clarified/re-emphasised this by adding the following paragraph:

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To section 4.2, page 10, lines 4-6:

"The cloud fraction used in the Sundqvist scheme affects the model indirectly through the autoconversion and large-scale condensation rates. To maintain consistency with the rest of the scheme the Sundqvist formulation must be used. However, the Xu and Randall scheme matches observations better in general and is preferable for the radiation scheme."

We agree that this is a clear problem that needs to be addressed; however, change happens slowly for the GFS. A unified cloud scheme that has consistent cloud fraction between microphysics and radiation is being developed by Ruiyu Sun (a co-author on this paper), but implementation of a change that produces any degradation in forecast skill is not possible.

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

We've added the following to the figure caption:

"Results are identical for all experiments without shallow convection, thus ShCuFlag and VvelOrig are hidden by VvelNewEntr."

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.

You are right; the first two of these – initialising in 1948 and with NCEP reanalysis –

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were done because that's what was possible. We've added some explanation to the text clarifying this.

We chose to do long runs because we wanted to evaluate the GFS in a manner similar to how it would be used when it is part of the CFS, since unified modeling is part of NCEP's goal. This means seasonal and longer runs. However, the CAPT approach is not at all exclusive to this and we agree should be done in future work.

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.

We deferred discussion of the GFS climate to Xiao et al (2014):

"Xiao et al. (2014) presented our CPT's comparisons of the simulated climate from multidecadal free-running simulations using an ocean20 coupled version of the GFS operational in late 2011 with comparable simulations using Version 1 of the CESM (which uses the Community Atmosphere Model Version 5, or CAM5, as its atmospheric component). They found that the simulated GFS climatology was of comparable or higher quality to those with CESM1, except for cloud cover and radiative properties."

Regarding changes in circulation, we have added the following paragraph to section 6:

"It is unlikely that changes in cloud radiative forcing directly caused the SST changes in deep convective regions, where the substantial change in shortwave cloud forcing was largely offset by a change in longwave cloud forcing (not shown). However, reductions in excess cloud cover in the offshore southeast Pacific may contribute to the increase

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in SST in that region and subsequent reduction in zonal SST gradient associated with a weakening of the Walker circulation. This can also be seen in the change in SST off the Peruvian and Chilean coasts, where positive SST biases worsen despite an increase in cloud cover. This is likely due to a weakening in coastal upwelling. We found that changes in wind stress also suggest a weakening of this circulation, with a decrease in surface easterlies in the tropical central and west Pacific and a reduction of northerlies in the southeast Pacific (not shown). Such sensitivity of the basin-wide Hadley-Walker circulation pattern to changes in marine low clouds associated with parameter changes in shallow convection and moist turbulence parametrisation is also found in other GCMs (e.g., Ma et al 1994, Xiao et al 2014)."

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.

Unfortunately these two short forecast runs are not enough to make conclusive statements about the important points that you raise here. We simply include these results to highlight that an improvement to model climate is not enough to implement a change, as the model forecast skill has to, at minimum, stay the same as well. Given this, we have decided to remove the figures and re-write all of Section 7 as follows:

"While testing parametrisation changes in climate mode for the GFS is an important aspect of our work, parameterisation development requires testing model changes' effect on forecast skill as well. Typically, data assimilation tests with runs of at least

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two months are done. If forecast skill is improved, especially in terms of the 500 hPa anomaly correlation, precipitation skill over the United States, or hurricane track forecast, the change is likely to be implemented. If the skill is neutral but the climate bias is reduced, there is still a good chance of implementing the change. If the forecast skill is degraded, modifications or re-tuning of other model parameters, such as those controlling autoconversion or the critical relative humidity used for condensation, will be tried.

A short data assimilation experiment implementing the model changes included in the NewEntr and ShCuClidCover SCM results of BOMEX and DYCOMS, respectively, has been performed. Initial results suggest that, while in many respects the forecast skill is improved or neutral, the root mean square error in tropical horizontal winds is increased. As a consequence of these experiments, further work must be done before these changes can be implemented into future versions of the GFS; climate improvements must, at the very least, have a neutral impact on forecasts. Single column tests (not shown) indicate that changes in horizontal winds are not a result of cumulus momentum transport – the NewEntr change has no impact on winds in the SCM. Instead, the change is affecting horizontal pressure gradients; thus more global model tests – and possible model-retuning – are needed to investigate this further. This work is underway by NCEP developers and will be reported on in a future study."

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?

It is very likely this is true. We've uncovered some compensating errors through adjust-

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ing the entrainment and precipitation/detrainment efficiency, but as you say to implement these changes the model would likely need to be re-tuned. We hope that NCEP undertakes this effort.

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

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