Geoscientific Model Development Discussions

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Comment on gmd-2021-19

Anonymous Referee #2

Referee comment on "Effect of horizontal resolution on the simulation of tropical cyclones in the Chinese Academy of Sciences FGOALS-f3 climate system model" by Jinxiao Li et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-19-RC2, 2021

This paper serves as documentation of the tropical cyclone activity simulated by the FGOALS-f3 models submitted to the HighResMIP subproject. While there is little unexpected in the comparison of low to high resolution models, it is important to have such individual model results in the literature. I recommend that it be sent to the authors for some fairly minor revisions that I describe in detail below.

Section 3.1:

figure 2. It is difficult to synthesize by eye the biases in figure 2. I would like to see either a bar chart figure or a table with observed and simulated TC counts both globally and by ocean basin.

Figure 4. Please note that the bias in the min pressure/max wind speed is worse in the North Atlantic than in the western Pacific in the high resolution model. Why is this?

Lines213-215 " Neither the single peak in the number of tropical cyclones in the northern Atlantic(peak month September), eastern Pacific(peak month August) and southern Pacific(peak month February) oceans nor the double peak in the northern IndianOcean (peak months May and November) could be reproduced in FGOALS-f3-L."

I don't think this is actually correct, although the low resolution model does not produce the magnitude of these peaks, it does appear to replicate the timing of the seasonal cycle. This would be more apparent by normalizing figure 6 by the number of storms per basin. Admittedly, the Southern Pacific does appear to be delayed.

Lines 226-229: It is a bit of a stretch to claim that the interannual correlation of ACE is improved with resolution in WP and NA as the differences are very small in figure 9. In fact, given that the correlation in interannual counts changes a fair amount in figure 8, one might expect that the ACE correlation should change even more, given the dependence on the square of peak wind speed and the differences in that field between resolutions. A more interesting quantity might be simply the average ACE per basin.

Section 3.3: Grammar. Instead of "The extreme position of precipitation", you mean "The position of extreme precipitation". Figure 9 is quite interesting. It may be clearer to express the bias in terms of an angle and radial distance. It does appear that the radial distance is quite good. Any thoughts on the error in angle? Also, the diameter of the eye would appear to be only one or two grid cells. It should be mentioned that although an eyewall is present, it is not resolved at this resolution

Section 4. In my view, the biggest source of difference between TC activity in the two models comes from the storm tracker. Despite the threshold adjustment table 3 (which is quite small), the trackers such as used here (or in TempestExtremes) are generally going to miss the weak storms in the low resolution models. Trackers such as TRACK show much

higher storm counts in low resolution models (see Roberts et al.). So while the improvements in MJO and GPI are interesting, it is hard to claim that they are responsible for the higher TC counts when there is such a strong dependence on the choice of storm tracker. You may consider shortening these sections.