Review for "Stratospheric gravity waves excited by Hurricane Joaquin in 2015: 3-D characteristics and the correlation with hurricane intensification,"

By Wu, Hoffman, Wright, Hindley, Alexander, Kalisch, Wang, Chen, Wang, and Lyu.

Summary:

This paper uses a WRF model simulation of Hurricane Joaquin (2015) to assess changes in the properties of gravity waves radiating upward through the stratosphere during the intensification and the steady state phases, and whether observations of these waves could be used to diagnose intensification or weakening.

This paper has several major flaws and many minor ones, and it should be rejected.

Major Comments:

1. I don't see this paper as sufficiently original from Wu et al. (2002). It uses the same very short and poor quality (see below) simulation of Hurricane Joaquin to draw many of the same conclusions. The additional findings about the changes in the properties of the gravity waves between the intensification and steady state changes are new, but some of them are not convincing.

2. The WRF model setup has several strange aspects. First, the minimum resolution of 4 km is on the outer edge of what is believed to be good for simulating rapid intensification. Second, the domain sizes are pretty small (actually not stated in this paper). Third, it uses one-way nesting which makes no sense at all, because 1) in today's computers it is just as easy to run two-way nesting as one-way nesting, so why not do it? And 2) because then the outer boundary condition for d02 is fixed by d01, and gravity waves will not be able to properly radiate out of d02 because of mismatches with d01 (which is like a very similar but still different simulation of the same hurricane). Fourth, the Kain-Fritsch cumulus parameterization is activated on the d02 (nested 4km) grid as well as on d01, which is hard to understand, and defeats part of the purpose of having "cloud-resolving" resolution. The paper states that KF on d02 was used to make the simulation match the best track intensity more closely, which means to me that they had a poor intensity/track simulation without KF on d02, but then discovered that it matched better with it, so they used it. This is also important later because it is not clear if they are accounting for the KF heating tendencies when the compute "HR."

3. The statistical analysis of the time series correlations does not account for degrees of freedom (DOF), and in fact, it artificially inflates the DOF. By making a series of highly overlapping 6-hour time series (each shifted by 6 minutes), you are giving the appearance of many independent data points, but because they are overlapping they are all relying on the same information. In reality there is one time series and even it has less DOF than the number of grid points, which can usually be estimated from the autocorrelation of the series.

4. The author list is suspicious. First, considering the overall effort of the paper, which is statistical and mathematical analyses of the output of a simulation that was previously performed, the author list is strangely long. As required by this journal, the authors provide a section at the end which describes the contributions of the authors. First, it's not clear whether the second "XW" listed is Xue Wu or Xing Wang; I get the impression from the text that it is Xue Wu. Second, two authors of the paper, YW and DL, are not even listed in this section! (Or maybe three depending on Xing Wang.)

Minor comments, by line number.

214-218: GWI should be more carefully defined, especially including what area it is computer over, in both d01 and d02.

222-224: "Maximum heating rate" is not defined and may be not physically meaningful. First, the mathematical expression shown $\partial T/\partial t$, is local (Eulerian) derivative with time, and is only poorly related to moist heating. Second, the "maximum heating rate" sounds like the maximum at any one point, which should not be expected to be physically meaningful because point values can vary wildly in magnitude and location from moment to moment. Something like a volume average over the core of the storm would be meaningful.

263: "more intense and stronger" – redundant

258-260: The fact that changes in MSFCW can precede changes in heating rate is suspicious, and may be caused by the statistical issues noted above.

358-365: It's not clear how the bulk values are computed. Are they storm relative, following the center? Is it just entirely in d02?

375: Here, and repeatedly before, the authors correlate convective "activity" with the properties of the GWs (narrower and faster for intensifying phase). But I am sure they know that what controls their vertical propagation is their wavelengths, and that comes from the horizontal and time scales and the shape of the heating, not from its "intensity."

416: "…treated as a 'black box' in this study." But you have the box! You have the model output and the heating. If you could relate the changes in the structure of the convection, i.e., the individual updrafts as seen in the simulation, to the changes in GW properties, that could alleviate Major Comment #1.

Overall, I would like to say that the authors claims in this paper, that GW properties above TCs change over life cycle and intensification rate, may very well be true. Along with addressing all of the issues above, better simulations and more cases are needed to make a convincing case for publication.