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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Effects of Sea Spray and Atmosphere–Wave Coupling on Air–Sea Exchange during Tropical Cyclone" *by* Nikhil Garg et al.

C. Fairall (Referee)

chris.fairall@noaa.gov

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This paper addresses the issue of sea spray effects using a coupled atmosphere-wave model. The advantage of using a wave model is that sea spray production can be linked directly to the wave properties. The effects of sea spray are implemented for momentum, sensible heat, and latent heat fluxes. The spray production parameterization is a fixed droplet spectrum scaled by whitecap fraction (this follows most approaches in the literature). The major advance in this paper is the use of Anguelova and Hwang's whitecap parameterization that is based on wave properties (taken from the wave model). The thermodynamic effects are implemented following the Andreas

approach. A net droplet heat flux is produced by integrating evaporation rates over the spray flux spectrum. Feedback effects also follow Andreas by fitting the difference in the COARE2.6 model vs observations from the HEXOS field program. The approach is tested via hindcasts of Hurricane Arthur.

I think the authors are basically on the right track. The results for whitecaps are much more credible than the old MOM80 formulation. * One major flaw of this work is the use of the Andreas approach for sea spray. It is based on questionable assumptions – COARE2.6 is purely interfacial so any residual is due to spray. Furthermore, the data used to arrive at the coefficients has essentially no observations past 20 m/s. Thus the actual differences in observed vs COARE2.6 fluxes is mostly noise. The formulation of the feedback coefficients might be ok, but the values are non-physical. There are more physically based formulations of feedback – Bao et al 2011 or Mueller and Veron 2014. So the results given here lack credibility. *Another thing that is not explained is the difference between Expt1 and 2. Why does this change lead to a stronger hurricane. On 26 they claim the change from 1 to 2 'increases the surface roughness, which results in intensification of the hurricane'. I find that strange – it is the opposite of conventional wisdom and needs to be explained. Also, the modest increases in windspeed lead to a doubling of sensible and latent heat flux (fig 15) for Expt 1 to 2. I don't understand that. This aspect needs a lot more explanation. *The examination of the sensitivity to spray focusses on heat fluxes, which have never been measured in hurricanes and therefore can't be verified. I suggest they could focus on the near-surface air temperature and humidity, which have been measured (e.g., we Zhang et al. 2017). *Comparisons of sensible and latent heat flux for Expts 2,3,4 suggest that spray has negligible effect on the thermodynamics. However, it is clear from fig 12b that much less spray is produced by the author's model compared to MOM80 in Andreas. I think it would be interesting to see a comparison of the total spray mass flux as a function of wind speed (this paper vs MOM80). Perhaps these could be compared to the laboratory data of Suslow et al. 2016. *On a more editorial subject, I think the description of whitecap fraction and spray function parameters (sections

5.1.1 and 5.2.1) should be moved to section 2 since they are not part of the hurricane simulations.

So, based on this I cannot recommend publication of this paper in its present form. I think it needs a lot of work.

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