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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Improving our understanding of wind extremes from Bangladesh tropical cyclones: insights from a high-resolution convection-permitting numerical model" *by* Hamish Steptoe and Theo Economou

Anonymous Referee #2

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This paper strikes me as technically interesting, but scientifically confused at worst, or badly explained at best. The authors have done ensembles of high-resolution simulations for 12 tropical cyclones, developed a statistical model to produce smooth maps of peak gust speed, and produced hazard maps that give the probabilities of given gust speeds conditional on a tropical cyclone making landfall in Bangladesh. The most thoroughly described aspect of the work is the statistical model. But the other essential aspects of the study are too deficiently explained for me to recommend publication. It might be possible to turn into a good paper, but the authors need to make clearer what

they are doing and why.

Major comments:

1. What is the study trying to achieve? Why is the problem formulated this way — what is the value of wind hazard maps conditional on a TC making landfall somewhere (anywhere) in Bangladesh?

In the introduction, the authors seem to be thinking about applications involving risk assessment, that is, knowing the long-term probabilities of a given hazard occurring. But for that application, one wants information that is not conditional on any short-term forecast; one wants the overall unconditional hazard. For this, 12 observed storms are not enough, I'd think, even with 9 ensemble members for each; one would want to span the whole space of possible storms. This normally involves looking at large numbers of synthetic storms, as in the catastrophe modeling done in the insurance industry.

Then, in the conclusions, the authors seem to be thinking about applications to short-term forecasting, rather different from the applications suggested earlier. But in that case, one would have an actual forecast of the specific storm that would be coming. Why would one want a general forecast only conditional on a TC anywhere in Bangladesh? Surely wind maps from a past storm whose center made landfall on the westernmost part of the coast, for example, are not relevant if the actual storm is heading to the easternmost part of the coast; but the maps produced here include both equally. It makes no sense to me.

I think the authors need to explain up front what problem they are trying to solve and why they are trying to solve it this way. I cannot actually see what useful problem justifies this particular approach. I might be missing something, but they need to make the argument more clearly.

2. The simulations are not described nearly enough. We know only the grid resolution. We aren't told where the domain boundaries are, the length of the integrations, etc. The

reader is referred to Steptoe et al. (n.d.) for this information. I don't know what "n.d." means, but this information is not available to the reader that I can see. At least the basics should be in the paper. Also we should see some comparison of the simulations to observations for these storms, so we have some idea how good the simulations are.

3. The statement that the median peak gust speeds simulated exceed those in IBTRACS and ERA5 by 22-43 m/s is shocking — that is an ENORMOUS discrepancy. However the statement also makes no sense, or at least is not explained well enough. What is the definition of a gust in the model, in IBTRACS, and in ERA5, for example? To my knowledge actually neither IBTRACS nor ERA5 includes information on gusts, as normally defined, just "sustained", i.e., 1-min or 10-min average, wind speeds; a gust is usually defined as higher frequency, i.e., 3-second average or such. Is a gust in the model resolved or parameterized? Is it just the instantaneous wind speed at a time step on the model grid? If so, at what time resolution was it saved – every time step (and what is the model time step)? And once we have the answers to these questions, what level of agreement should one expect between the model and ERA5 or IBTRACS, given what I'm nearly certain are the very different natures of these data sets?

More minor, specific comments:

Line 75: How were these 12 storms chosen? Of what are they meant to be representative? What is the strategy here? See major comment 1 above.

Section 2.1: The authors go right into describing this somewhat sophisticated statistical model, but the reader at this point doesn't have enough of a clue what the objective is. "Condense information from all 9 regional model ensemble member footprints into a coherent spatial summary of the tropical cyclone hazard" is not enough. What is the reason to think these 12 storms x 9 ensemble members are representative enough for the purpose here, whatever that is? Can the authors please explain in plainer English what is being done here and why? And how can we determine if the answer we get is good or bad?

Line 147: isn't it a bit disturbing that the results are so strongly influenced by just two storms? Don't we want something more statistically robust than that?

Section 3.1: As in major comment 1, what is the problem being solved here? What can one use these results for that wouldn't be better served by a normal operational forecast simulation (or ensemble of same), which would have details of the specific storm?

Line 184: "and in reality it would be updated..." How so, how much would this change the answer, and again why do it this way?

Line 194: "to be sure they would be identifiable within the low-resolution ERA5 data." I don't understand this at all. Why do the storms need to be identified in ERA5? How is ERA5 data being used, other than to initialize the model? This is totally unclear.

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