# Review: *Fully Coupled High-Resolution Atmosphere-Ocean-Wave Simulations of Hurricane Henri (2021): Implications for Offshore Load Assessments*

## General Comments

This study develops a framework for two-way coupling atmosphere-ocean-waves called C-WFS. It provides improvements over existing frameworks such as COAWST including that the ocean model is on an unstructured grid so that no boundary artifacts from nesting appear at the ocean-atmosphere interface, and the inclusion of non-breaking waves. The study looks at a single hurricane case and compares atmosphere-only, atmosphere-ocean coupled, and atmosphere-ocean-wave coupled cases. The analysis shows little improvement in hurricane track as was expected by the authors (though it appears the C-WFS simulation produces the worst track), but reduces the high-bias of wind speeds over the ocean found in many atmosphere-only hurricane simulations. With that said, the C-WFS model still largely over-predicts wind speeds at all levels and regions of the storm for this case. The paper shows SST differences between a derived SST product (OSTIA) and the simulations with, again, some subtle improvements to the SST field in some instances, but large inaccuracies in others (Fig. 9a and 9d). The C-WFS model largely reduces the surface roughness due to the coupling of the wave model yet wind speeds are reduced in the C-WFS simulation. The reasons for this were not apparent to this reviewer. The only wind energy specific sections consider wind veer over a theoretical rotor swept area and wind-wave misalignment. All simulations showed very poor performance for wind veer, and there were no results for wind-wave alignment in the main part of the paper (the appendix has a figure for a single simulation).

This paper, like the review, is very very lengthy. It would benefit from significant editing or a re-write. Some of the findings seem to be exaggerated in their importance or significance based on what the plots present to the reader. The topic as a whole is relevant to wind energy but the paper lacks sufficient wind energy specific analysis and discussion. For these reasons and the reasons below, I am recommending to **reconsider the paper after major revisions.**

## Major Revisions

This paper is very long and has several sections with excessive amounts of irrelevant information. Overall, the paper is not very well organized, the figures are illegible or inappropriate for the data being presented, and the findings seem to be exaggerated in instances based on the data being shown. There are no glaring “major” revisions, but the list of minor revisions is significant to the point that an overhaul of the paper text and figures is necessary.

Additionally, as the paper stands, there does not seem to be a sufficient link to wind energy as presented in the manuscript. Wind veer and wind-wave misalignment are the two sections that address wind energy concerns and they are extremely weak. Without significant improvements to these sections, it is unclear why this paper would show up in Wind Energy Science.

I applaud the author's design of a new atmosphere-ocean-wave coupled framework and look forward to seeing the results of more cases, and a more rigorous analysis pertinent to wind energy.

## Minor Revisions

- L60 - wave impacts on albedo immediately stuck out to me in thinking "how would albedo affect things below a hurricane in which the clouds would block most light from reaching the surface." Does this refer to the waves outside of the hurricane, or within it?
- L92 - my understanding is that convection-permitting resolutions for the atmosphere are typically 4-km or less, so it is strange to see the use of 5-km grid spacing and tout the importance of convection-permitting resolution. This is also mentioned directly in this paper on L198.
  See also:
  https://doi.org/10.1175/1520-0493(1997)125<0527:TRDOEM>2.0.CO;2
  https://doi.org/10.1002/2014RG000475
- L97-109 - COAWST is mentioned and the model framework is stated to be different, but I'm having trouble distinguishing which of the components are different from COAWST. For example, the regional refinement is clear, but then does COAWST not include non-breaking waves (the sentence begins with "in addition" so I'm led to believe COAWST does not include this either)? In line 105 the framework is said to be more realistic than statistical-parametric models - does COAWST use these for its parameterizations or are we no longer talking about COAWST here? I'd recommend restructuring the paragraph to make clear how the framework is different from COAWST specifically, and then maybe a new paragraph for the other benefits (if needed).
- L208 - 12 km seems much coarser than the ocean and atmospheric resolutions to simulate something that is very small in scale; waves. Could you provide evidence that this is a typical model grid spacing for wave simulations?
- L220-223 - are these supplementary results or just not shown? Please specify.
- L243-244 - makes sense. Do other studies also adjust dropsonde positions like this?
- L244-246 - "shown as blue and colour dots" reads strange. Consider listing the colors you want the reader to focus on (black is, afterall, a color), or changing the shapes of the ones you want the reader to focus on so you can state "shown as squares" or something like that.
- General model setup: GFS is used and it is mentioned that other IC/BCs were tested using reanalysis datasets, so was this the GFS final analysis data that was used here, or was this a reforecast of some sort? Additional clarification would be nice for the boundary conditions of each of the models to answer if this was a reforecast/hindcast or a simulation of the storm. Also, could this model be run operationally or do we lack the necessary boundary conditions for the ocean and waves? I now see on L400 that GFS "reanalysis" data is used. I do not think GFS offers a reanalysis product but instead

considers their product a “final analysis” product. It’s a subtle, but significant distinction. GEFS has a reanalysis, so if that is what is used, it should be corrected.

- L336-349 - I cannot follow this comparison due to the coarseness of the observations in Fig. 6a. The observations and simulations seem entirely different. Consider a different plot for this comparison (vertical profiles seem like an obvious choice).
- L378-384 - this figure seems to show that A matches observations much better than AO and AOW with the ocean model adding a lot of warming to the surrounding environment. The paragraph seems to argue the AO and AOW models are similar to A and simply overestimate the cooling, but the figure shows differently. It is unclear how/where the statistics from Table 2 are calculated but they do not match the eye-test.
- L407 - Can this not be determined from the model as has been done in other studies? This framework is the novel aspect of the study, so showing that the C-WFS methodology is improving on answers due to specific aspects being resolved that other models miss should be included.
- L427-447 - comparisons with these buoys, particularly 41002, depict inaccuracies in the wave model. Buoy 41002 is only 10 grid cells away if I have it right that grid spacing is 12 km. Is it possible that the issues are due to too coarse of resolution in the wave model?
- L456-462 - this paragraph is leading in that it highlights the positive aspects of the “AOW” experiments but so far the improvements appear to be minor and in some instances it looks like AOW isn’t really better at all (or in the case of storm track, is worse). While it makes sense to not include A since there are no ocean-atmosphere interactions, in some cases A appears to be the best or comparable simulation. This paragraph could be reframed to simply state that surface enthalpy flux will be examined for the atmosphere-ocean coupled models.
- L475-477 - Figures 11a and 11e have notable differences so saying that this clearly shows z0 from the AO simulation is solely a function of surface wind speed is inaccurate.
- L477 - “This implies…” isn’t this something that other papers have already stated?
- L490-491 - there are studies that have suggested that surface roughness goes down in high winds such as hurricanes. It might be good to include some references to show that a weakening roughness with higher wind speeds has merit and is not nonsensical given that traditional knowledge of over-water roughness says that roughness increases with wind speed.
- L478-491 - the AOW simulation produces lower roughness *and* lower wind speeds. The last sentence in this section states that lower z0 leads to higher wind speeds. So, how can the roughness decrease be attributed to the decreased winds of the AOW simulation? Also, if you calculated Charnock’s formulation of z0 for these wind speeds, you likely would see lower roughness values since the wind speeds are now reduced. There must be other effects here that are resulting in the reduced wind speed.
- L509-511 - this sentence (and following sentences) is about the boundary layer but the plots are showing up to 150 mb which makes the comparisons with the boundary layer (the lowest sliver of the plot) difficult to follow. The “anomalous” inflow at upper levels described in L526 appears to be the only reason for including such heights in the plot. I’m not sure it’s worth it to sacrifice the clarity of a figure for a paragraph with several

lines dedicated to the boundary layer just for one short sentence about inflow at upper levels that isn't elaborated on or really proven to even be "anomalous."

- L492-526 - this paper is very long as-is, and this section in particular seems to take what could be a couple sentences and turns it into a page of text. Consider modifying for brevity.
- L546 - I'm not sure "wind veer" requires citations of a couple of papers that use it. It is well known.
- L552-555 - are these scenarios (particularly the first one) realistic? What would cause a hurricane to disrupt the connection of an offshore wind turbine? Do hurricanes typically see rapid wind direction changes? Additionally, this section is discussing veer - but both examples are of disconnectivity and wind direction change (which the IEC standards do cover). This section is only introductory, but it is poorly formulated.
- L555-557 - wind veer is the difference in wind direction with height, so this sentence is saying you are estimating wind veer by calculating wind veer.
- L556 - the model setup has 12 levels below 100 m, so is there interpolation being performed to get to exactly 10 m intervals in this layer, or is it roughly 10 m intervals?
- L560-571 - the results here are remarkably poor. Is it possible that the data are not taken from similar locations in the model? Particularly nearest to the eye of the storm, the performance seems unreasonably bad. Is it truly that well-mixed within the simulations?
- L604 - so Sanchez Gomez et al. 2023 was able to simulate reasonable levels of wind veer in a hurricane? How were they able to do this? Did they use a coupled ocean-atmosphere-wave model?
- L606-610 - this comes out of nowhere. The results that were just shown were very poor, but the last few sentences of this paragraph seem to claim that this framework does a good job. It also suggests that all prior studies relied solely on atmosphere-only simulations, though there have been numerous studies that couple atmosphere-ocean and even atmosphere-ocean-wave as has been cited within this paper already. In all, it seems that there is a study that did better at simulating wind veer in a storm (Sanchez Gomez et al 2023), but this isn't elaborated on or used to explain why C-WFS (and the individual members of the system) performs so poorly.
- L639-645 - this information requires citation.
- L642 - misalignment causes more strain; L644 wind alignment causes the turbine to face more severe impacts and is at a higher risk of failure. How do these conflicting ideas exist? There are no citations for such claims.
- L646-650 - the results so far do not indicate that including the impacts of oceans and waves are really "essential" and in some cases the atmosphere-only models appear to do just fine. Furthermore, they do have wave information in the models, it simply isn't coupled. Wind-wave alignment can be assessed in an atmosphere-only model if wave direction is provided as a boundary condition from something like ERA5 or GFS.
- Section 5.2 - this section provides no new insight or results. Wind-wave misalignment is simply discussed but there is no actual evidence supporting the usefulness of the C-WFS framework. Consider adding such analysis (best option) or removing the section entirely (last resort since it is argued that wind-wave alignment is important).
- L684 - a "more stable atmospheric boundary layer" was never actually shown. This was deduced from a lower surface enthalpy flux and lower TKE at a single level, but

atmospheric stability is a function of height and typically shown through atmospheric profiles or the calculation of quantities such as the Richardson Number.

- L686-687 - while the values are slightly closer to observations, they are overall very far off which suggests that there is something inherently missing or wrong in the modeling system as a whole. Is it worth highlighting marginal increases in performance in something that is overall simulated very poorly?
- L687-689 - these sentences are inaccurate and/or worded incorrectly. It reads as if saying ocean coupling isn't included in IEC standards. Additionally, it seems to make the claim that this is a novel finding of the paper.
- L689-690 - are the comparisons to buoy data shown in the paper? How do we know that it's more accurate than the other two models when only the AOW experiment was shown?
- L690-691 - after reading *this* paper, I'm not convinced of the critical risk of wind-wave misalignment. Additionally, both atmosphere-only and atmosphere-ocean models absolutely can simulate wind-wave misalignment through the boundary conditions.
- L711-712 - if the 4 cases in the sea spray study are insufficient for generalizable results, then this study with a single case also must not be presented as generalizable. There is no "limitations" section in this paper although there are certainly many to be mentioned; this is one of them.

## Technical Suggestions

- L124 - not a big deal, but the C-WFS acronym might be helped by underlining the letters for which it represents (<u>c</u>oupled <u>W</u>RF-<u>F</u>VCOM-<u>S</u>WAN).
- L180-182 - similarly here: the "A" experiment wasn't immediately apparent and I was thinking the next experiment would be "B" but it was "AO" and then I had to think a bit before registering "Atmosphere-Ocean." So it might be helpful to spell this out (e.g., "The WRF standalone simulation – representing the <u>a</u>tmosphere only – is named experiment 'A'..."). It also may be worth considering aligning the experiment names with the acronym for the model framework (e.g., "WRF", "C-WF", "C-WFS") so that the "AOW" experiment is clearly the full C-WFS framework.
- L326 - why is this figure mentioned in the paper but included in the appendix?
- Figure 6 - the observations are difficult to compare against the model. Would profiles not be better? The x-axis is also poorly formatted.
- L386-387 - "The area-averaged… Table 3" - this sentence has something wrong grammatically. Possibly at "calculated and OSTIA and"
- Fig 10a never referenced.
- L519 - missing word. Z0?
- General comment: the section titles without punctuation are strange. For example, section 5 reads "5 Implication for Potential Risks…" as if there will be a list of 5 things.
- L560-563 - this refers to Fig 13, right?
- L625 - why is this discussed below? Consider just discussing this here to give context to the reader.