

Review of “Complementarity between Large-Eddy Simulation and Synthetic-Aperture Radar observations to characterise surface wind in an extratropical cyclone”.

Technical note:

1) While the description of the data is nicely made, the sources are not really specified, especially for satellite data. Due to your co-authors, I am guessing, you have used GMF developed by Mouche2019 for SAR observations, and 12.5 km coastal mode of Ascet data from KNMI. Yet several products are existing and I think it would be useful to mention explicitly where the data are coming from. Indeed, a data availability section could be convenient for all the observations you have used in this paper.

Scientific comment:

2) In Section 3, you examine the overall consistency of wind speeds between the observations themselves and the simulations.

In 3.1, you mention a discrepancy of 3 to 5 m/s for wind speeds below 10 m/s, and you notably discuss the effects of land attenuation. It is also worth noting, since you mentioned earlier the calibration of SAR with hurricanes, that the data used for this calibration comes from SFMR data, which winds relies on dropwind-sondes. There is a known bias between buoys-calibrated data and dropwind sondes ones, that might also partly explain some of the discrepancies, despite the overall good agreement. Secondly, for winds below 10 m/s, I think SAR winds rely solely on co-polarized signals, (due to the lower signal to noise ratio of cross-polarized signal). Wind retrieval from co-polarized signals has more dependencies, like with wind direction, which has more ambiguities for low wind regime (Chou2013). All these are of course just details, but they also might explain part of the differences observed.

Finally, I'd like to get to my main point, regarding strong winds. There's an important factor that may have been overlooked in the discussion of your results: rain contamination. Looking at your Figure 3c, distinct bands of rain in the south are of course displayed, but what interests me are the small cells present in the western part. This is not sure, given the size of the figures, but in your western box, there appear to be rain signatures within the wind field that could also explain the higher values observed in the SAR data. As you know, C-band data are sensitive to rain, with complex patterns that include both amplification and attenuation effect on wind retrieval (Mouche 2026, doi: [10.1016/j.rse.2025.115139](https://doi.org/10.1016/j.rse.2025.115139)). In this situation, it appears to have some amplification, so I was wondering if you have looked at the difference between the co-polar and cross-polar sigma0 values, as rain effect can differ? Part of the underestimation of the strongest winds made by your simulation may not only come from your model scheme but also from rain contamination within data; at least in West box, where stronger differences are observed.

In this respect, I was also wondering if part of the noise observed in your south box, for your analysis conducted in section 4 (figure 8b) could arise from this issue, and explain the more ambiguous results you observe in your score for streaks dominance?

3) C_d is paramount parameter to characterize heat & momentum flux, and you have looked in section 3, the influence of your parametrization in your wind validation. Did you also check this influence on the streaks signature simulated by the LES, especially in the West box zone ? Extratropical cyclones are of course different from hurricane, yet they can both generate different wave-field according to quadrant location, especially in the north-left front, i.e. west- box for you, (Holthuijsen2012, doi: 10.1029/2012JC007983 & Ponce de Leon2014, doi: 10.1016/j.ocemod.2014.07.005). Could the boundary layer rolls simulation be also affected by different sea state regime in this area? As C_d in left front can be drastically different in this region, and as we are speaking here of fine scale processes.

4) In your figure 4a), while you are focusing on few first km in the rest of your manuscript, there is still a region of strong discrepancies, that you did not mention in your text, near the top of the troposphere. Is it rising from boundary conditions? I am not sure in your figure, if your simulated vertical profile is one simulated exactly at the location of the station, or averaged in certain box around this location.

5) Overall, all the figures are nicely made and have comprehensive captions. I would just suggest for your figure 4a) adding a grid, to ease the reading of the vertical axis.

6) Finally, in your conclusion you have stated line 584 : “The limited number of Wave mode SAR acquisitions, [...], remains a significant constraint for systematic analyses of extratropical cyclone events”. Yet SAR constellation capability has drastically increased in recent years (since 2022), this could possibly mitigate this previous shortcoming for future cases, isn't it?