

Review of os-2021-53

## Variability of surface gravity wave field over a realistic cyclonic eddy

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The manuscript has much improved since the previous submission.  
major comments:

- Some paragraphs are rather long and it would help the reader to better introduce them.
- section 6 is kind of a paper in it self. I wonder if extending to a broader spectrum and turning on the  $S_{nl}$  terms does counteract the statements in the previous section (ability to reconstruct  $\text{grad}(H_s)$ , and L 311f).
- section 6: Why is only the non-linear term discussed and not the dissipation term. strongly enhanced wave-steepness will have to lead to significant dissipation as well, which would further erode the signal for potential inversions. The authors say they performed these simulations but did not say what their impact was.
- L 34f: I think this statement is not well qualified. The effect of current on wave statistic is still local and hence the potential impact on air-fluxes as well. I suggest revising the statement

A few additional comments to clarify the outcome of. please see below.

- L 203 ... , for shorter ...
- L 269. Why is this a Monte-Carlo simulation? What is a Monte-Carlo tracing simulation? Please revise, cite, or explain.
- eq. 6: If there is an actual derivation of this formula, you may want to put it in the appendix
- L 377: are the slopes in the figure? What Are the numbers in the brackets?
- L 379: I don't understand that sentence
- Section 5: This section is likely worth keeping but I would recommend restructuring it. Please introduce better why this is relevant
- L 455: What happen when these terms are turned on?
- L 492: I think you mean the current has very strong vorticity gradients?
- L 506: where does this come from? This should be also mentioned and cited in result section when it is derived.
- L 518: null  $\rightarrow$  small
- L 521: ... And the potential effect of the non-linear wave-wave interaction probably as well