Dear Editor,

Please find attached the third review of our manuscript, “Variability of surface gravity wave field over a realistic cyclonic eddy”. We want to thank the two referee for their remarks which improved significantly this paper.

Please find below all the detailed corrections (in red).

First referee:

Usually "regardless" –> "regardless of", "downstream" –> "downstream of" (if followed by an object). European/French(!) spelling of "metre" please.
It has been corrected throughout the paper.

Line 17. Better ". . bias; using a coarse-resolution eddy field may severely underestimate . ." ?
It has been edited.

Line 20. "at the superficial" –> "in the surface"
It has been corrected.

Line 52. Perhaps ". . more significant for an eddy field with dynamics in the meso- and the submesoscale range . ." ?

Changed by: We demonstrate that wave field characteristics are strongly modified by the presence of the eddy and that those changes are even more significant for an eddy field with dynamics in the meso- and the submesoscale range.

Line 53. "rather" –> "rather than"
It has been corrected.

Line 134. ". . evolution of the significant wave height Hs and . ."
It has been edited.

Line 203. ". . previously, the shorter the incident waves, the lower . ."
It has been corrected.

Lines 214, 215. ". . 95th percentile of . ."
It has been corrected.

Line 336. "Inverse" –> "Inverting"
It has been corrected.

Line 364. "privileged" –> "preferred" or "prevalent"?
It has been corrected.

Line 365. Omit "the part of"?
It has been removed.

Line 392. "which make emerge ∇Hs" is unclear; maybe "causing non-zero ∇Hs"?

It has been corrected in agreement with your proposition.

Line 421. Better ". . 1 m; actually, . ."
It has been edited.

Line 423. Omit "of the values"
It has been removed.

Lines 433-434. Omit "provide" or "put in place"
I removed put in place.

Line 478. Better "1985); it" or even a new sentence.
I did a new sentence.

Line 493. Better ". . flows, consistent with . ." (avoid "which" if not adjacent to object referred to)
I did two sentences and removed the which.

Line 504. "Redo" –> "Re-doing"
It has been corrected.

Line 505. Simpler ". . would improve their . ."?
It has been edited.

Line 509. "inverse" –> "invert"
It has been corrected.

Line 521. "fact" –> "assumption"?
It has been corrected.

Second referee:

Some paragraphs are rather long and it would help the reader to better introduce them.
• section 6 is kind of a paper in itself. 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 grad(Hs), and L 311f).
This section has been added in agreement to one referee remark during the first revision of this paper. I added a sentence in the section (3.5) to answer to your comment.

"Also, because the wave-wave interactions modify the intensity of the grad($H_s$), it would be interesting to characterize again the proportionality between grad($H_s$) and the vorticity of the flow (Eq.6). Without doing the scaling the wave action equation while considering the wave-wave interaction source term, as performed inVillas Boas et al. 2020, the relationship between the grad($H_s$) and the vorticity of the flow reveals a drop of the R2-score to 0.42 (not shown, we recall that R2$=$0.67 for simulations without wave-wave interactions)."

Also, for your information, I verified the proportionality (6) in the case of realistic simulations in the Agulhas current where the source terms (Sin, Snl, Sdis) are activated. In those simulations, the proportionality is still verified with a $r^2$-score equal to 0.53.

• 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.
In my oppinion, verifying the effects of all the source terms would be very interesting to qualify and quantify their individual effects on wave-current interactions in this idealized framework, but it would necessary to redo the numerical framework with idealized (or realistic) wind field and focusing on the breaking parameters of the sea-states. The choice of focusing on the effect on Snl was principally to answer to the previous referee. I added a paragraph in the section (3.5.2) to answer to your comment.

"This preliminary work on the effects of the source term $S_{nl}$ on the wave field in a realistic eddy field has shown that wave-wave interactions modify the wave field in a current field with strong current gradients. Those nonlinear interactions led to a significant change of wave parameters in the whole domain with the tendency to smooth wave parameter gradients. This work could be extended to other source terms such as $S_{in}$ (describing processes of wave generation due to wind) or $S_{dis}$ (describing a great number of processes of wave dissipation). As an example, we showed in the section 3.4, that the wave steepness ($\mu$) is strongly modified due to the presence of the eddy field. These changes of $\mu$ ccould lead to an increase of the probability of breaking, subsequently leading to a strong dissipation of the wave energy. It would have large consequences on the potential inversion of the wave signal to estimate the statistic of the underlying current (Eq. 6)."

• 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

I revised slightly this paragraph :

"Surface currents seem to increase the deep-water breaking wave probability and the related air–sea fluxes (Romero et al. 2017, Romero et al 2020.) The reader can refer to the instantaneous numerical outputs of Romero et al. 2020 and notify the local effect of the sharp current gradients on the simulated whitecap coverage (see Fig. 5d and Fig. 5i of the same reference). Wave breaking at the air-sea interface is the major source of momentum and heat exchanges between the atmosphere and the ocean (Cavaleri et al. 2012) or gas and sea spray production (Monahan 1986, Bruch et al. 2021). Therefore, surface mesoscale and submesoscale currents have a significant impact on air-sea fluxes (momentum, gas, heat, sea-spray, ...) through their interactions with the wave field.".

A few additional comments to clarify the outcome of. please see below.
• L 203 ... , for shorter ...
It has been edited.

• L 269. Why is this a Monte-Carlo simulation? What is a Monte-Carlo tracing simulation? Please revise, cite, or explain.
I added an additional sentence to describe the method and some reference as example.

• eq. 6: If there is an actual derivation of this formula, you may want to put it in the appendix

The proportionality is partially described in Villas Boas et al. 2020 and in the supplementary material of the same reference. A future publication is in preparation which will demonstrate this equality analyticaly with a analogy with the Synthetic Apperture Radar imagery technique.

• L 377: are the slopes in the figure? What Are the numbers in the brackets?

It has been clarified in the paragraph starting L.377.

• L 379: I don't understand that sentence
It has been clarified in the same paragraph L.377.

• Section 5: This section is likely worth keeping but I would recommend restructuring it. Please introduce better why this is relevant
Thank you for your interest about this section. The envidence of the relevance of this section has been improved at the beginning of the section (3.4) and the section has been reorganized.

• L 455: What happen when these terms are turned on?

I edited the end of the paragraph as follows:

"More detailed studies will have to be conducted such as with the activation of the other source terms. For instance, activating the wind input source term with a given wind field, will have an effect in the high frequency band of the wave spectrum (the development of a wind sea) which, subsequently, will interact with the current field. Also, the presence of both wind and current will modify the wind work at the surface of the ocean. This work is function of the difference between the wind speed and the surface current speed. This relative wind will modulate the wind growth and therefore the wave height in the current field. It would be interesting to scale spatially the effect of this relative wind on the wave parameters ($H_s$, $T_{m0,-1}$). Also, considering the wave dissipation source term will constrain the wave energy in the domain, especially in the areas where the wave steepness are very sharp (Fig. 8)."

• L 492: I think you mean the current has very strong vorticity gradients?

The vorticity is still a gradient. However I edited this sentence as follows: "These changes are more pronounced where the underlying current has a stronger vorticity."

• L 506: where does this come from? This should be also mentioned and cited in result section when it is derived.

I rephrased it as follows: "Following the relationship introduced in Villas Boas et al. 2020 based on the balance between wave action advection and current-induced refraction, the significant wave height gradients normalized by the incident wave frequency has been described as a function of the surface current gradients. "

• L 518: null -→ small
It has been edited.

• L 521: ... And the potential effect of the non-linear wave-wave interaction probably as well"

I added your proposition.

"I think that the manuscript could be of value to the community. After a third revision, the manuscript still needs substantial revisions. Several paragraphs are not well structured, and some are one sentence long. Similarly, the paper organization could be improved by using the typical format: introduction, methods, results, discussion, and conclusions. Below I provide addtional comments and suggestions.
I modified the form of the paper. Thank you for this advice.

Abstract: Specify the range of scales investigated. “Fine scale” means different for different readers.
It has been clarified as follows: "The study of these simulations illustrates how waves respond to the numerous kinds of instabilities in the large cyclonic eddy from a few hundred to a few tens of kilometers."

Equation (6):
The slope_{KE} factor of the proposed scaling by Villas Boas et al. 2020 is the reciprocal (1/slope_{KE} ) to that in equation (6) and Figure 7. In other words, slope_{KE} should be in the denominator on the left-hand side of equation (6). I suggest revising the text and the results in Figure 7, accordingly. Also, comment on the outcomes. Specifically, is the 1:1 relationship in Figure 7 lost after correcting the factor?
Indeed, this is an mistake from my self. Thank you for this review. This mistake was not in my computational diagnostics and in my numerical simulations. All the figures have been corrected as well as the associated text.

Figure 7: The y-label is missing the frequency (\sigma)."
The figure has been edited.

Gwendal MARECHAL