We really want to thanks every referees that allow to produce a new version of the manuscript scientifically and grammatically improved. We tried to highlight a better key message. Every elements which were not accurately discussed in the first version are now supported by references, additional figures and examples.

Most of the paper has been rewritten so we advice the reviewers to read again the full text before look at the answers of the first review. Although the answers are given below, some of them (principally minor remarks) are not really answered because of the new structure of the manuscript.

We hope that reviewers will enjoy this new version of the manuscript. We sincerelly thank them for the time that they will dedicate for the second review.

Best regards.

Gwendal MARECHAL (and co-author)

Asnwers to referees :

Colored text are answers.

##
Referee #1
##

- Title: it is not clear if the study will focus on the scale of the eddy. Reading the manuscript, indeed, it seems that a real distinction between mesoscale and submesoscale is not done, and the eddy is taken as a whole. I suggest, for the title, to focus on the actual subject of the work, that is the variability of the wave field over a realistic cyclonic eddy.

The title has been modified.

- Line 2: the wave amplitude is mentioned here, while later (Line 13) the wave height is used as the reference vertical scale. Probably, given the results presented in the study, the use of wave height is more appropriate.

Wave amplitude has been changed to significant wave height.

The following mistakes have been corrected.

- Line 17: an “and” between the two references is probably missing

- Line 17: wave height == > significant wave height

- Line 23: gaz == > gas

- I have not a particular suggestion on how to improve the Introduction (section 1), but at the present stage, it seems to be more a list of results instead of a place where briefly introduce the

study in a broad context and highlight why it is important. My suggestion is for a reshaping of this section, to focus on the current state of the research field and key publications.

We have improved the introduction. After the long state of the art of how current affect waves we showed that studying a more realistic shape of eddy will have a significant impact on the Sea-Level Anomaly from space but also on waves forecast.

- Section 2.1 title. I'd rather say: "A realistic cyclonic eddy" (see also my comment below).

In agreement with referee 3 we proposed a new title for this section.

- Line 62: define the variable Bu.

It has been defined.

- Lines 69-70: here is mentioned the duration of "half a year", while later (caption of Fig1) the duration is 210 days (more than half a year). Please homogenize and keep throughout the whole paper the actual value used for the simulations.

« Half a year » has been corrected by 210 days.

- Fig. 1. Since the current field (speed and direction) is relevant for the wave model, I'd add two panels showing this variable, which will largely help readers to interpreter the changes in wave parameters.

The current intensity/direction have been added in two new panels in Fig.1

- Fig. 1. what is the meaning of f_0?

It has been clarified.

- Fig. 1. Since the current field is not stationary, is there any reason to choose that specific interval (210 days) after initialization? To me, it seems an arbitrary choice that influences the results and must be carefully motivated in the text. Please add a comment, also about what authors mean for "final state of the simulation" (Line 69).

We gave more informations to explain why we have chosen this current forcing and not another one. It has been clarified in agreement with referee #3

- Figs. 1, 2, and 3. To understand the effect of the eddies on the wave parameters, a comparison with the undisturbed wave field (i.e., no current) is necessary at this stage.

Instead to overload the manuscript, with too much figures, a line has been added in caption of Fig.2 and Fig3 . The difference mean period between simulation with and without current has replaced the instantaneous mean wave period in this new version of the manuscript.

- Page 4. It is defined the surface current, but waves “feel” a wave-averaged current even below the surface. Probably it is not necessary to change the formula, but it is important to specify how waves behave over a realistic current field and that the use of the surface current is an approximation of the real process.
It has been clarified in the wave model setup.

- Fig. 1 and other Figures labelled with X- and Y-axis. Since in the text geographical coordinates are used (i.e., west, north, longitude, …), I suggest placing them together with labels X and Y on the axes specifications.

« Longitude » and « latitude » have been removed, « west » and « east » as well. We have re-write the manuscript such that paragraphs are consistent with figures axis.

- Page 5. It is not clear how simulations were performed. In particular:

What kind of “narrow band spectrum” was used?

It has been clarified (gaussian in frequency)

For the three next remarks :

We have redo all wave simulations such that a new wave train is propagating in the domain every hour. Thanks to this new parametrization waves reach a stationnary state for all initializations (Tp=7, 10.3 or 16.6sec). Details of the new parametrization are given in the paper.

Were simulations initialized with waves travelling from left (west) to right with no boundaries conditions (see the next comment on figures showing the results)?

If so, simulations do not reach a stationary condition, therefore results are representative of a specific time step (as it is mentioned later; indeed, at the given time steps, Hs at X < 100 km is zero, as well the wave period): does this selection affect results and conclusions? Mind also that, because of the different current fields between unperturbed and perturbed simulations, the two wave fields (for a given Tp and time step) do not necessarily correspond to the same “state”.

Would have changed the conclusions if, alternatively, the simulation had been done by forcing the field from the boundary and then by trying to reach a steady state for the interior wave field?

- Line 117 “the intensity of the current has been multiplied by five”. Does the artificial increase of the speeds cope with the assumption of “realistic cyclonic eddy”?

In term of surface current intensity, multiply by five the initial vortex make the new vortex still realistic. Nevertheless, as noticed by the referee #1 the vorticity do not remains realistic.. We thus multiplied by 2 in stead of 5 the initial vortex of de Marez et al. 2020. The new intensity and vorticity are given in Fig.1.

- Line 122. Cg is the speed of the wave energy.

It has been clarified.

- Line 124. Longitude, without geographical coordinates specified in the Figures, is meaningless (see my comment above).

It has been corrected.

- Line 152: it is not easy from the Figures inspection to appreciate the gradient. A new Figure showing this variable would help. As well it would help a new Figure showing the relative differences between the simulations with different eddies. I let, however, authors decide how to improve the presentation of the results.

The initial Fig 2g has been extended to the mean wave period (Fig.4g). Wave parameters gradients have been described more accurately by adding some indications in the caption of Fig.2, Fig.3 and Fig.4.

- Line 162: what do authors mean by “spurious”? It is shown a value of 360, while it should be 270.

It was due to the fact that only one wave train was propagating in the eddy field. That is why when the unique wave train have propagated over the surface current field the wave direction becomes 0° (=360°) . With the new parametrization this large yellow band has been removed because waves are continously emitted from the left boundary.

- Line 265: I wonder how one can obtain the Hs field over such a large area. A comment on the available (or planned) instrumentation would be appreciated. It has been clarified.

Moreover thanks to the new parametrization the conclusion of the paper has changed. A remark on high resolution wave height measurement from space has been added.

##########

Referee #2

##########

A comparable scaling in terms of the peak wave direction was proposed by Villas-Boas et al. 2021 and used to infer the current gradients. Neither method can accurately invert the current gradients, which is mainly because the effects of refraction are non-local. However, the present work would benefit from applying the scaling of Villas-Boas et al. 2021 to invert the current gradients and comparing them to the current gradients estimated using their method. Also, it would be helpful to apply the current gradient inversions not just for the 7 s waves but to the other cases. Overall, the manuscript needs substantial revisions. Several paragraphs are not well structured. Below I provide several specific comments/suggestions to improve the manuscript.

The relation proposed by Villas Boas et al. 2020 between surface current gradients and significant wave height gradients have been here demonstrated step by step, some lines have been added in the Appendix. The final relation presented in equation 8 is the same than the one proposed by Villas Boas et al. 2020 but in another form. We highlighted that significant wave height gradients are proportional to the wave steepness (kHs), making among other, non linear wave-wave interactions crucial in the intensity of significant wave height gradient. Thanks to referee #3 we have proposed a complementary numerical experiment to show the contribution of the non-linear wave-wave interaction source term in comparison to the results proposed in a very idealised case (S=0). The scatterplot proposed in Villas Boas et al. 2020 (see their Fig.12) have been extended to our study in Fig.7 (for all initial frequencies and with the contribution of the divergence of the flow). The non-local aspect of the current on waves have been even more highligted thanks to the new parametrization of the wave model (waves are emitted from the left boundary continuously every hour rather than studying a unique wave train). The signatures of an isolated eddy on wave parameters reveal that the modulation induced by current on waves have a strong effect downstream the eddy field making the wave parameter inversion to infer current gradient limited.

The following typos have been corrected and expressions reformulated. Some remarks have not been taken into account due to reformulations but most of the proposed semantic have been included. The body of the manuscript has been highly modified so certain expressions have been entirely re-written.

1: small scale --> small-scale. Modified.

8: The word “retrieved” is overly optimistic based on your analysis. You can at most “identify” the current gradients. Retrieve has been removed and replace dby infer/approach/identify

23: gaz--> gas Corrected

26-28: This paragraph is very short. It could be improved. It is unclear what is the paragraph trying to convey. Is it about mesoscales or submesoscales? it has been rewritten.

41: “anticipation” do you mean prediction? It has been changed.

43: “?”? It has been removed.

47: Reads “…numerical model built from Ardhuin et al. (2017) without source terms.” What does this mean? Is basically WAVEWATCHIII without source terms. What is the relationship to Ardhuin et al. 2017? Please revise. It has been revised.

56: Parameterization of what? Please explain. We add some lines of the model prametrization where we described how the current field has been obtained.

62: refer --> refers. It has been corrected.

63: Delete “During the simulation,”

92: “. g” --> “, g” It has been corrected.

100: “performances” --> performance. “have” --> has. It has been Corrected

99-102: The studies cited used the source terms (wind input, etc). It is worth specifying. It has been changed.

109: “Indeed, dealing with high” --> High. “allows a better” --> is required for a

110: current --> currents. Sometimes ’’current’’ are wiser but most of the time ’’surface currents’’ has been used.

114: “the wave one” --> that of the waves. It has not been taken into account. We have reformulate the sentence.

122: There is not need for a new paragraph. I suggest the author revise their use of paragraphs. In several instances the paragraphs are too short. We tried to better define paragraphs according to message in each of them.

122: “are propagating in the current” --> propagate. Also the next sentences (“T_p= 7 s…than longer wavers”) can be replaced with “For T_p=7, 10.3, and 16.6 s the corresponding group velocities are 11, 16 and 26 m/s.”. It has been modfied.

126: “modulate amplitude” --> “modulate the wave amplitude”. It has been edited.

127: “respond” --> variabililty. “waves” --> wave. “for a prescribed underlying current” --> such. It has not been taken into account. We have reformulate the sentence.

Figure 2g – line colors would help distinguish the lines better. Figure has been modified.

132: “Wave train is propagating” --> The waves propagate. It has been reformulated.

148: “occurring” --> apparent. ’’Occuring’’ has been changed to ’’apparent’’.

150: “actions”--> heights. “is”--> Typos has been corrected.

163: “is function of both” --> depends on It has been corrected.

165: “with perturbed” --> with the perturbed The designation of the two eddy forcing has been modified.

166: insert “the” at the beginning It has been corrected.

167: “the current was turbulent” --> turbulent the current is ->It has not been taken into account. We have reformulate the sentence.

169: “trajectory” --> trajectories. "current” --> currents. It has been corrected.

171: Add “(Kenyon 1971; Dysthe 2001)” at the end. It has been added.

182-184: What does this mean? Please be more explicit in terms of the physical processes.. → It has been clarified

185: what do you mean you "guess". Please choose a better scientific word. Also, the text reads “… would be more impacted…” please elaborate more impacted relative to what? We tried to be more scientist in the choose of words.

186: is --> are. Typo corrected

197: Does not make sense. Please revise. It has been corrected.

200: “wave field” --> the wave field. Train --> trains : The typo has been corrected

201: “kinematic: --> kinematics Corrected and modified by wave energy propagation in some part of the paper.

202: “unperturbed” --> the unperturbed Same remark as above.

203: “initial” --> the initial Same remark as above.

205: “Indeed, the” --> the We tried to remove as much as possible link words like ’’Indeed’’ but some have been kept.

206-207: delete “confirmed by theoretical works performed by” It has been deleted.

261-=263: please revise is not very clear. This part has been rewritten.

263: “Here, knowing” --> Knowing. Delete “spatial”. It has been also rewrittend.

264: “such perturbed” --> an It has been rewritten.

265: delete “that the different”. Replace “that occur in” with “within” It has been corrected

266: “approached” you could instead say infer (?). It has been corrected.

263-265: The closing statement is rather promising but is not fully supported by the findings. The analysis does show that you can detect the presence of an eddy. But the details of the structure are not reproduced, which is in part because as mentioned earlier in the text the main mechanism for the modulation of the wave field is non-local.

A long discussion on this point has been given in the discussion section. For example the limit of Eq.8 to infer current gradient from wave field inhomogeneity.

268: Opening sentence of the conclusion is no clear. Please revise. It has been removed

270: frequency --> frequencies Corrected in all the manuscript.

275: delete “all the”. Replace “shorts” --> short ‘’all the’’ have been removed in the Paper. Short has been corrected.

274-275: You mention the current energy. What about the current gradients. We discussed more about current gradient rather than current intensity.

278: “order” --> terms the use of ’’order’’ in such expression has been removed in the paper.

280: delete “until”. Replace “dynamic” with dynamics It has been corrected.

281: delete “the used of a constellations of: It has been removed.

283: what do you mean by approach? It has been clarified

Appendix: Equation A4 – what is Cte It has been clarified. It is a integration constant which is removed then.

##########
Referee #3
##########

Major comments:

- Choice of a narrow banded spectrum: What is the effect of the narrow banded spectrum on the wave-wave interaction? Broader banded spectra result from ongoing wave-wave interaction (Hasselmann and Hasselmann 1985, and others). If the spectra are limited to a very narrow band, how does the cross-spectral energy flux change this spectrum over time, even without any perturbation? Does it potentially impact the model results?

  A section has been added for this remark at the end of the manuscript. This section is not developed a lot because we wanted to focus only on wave-current interaction in a very idealized framework. We are counting on a second review to know if this section have to be removed or not .

- Sec. 2.1. Even though the model simulations are borrowed from another work, they should be sufficiently described in the method section. Why does it matter the use the full equation of state of seawater? What is the advantage of using this set of equations? The title claims that his simulation is realistic, but it is not explained why. I would suggest that the authors better explain the models' features and advantages.

    - Marez et al. 2019 derive a "composite eddy" this is at least mentioned twice but never explained what that means. If it is a composite of several eddies, how can this be a free model run? How can a composite be realistic and not an average? This needs more explanation.

It has been clarified in the Manuscript, more informations have been added to proved the realistic aspect of the eddy used as model forcing.

- Section 4. I think this is the interesting part of the paper. But it is not well connected to the other parts. I suggest reorganizing the paper such that this analysis is better placed. At the moment, this is neither a result nor really a discussion, it is a deduction on a weak basis:

- Where does eq. (6) appear from? WKB is assumed on what? Would you please state how this relation was derived and where? I see a coherent pattern between both panels of figure 6, but, given this color scale, this is a purely qualitative statement, which similarly appears in other studies.

  The demonstration of Eq.8 has been more accurately demonstrated in the Appendix. We also added in the body of the manuscript the different basic elements to obtain this expression.

- I think the simulations allow for a more quantitative assessment of eq. 6. If the gradients of Hs and U "match" (eq. 6), this should be seen in a scatter (regression) between all pixels in Figure 6 a,b. Since a more rigorous analysis in this section is missing, it is hard to follow the rest of the section, which reads like a discussion of possible analysis but not necessarily of this paper (L241 - 267).

  We have added new Figure and more comments to better describe the performance of the expression. (eq.8)

- In particular, how can one invert for wave height gradients from observations but not for the surface height directly? Why are the altimeters unable to reconstruct this SSH field? I think the authors miss to say in the beginning that the SSH is a (still dynamics) but average quantity that is not directly observed from a single altimeter track. Altimeters observe the total height changes that appear to be dominated by waves. I think this could also be more clearly stated in the introduction.

  As we are not expert of the altimetry we hope that this new version is sufficiently clear.

- I recommend using "initial/ linear" and "fully developed/resolved" currents rather than "unperturbed" and "perturbed" here and throughout the text. Both current fields are perturbations to the incident waves. The linear eddy is somehow a representation of the under-resolved eddy conventional altimeters might see, while the turbulent eddy field is a better approximation of reality. I think this might be a hidden motivation of the authors but is never clearly stated or mentioned except in the discussion. This train of thought should be introduced from the beginning of the paper. Hence the naming of the different experiments is more than just semantic and rather reflects the structural and communicative problems of this paper.

  We have edited the designation of the different current forcing.

- L 167 - 173. What do the authors try to say here? What do you imply? They mention three principles: Random walk, Fermat principles, and \chi/c_g for deep water waves. None of these principles are directly referenced nor explained. If waves behave like in optics, how is it related to a random walk? Or are they just arguments from Villas Boas and Young restated? This paragraph should be revised and statements justified, as well as grammatical errors corrected. I would suggest starting with the last sentence as a topic sentence.

  Indeed, the message was drawn into too much and not explained processed, we removed/edited this part.

- Title: This is to the authors, but I would suggest something like: "Spatial wind-wave variability from (more) realistic meso- and submesoscale eddies"

  The title has been edited as adviced by the referree one.

- L 276 This is not true. You do not give a functional relationship between Hs gradients and U gradients. Eq 6 is a proportionality that is not further accessed, or justified. This statement should be revised or removed.

  It has been clarified in the Manuscript.

minor comments:

All comments has been taken into account, sentences have been corrected, edited and completed.

L 32 "the ubiquity of eddies is no longer proven" what do the authors mean by that? please rephrase.

It has been rephrased.

L 43. "?" something is missing there.

L 74 what does "surface velocity fields" mean exactly. how is surface defined? Both, currents and waves have a complex vertical structure.

Following Referee#1's remark, we indicated what surface current means. What are the real action of three-dimensional current on wave properties at the air-sea interface.

L 94 T_{m0,-1} why this complicated name? what stands the -1 for?

This is the output of the numerical model, we explained more this variables outputs. We chosed this variables more than T_{m0,1} or T_{m0,2} (weighted on higher frequencies of the spectrum) because waves studied in this analysis are long waves (swell).

L 121 I think what the authors mean is that this section analysis the dependence of the wave field on the complexity of the surface currents and the waves peak frequency. And, that longer waves travel faster (c_g= g T / 4 pi). I recommend rephrasing the beginning of section 3.

The new parametrization of the wave model shows a stationnary state for all wave initializations thus this point has been removed.

Fig. 2. The lines are hard to distinguish in panel g. I recommend to use color and show the same colored section in the corresponding other panels to guide the reader. It might be also useful to show the approximate center zero-line of the eddies as a single contour in all panels and all figures to show the position of the mesoscale eddy. caption: use "row" rather then "line"

It has been modfied**.**

L 132 i think "initial" should be "incident. The angle convention is confusing. The direction convention is where the waves are propagating TO or FROM? Is this the mathematical or nautical convention?

The convention has been indicated.

L136 enhancement → increase

It has been corrected.

L 142 Y=[150, 300] I am not sure if this appropriate in this journal. normally this should be spelled out.

Those kind of expression have been removed

L146 Here and throughout the text. I would rather talk about different simulations than modelS, since this supposed to be the same model.

We replaced model by simulations where it was necessary.

L146f first the authors talk about stronger spatial inhomogeneity for the turbulent simulation but then say its similar to the linear case. please clarify.

It has been clarified.

L150f These sentences are hard to follow. what do the authors try to say?

It has been rewritten.

L 155 suggest: at the first order → to first order

It has been replaced.

L155 are turning .. → refract in the current field and turn southward .. and northward.

We used the appropriate word. « Turning » has been kept for the description of the refraction process.

L160 / Fig 3. Large yellow striped should be removed.

Removed thanks to the new parametrization.

L164 ".. is stronger for simulations with a shorter peak frequency (Fig. 3a,d)". No need to repeat three float point numbers over and over again in the text.

As this manuscript shows a lot of sub panels we prefered to be as much accurate as possible in the references of Figures.

L175 This is a methods sentence, I would recommend rewriting. Again, what is the purpose of this section?

We explained why we studied the mean wave period. In the conclusion a direct application is given.

L183 "super position of processes" Be more explicit, don't let the reader hang. Name these processes, rather than diffuse the attention to 3 other publications and this whole manuscript.

It has been clarified. Processes are given accurately

L185 Why are guesses about a fully divergent field are made here? Even though, from my understanding, the currents are mainly rotational? Or is this just a restatement of the Villas Boas et. al results? please revise.

It has been corrected, it was a pure mistake on my part

L201 Suggest: "wave kinematic" → "wave energy propagation"

It has been corrected.

L217-218 I think what the authors mean is that (local) refraction by currents has non-local effects for the wave energy. Please revise.

It has been revised.

L282 "This manuscript shown .. " other work that did similar work should be cited here.

Citation has been added.