

Ocean Sci. Discuss., author comment AC1  
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## Reply on RC3

Gwendal Marechal and Charly de Marez

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Author comment on "Variability of surface gravity wave field over a realistic cyclonic eddy"  
by Gwendal Marechal and Charly de Marez, Ocean Sci. Discuss.,  
<https://doi.org/10.5194/os-2021-53-AC1>, 2021

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**Dear Referee #3,**

**Here we answer to the main remarks and propositions for the manuscript improvement.**

**First, we really appreciated your very detailed remarks, in a global aspect we hope that the goal of the paper has been clarified. We apologize for the typos and hope this new version will be more correct. We are also grateful for some remarks that will expand the potential applications of this paper. A more accurate focus on remote sensing applications have been added. Finally all point has been justified by citation and/or results.**

In the submitted article, the authors use two different surface current fields to force WW3 model using three narrow banded frequency spectra. They analyze the response of surface wave properties to the underlying current field by contrasting smooth currents and fully turbulent currents. The paper's idea is well sounded and of current interest from the remote sensing community. The beginning of the paper is organized well, but the second half has some unsupported claims that I cannot necessarily follow. The relation to the results cannot easily be seen by the reader. In addition, the paper has numerous grammatical errors that sometimes make it hard to comprehend. About 20-30% of the sentences miss a pronoun, mix-up singular/plural, or have other grammatical errors on a sentence level. Further, for some sections, it is hard to identify why these analyses are turned out (sec. 3.1.3, 3.2., 4); even though the authors have probably an idea in mind, they miss to state it clearly. Some analysis and conclusions seem to fall from the sky rather than be referenced, which weakens the paper because it could potentially well outline the impact of wave heights retrievals from altimeters on SSH resolution. The figures are mostly clear even though the main result discussed in section 4 only appears to be qualitative (and hence not that novel) rather than quantitative, even though it could. This paper needs more work such that the abstract, results and discussion of the paper appear to be more coherent. All statements should be justified.

**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 you if you feel that this section have to be removed. If you think it is interesting we will keep it in the final version of the paper.**

■

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  $H_s$  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 advised by the referee one.**

▪

L 276 This is not true. You do not give a functional relationship between  $H_s$  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 rephrase.**

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"

**Very good idea, it has been edited.**

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  $\square$  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  $\square$  to first order

**It has been rplaced.**

L155 are turning ..  $\square$  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 preferred to be as much accurate as possible in the references of Figures. If you still think that it is too heavy we will removed some of them for the final version of the paper.**

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.**