

Ocean Sci. Discuss., referee comment RC3
<https://doi.org/10.5194/os-2021-53-RC3>, 2021
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Comment on os-2021-53

Anonymous Referee #3

Referee 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-RC3>, 2021

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?
- 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.
- 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.
 - 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).
 - 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.
- 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.
- L 167 - 173. What do the authors try to say here? What do you imply? They mention three principles: Random walk, Fermat principles, and χ/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.
- Title: This is to the authors, but I would suggest something like: "Spatial wind-wave variability from (more) realistic meso- and submesoscale eddies"
- 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.

minor comments:

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

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

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.

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 than "line"

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?

L136 enhancement \square increase

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

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

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

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

L 155 suggest: at the first order \square to first order

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

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

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.

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

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.

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.

L201 Suggest: "wave kinematic" \square "wave energy propagation"

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

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