Dear Editor,

please find below my responses (in black) to the referee's comments (in blue). Again I would like to thank the referee for his efforts and constructive remarks.

I would also like to mention that I slightly changed notation in Eqs. (10) and (11), explicitly adding the list of arguments. This makes notation more consistent with the notation used in Eq. (5) but does not affect the content or meaning of the equations. I also added the reference to Staneva et al. (2021), a relevant paper published in these days.

I found the manuscript significantly improved. The objectives of the paper have been clarified and are now more realistic, the descriptions of the model and methodology have been expanded, and the description of results and associated figures are better organized and clearer.
I think that my main concerns have been addressed, and I recommend the paper for publication.

There are still some minor points listed in the following that I would like the author to consider before publication, but I do not need to see the final revision (lines in the following refer to the edited version):

- **Line 10**: The last phrase of the Abstract is not very clear, especially the word "vagueness" does not seem correct: " indicating also vagueness of drift simulations being used." I recommend to delete the phrase or to change it with someting like " indicating that direct backward simulations of trajectories can be very sensitive and therefore unreliable"?

Has been changed to: "… indicating when simulations of backward trajectories are unreliable because of their high sensitivity to tracer seeding positions."

- **Line 54**: Typo: "weakly" instead of "weekly"

Thanks, has been corrected.

- **Line 90:** phrase not clear, may be a verb is missing?

The wording has been revised.

- **Line 110**: "provides" instead of "tries to give"

Has been replaced.

- **Section 2**: I am still confused about using a hydrostatic model to provide estimates of surface convergence. Don't you expect that vertical velocity estimates are not very reliable? Please add a comment.

The hydrostatic approximation refers to vertical accelerations rather than vertical velocities, assuming that these accelerations are substantially smaller than gravitational acceleration. Given the Boussinesq approximation, vertical velocities are obtained from the constraint of three-dimensional divergences being equal to zero. There shouldn't be any problem with these assumptions, in particular when the relevant horizontal scales are much larger than the vertical scales. This is the case for the shallow shelf sea (see Fig. 1). Note also that in this study vertical velocities are never used or addressed explicitly. The FDLD (Eq. 11) is calculated from 2D horizontal velocities. Therefore, I couldn't find a place in the manuscript where it would be meaningful to discuss this issue.

- **Section 3**: I think the presentation is much improved, but there is still some confusion. I think that there should be a brief discussion at the beginning of the Section that explains the rational behind the choice of examples. Are they driven by the type of FTLE structure? Or by the type of wind forcing? Or what else? Also the titles of the subsections "First example" and so on are a bit missleading, in the sense that in some cases there are several examples grouped together… It might be better to refer to the type of example, as indicated above

Yes, I see your point. The very generic titles of subsections have now been replaced by specific titles that summarize the main issue addressed in the respective subsection.

- **Discussion on the choice of tau=250 h**: I think that motivation, rational and sensitivity of this choice should be put up front in Section 3, not at the end and in the Discussion

In agreement with your suggestion, a motivating statement has been inserted at the beginning of Section 3.