RC2: 'Comment on os-2021-80', Anonymous Referee #2, 20 Oct 2021

We thank this anonymous reviewer for the relevant comments. We also propose a point-by-point response, sometimes referring to responses already developed in other reviewer questions above.

This manuscript tackles an important question which is how to separate well the mesoscale variability from the internal tides in the altimetric record, with a focus on the internal tide component which is coherent. The approach is innovative, and the results are tested first on artificial fields, and then on the real altimetric sea level record, with a validation and estimation of the skill with recent data not used in estimating the solution (for the internal tides).

The pproach relies on a set of assumptions on the respective spectral characteristics of the meso-scale variability and the tidal characteristics. The tests are done assuming a certain spectral shape of the meso-scales and tides which follow the classical (linear) dispersion characteristics and are low order (1 or 2, depending on the tidal mode). The tests indicate that with these assumptions, the joint inversion approach (which is numerically rather heavy) performs better than separate approachs.

I wonder whether the authors could go further and estimate how much the gain depends on the spectral shape. After all, it originates from the overlay of the time-space spectra of the meso-scales and of the tides. One can also wonder how sensitive is it to the exact shape of spectrum. It could be interesting to test different shapes overlapping more or less.

This is an interesting question, as the solution indeed relies on a number of assumptions that were necessary to extract specific signals among sparse observations. This is one of the difficulties : we need to implement enough assumptions to have a reduced basis with sufficiently

low degrees of freedom, but this reduced basis must still contain the signals we want to reconstruct, with correct prescription of variance.
We made different tests, in small domains to work with reasonable computing costs. We first verified that increasing the prescribed spectrum for mesoscale tends to overestimate the mesoscale solution, with some leakage from internal tides. And vice versa. We then verified that with a correct spectrum, and with a noise matrix set up accordingly with altimetry noise plus representativity errors, we could minimize the error with respect to independent Altimetry data. This is why we ended up using a database of spectra, varying regionally to treat the problem globally, with noise floor removed as in Dufau et al., 2016, from the AltiKa satellite.
The descriptions regarding how we use the spectra were poor in the first version of the manuscript, we now propose a more detailed version.

What is the impact of the assumptions on spectral characteristics for the mesoscales, as well as for using a specified dispersion relationship with modeal decomposition, extending only to order 2 or 1 depending on the tidal component, of course compounded by the use of a (spatial) Hamming window. The width of this wndow has to have an impact. What fully motivates the choice?
We answered the first part of the question above for mesoscales, and for the internal tides, we also verified that the solution was sensitive to the variance prescription for each mode of each tidal component. However, it is more difficult to have a direct observation of how much energy must be prescribed for coherent internal tides. Although we could use some estimations such as presented in Zaron 2019, based on the frequency width of internal tide peaks from Altimetry spectra, we specified the same 2cm**2 variance fore modes 1 and 1cm**2 for modes 2, which is more or less the maximal values in the zones of highly coherent and strong internal tides. So this is certainly why in some region of low coherent internal tides, we tend to slightly overestimate internal tides, even though the consideration of mesoscales helps to mitigate the overestimation. This can be observed for instance on the Figure 4 of this document, in the Equatorial Pacific (where internal tides are known to be incoherent).

Then the question of the spatial Hamming window is also a very interesting one, and the impact of its size is crucial, which is certainly not discussed enough in the first version of the manuscript. The shorter this size is, the more possibilities (or degrees of freedom) there are, which is obviously more prone to leakage; Inversely, if we increase the length, we only restrain the solution to large-spatially-coherent plane waves, excluding the others. Setting this length is therefore a complex balance, and we ended up choosing 2.5 times the spatial wavelength of the wave (as it can be visualized on Figure 3 of the manuscript, upper panels), which turned out to give good results w.r.t. Independent data. This factor is set the same everywhere and for all modes (1 and 2).
We propose to update the manuscript with such details.

Section 3.1.1 summarizes the choices made in Ubelmann et al (2021) in a few sentences. This is fine not to present in details what is in this paper, but one is left a little bit wondering about what is been done. I was in particular wondering whether the choice to fit the covariance on the

altimetry mapping covariance, which filters out some of the smaller oceanic spatial scales has an impact on the internal tide solution.

Good point, and this is why we tried to resolve (as much as we could) the mesoscales at the wavelength on the internal tides, say 200-120 km for the first mode and 100-60km for the second mode. To to so, we defined the smallest wavelength of mesoscales at 80km. This does not mean we do resolve mesoscales at such wavelength all the time, but that we consider them and partially represent them (near observations). This was indeed not very clear in the manuscript, as we commented that the covariance representor was similar to the one in Aviso mapping. We propose to modify it with the above explanation, and mention that our actual covariances do allow shorter scales.

Also, when mentioning the full altimetric record, it should be indicated what is the data set. I assume that the adjustments between the different altimetric missions (and other corrections and filtering of the data along track, but that I am less sure) are performed before hand. Have these steps (if done) some implication on the internal tide characteristics that will be afterwards retrieved.
Yes, this was definitely missing, as written above in the response to another reviewer comment, we have added a paragraph dedicated to the input dataset.

My other comments are minor and could easily be fixed:
Thanks, we do appreciate the following grammatical corrections that we fixed among others, we hope the new version will be improved.
107: For each component k…
Done
115: index p should be explained.
Yes, p was actually N (corrected) and N, the number of components, is now defined. Thanks!
142 'only mode-1 is considered…'
Yes
155: '(sources and sinks)'
Yes
157: 'not too large'
Yes
162: I assume '(upper panel) …'
Yes
182, I don't understand the end of the sentence?
We propose the following:
The matrix is therefore more dense (sparse in space only), as illustrated on the left panel of figure \ref{Gmatrix} : \added{the element contains more data (covering the 25-years of altimetry).}
203: 'for each component'
Yes

207: I am not sure I got the end of the sentence: why ‘supposedly’?
We propose to remove ‘supposedly’, indeed, this was because initially we prescribed the possibility of K1 waves even above the critical latitude 30°. In the version of the paper, the K1 waves are prescribed only in the -30°+30° band.

210: ‘the stationary persists’ (word missing?)
Yes : stationary assumption

249: why is the Cryosat-2 mission specifically mentioned at this point (and not earlier)

This was a mistake! Initially we used Cryosat, but as now described in the paragraph above, we use all Altimetry data for validation, in the Spetember 2017 -December 2020 period. Thanks for noting!

257: ‘could be an interesting next step…’
Yes, corrected.