CC1: 'Comment on os-2021-80', Zhongxiang Zhao, 19 Oct 2021

This is a nice work that will draw interest from the altimetry community. It provides another new approach to map internal tides from satellite altimetry. It is a very cool method. It should be accepted with some minor revisions. In particular, the writing has a large room to improve.

We thank Zhongxiang Zhao for providing community comments that are very useful in many aspects.

L1, 'Ocean Surface Height', capital letters!?
Thanks, we hope we fixed all problems regarding capitalization.

L3, 'reduced-order basis with conjugate gradient resolution ' is vague in abstract, please specify.
Yes, we propose to rephrase as follows: “The inversion is performed \deleted{through} \added{in reduced-order basis of topography and practically achieved} with conjugate gradient”

L6, should be Msub2 etc.
We apologize, we do not understand the suggested edit?

L8, 'benefits' better be 'performance'?
Yes, and we would propose this modified sentence :
From the solution, we use altimetry data after 2017 for an independent validation, to evaluate the performances of the simultaneous inversion and compare it with an existing model.

L10, 'Internal Tides', capital letters?
Corrected, thanks

L11, '150 km ... and below' is not accurate, better say 'below 200 km '
Good suggestion!

l12, 'at first order'? Please explain or just cross out.
Yes, so we would propose “affected by vertical stratification”

L13, 'seasonal' a convincing paper is Zhao JPO (2021)
Yes, the reference is added

L17-18, 'O...G...C...M' no need in uppercase
Ok

L18, 'accurately' is not accurate

Yes, we would propose ‘with realistic amplitude’

L21, ‘later’ or ‘latter’?
Yes, latter indeed

L47, EQ2, move R definition here from a later place
Yes, done

L90, here and later, why do you repeat it ‘100’ times? Any reason or criterion? Please give more details
100 is to get sufficient realization for estimating the dispersion of errors on Figure 2 (especially right panel). So there is no rational criterion, 10 would have been too small to get smooth spectra on figure 2, 1000 would have worked, with would have given too much points to plot on the left panel. So 100 was a good compromise for this experiment that is purely qualitative. We propose some details in the new manuscript.

L97, ‘precisely’?
Yes, we propose to delete this word.

L143, Did you test with mode-2 S2 and O1?
Yes, we did test, but the variance reduction tests against independent data suggested that the solution obtained for mode 2 of S2 and O1 contained more errors than signal, so we decided to consider only mode 1 for these secondary tidal components..

L151, ‘3.5 m/s in the tropics’? I think higher speeds are at high latitudes, mainly due to the effect of f in EQ12.
Yes, we mention here the first baroclinic phase speed in the Chelton et al. climatology. We agree that the induced wave speeds are higher at higher latitudes because of Eq. 2.

L166, ‘c2=c/2’ is wrong! Please read Figures 3 and 4 in Zhao 2018 (JGR)
Thanks for pointing this, c2 is indeed a more complex computation based on the density profile. This would actually bring some room for improvements of a future version of internal tide solutions. We propose to add a sentence explaining this is a crude approximation, that may be revisited in a next version.

Figure 6, please give unit
Yes, and we also forgot the variable name. All added now.

Figure 8, based on the bottom panel, may we draw that MIOST-MSIT might underestimate internal tides, compared to MISOT-IT?

Yes, we would rather say that MISOT-IT overestimates internal tides, because there is more contamination of the mesoscales in the internal tide estimation. Indeed, the green bars suggest than theMIOST-MSIT is better in term of variance reduction, even though the signal is weaker. So the MSIT solution remains better. Thanks, this was a good point, we propose a clarification in the text.

Figure 9, for the bottom statistics, are you using the whole region above?

Yes we do, it is true that the region extends beyond the Gulf-Stream, but as for the scores, they are clearly explained by the Gulf-Stream area where the signal is energetic. The contribution of the northern region is very small since there are almost no internal tides resolved. Since the goal of the figure is to make a qualitative point regarding the signal variance and the reduction of variance after applying the signal, we think this is fine to keep this (a bit large) domain.

Table 1 is tedious: most of these numbers are very small. Please consider compress Table 1. These regions are defined in Carrere et al. (2021)? Many values are lower than 0.1%, may we say they are insignificant or within errorbar? 'Cryosat data' are from 2017-2018?

L262, I am not sure that 'opens the door for solving uncoherent internal tides'. There are TWO existing methods (Zaron; Zhao).
We propose the following paragraph to explain how it could be extendented toward incoherent internal tides, of course with some caveats:
The methodology applied in this study \added{could also be extended to internal tides with phases varying seasonally, as already implemented in \cite{Zhao21}. Practically, we would introduce additional components, that could be built with the same plane wave basis, modulated with sines and cosines at 1-year frequency. Also, with the perspective of future wide-swath altimetry providing a larger volume of data, the uncoherent internal tides could also be tackled with time-finite modulation of the plane waves. This is obviously a challenge that will not be met everywhere, in particular where mesoscale fields dominate, but we hope that in some regions, some part of the uncoherent waves could be captured}.