

Response to Christopher Unsworth :

Dear colleague,

Thank you for your interest in this manuscript and for the comments and suggestions you make.

I reply to your comments and suggestions to change hereafter in blue.

Best regards,

Loren Carrere

Description: Carrere et al., present a timely and desired analysis of a range of methods to correct satellite sea level altimetry data for the effects of internal tides. The effects of internal tides on satellite altimetry data are well known to be aliased due to satellite return periods being greater than the dominant periods of internal tides. Carrere et al., select 7 regions of the Earth to do their analysis – notable for their lack of seasonal stratification and the presence of internal tides.

Comparisons are made from the perspective of variance reduction - as no pure “truth” measurement is possible - the only way to know if better results are made is by comparison. Comparisons are made for “free” models, and models with data assimilation. However, it should be noted that some of the data used in the assimilation is also the data used for the comparison – so it is perhaps unsurprising that these show the least variance. [LC: That’s why I use 2 different altimeter missions for the validation procedure, and the C2 mission is independent from all models tested.](#)

Main Comments: Two other reviewers have picked up typos and inconsistencies, but both say the manuscript is clear. The majority of our reviewers found the manuscript very confusing. Our comments are summarised below.

1. There is a considerable amount of assumed knowledge in the manuscript. It took us a long time to work out that you were trying to use the models to correct the altimetry data. The abstract is especially hard to understand – this needs to be as clear as possible! Try getting a colleague that doesn’t know anything about the subject to read it and see if they can work out the purpose, key results and implications of the paper. [LC: abstract has been modified to make it clearer.](#)
2. Although the tables and figures (once font sizes have been made large enough to read) communicate the results well – the method of getting the data in the tables is highly unclear. No equations are presented and no description of how comparisons are made across models is given – as the different models may have different resolutions, time stepping and may have been run for different lengths of time it is hard to know if you have made “like for like” comparisons. A separate table, or additional columns in table 1, would be very useful to understand exactly what you have done. [LC: font size of figures has been made larger. The equation of the computation of the STD has been added in the text in section 3.2, and an equation has been also added in section 4.2. I added the information about the resolution of the models used in table 1, then all models are described with more details in section 2. Notice that only HYCOM model is a time-stepping model for which a harmonic analysis has been performed to get the tidal atlas.](#)
3. There is a considerable amount of vague and non-scientific terminology used when describing the results, in the discussion and in the conclusions. Phrases and words such as “performs well”, “some”,

“weaker”, “some problems” are very unscientific ways of describe your results – especially considering the manuscript focuses on validation and numerical comparisons of models. If you have the numbers to quantify these statements you use them at all times. The use of the word “significant” is very misleading. No statistical test is described to give that word a scientific meaning. If you have performed a significance tests on your data you need to detail that because it would help you communicate your results more clearly and forcefully, if not – you need a different word, or a quantification (e.g. a fraction or percentage). LC: I’ve replaced some of the non-scientific terminology you’ve pointed out above and removed the word “significant” when not appropriated.

4. Your objectives seem to be incorrect or are misleading. Objective one is stated as: “The objective of this paper is to present a detailed comparison and a validation assessment of these internal tide models using satellite altimetry”. As far as we can tell, this is untrue. No validation of the models themselves is provided. You are comparing the internal tide models, but you cannot validate the models per se- as no “truth” measurement is possible. We found this objective confusing – as we tried to find where you had validated your models. Objective 2: “The analysis focuses on the correction of the satellites’ measurements from the coherent internal tide signal for the main tidal constituents, M2, S2, K1 and O1”. This objective seems more correct based on the results presented. LC: the paper provides a detailed comparison and also a validation assessment procedure of the internal tide models. We don’t use any in situ measurements, but altimeter measurements are “truth” measurement of the ocean variability and using this large dataset is a valuable validation database. The methodology consists in the correction of satellites data using the IT models and then computing the variance differences and some spectral estimations as described in section 4.2. The description of the objectives of the paper has been a bit reformulated in the introduction.

We agree with reviewer #2 that the font in the figures needs to be larger, at least 2x as large. We also suggest using a different colour scheme - one that colour-blind people can use. Colourmaps that do this are common on the internet, some examples are given below:

<https://colorbrewer2.org/#type=sequential&scheme=BuGn&n=3>

<https://www.scientificamerican.com/article/end-of-the-rainbow-new-map-scale-is-more-readable-by-people-who-are-color-blind/> LC: the font in the figures has been made larger + the colormap of the figures 1-6 + A1-4 + S1-5 has been changed.

The manuscript does present highly relevant work that the community would like to see, however its communication is poor. As it does not appear that re-working of the models or analysis is needed, we think the study needs minor revisions; but the authors need to take care how they communicate their work.

Minor Comments:

Abstract: 1) One sentence does not make a paragraph (this error is present throughout the manuscript) LC: OK noted

2) “In order to access the targeted ocean signal...” what is the targeted ocean signal? This phrase is not used anywhere else in the manuscript and it is not explained. Start with satellite altimetry- and why it has a source of error due to internal tides, and why correcting it is important (in ~2-3 sentences). LC : targeted signal is ocean circulation and mesoscale variability . Sentence has been changed.

3) “several geophysical parameters” – the list of geophysical parameters that exist on Earth is enough to fill a book – what do you mean exactly? LC: I’m talking of the geophysical parameters used to

correct altimeter measurements only, they are listed in section 4.1 which describes the altimeter database.

4) I would also include that this work led to the Zaron model being implemented in the GDR standard (if this is true) [LC: yes this is true.](#)

Methods: You need to describe your models more consistently. At present the level of detail given to these models is sporadic. What years are they all run for? At what resolutions? What time-stepping? This kind of information is important as it is relevant for how you compare the results. Description how you compare the data is also needed, equations may well help you communicate this clearly. Are any statistical tests used? How many degrees of freedom are allowed when you calculate the variance? [LC: I added the information about the resolution of the models used in table 1, then all models are described with more details in section 2. Notice that only HYCOM model is a time-stepping model for which a harmonic analysis has been performed to get the tidal atlas. Some equations have been added in sections 3.2 and 4.2. We compute the variance on each box of the map and the number of points is different within each box, but we assume a gaussian distribution of the samples in each box.](#)

Discussion: The paragraph starting line 597: “Following the results presented here, a recommendation.” This is very interesting, but it unclearly worded. You say the results presented here (as in now) led to the Zaron model being implemented in the GDR standard, which was decided a couple of years ago. This is very significant and important. You could reframe the manuscript as the scientific justification for that decision (which has obviously been presented at conferences prior to submission to the journal – as is usually done). I would include this decision, and the implications of the increase in capability, in the introduction as well. [LC: sentence has been reformulated in the Discussion section, and some words have been added in the abstract.](#)

Break these long sentences up. E.g. “The present study indicates that the use of the altimetry database is a valuable tool to validate models of IT surface signature on the global ocean and particularly it complements efficiently the in situ validation processes which are generally more localized in space/time due to the availability of in situ datasets (Dushaw et al. 2017, 1995; Zaron and Ray 2017).”

Turns into: “The present study indicates that the use of the altimetry database is a valuable tool to validate models of IT surface signature on the global ocean. It particularly complements the in-situ validation processes which are generally more localized in space/time due to the availability of in situ datasets (Dushaw et al. 2017, 1995; Zaron and Ray 2017).” [LC: done](#)

A general rule to help is: once sentence has one point, or message. If you have two messages, you need two sentences. This shorter and more direct structure especially helps for non-native-English readers. [LC: OK](#)

“In addition, many initiatives are now being conducted to try to better understand and model the non-stationary component of the internal tides. Work is progressing on the modelling of the seasonal and interannual internal tides variability: Zhao (2019), Zaron (2019), Ray (personal communication), Ubelmann (personal communication). Within the SWOT Science Team and other projects, several teams also work on 3D simulations using different general circulation models such as HYCOM, MITgcm, NEMO (CMEMS-Mercator-Ocean project in progress), or even a specific spectral approach (S. Barbot et al., in preparation).”

We can understand why you want to say this, but none of this is produced in the manuscript you present, so it is not a conclusion you are able to make. You could discuss these ongoing efforts in the light of your new findings, and the implications of the present work has on these efforts. But it is not a conclusion. LC: I keep the discussion about the ongoing efforts listed above as I think it is important to list them in this paper, but I've changed the sentences for clarification.