Authors answers to RC2 comments/questions/recommandations

(for commodity reasons, we have reproduced the reviewer text in black, answers are in light blue, further action to the revised manuscript in bold blue)

This paper provides a thorough description and evaluation of the FES2014 tide model, in several variations. The rationale and design of the modeling and assimilation strategy for developing FES versions is explained very clearly, and the detailed description of their ensemble technique will be a welcome addiiton to the literature. This information will help ocean modelers understand the potential strengths and weaknesses of a model like this, and it will also guide others hoping to build on their work in the future.

The detailed evaluations of the model and comparisons of its various versions and other tide models will be useful to potential end-users of the tidal atlas. I believe this article will be of interest to readers of Ocean Science Discussions, and I recommend that it be published after some minor revisions, detailed below.
There are two main areas where the article should be revised to make it easier for a reader to understand and use the provided information:

1) In some places the article lacks clarity regarding the rationale for the comparisons using FES2014a, FES2014b, and FES2014c. Examples are noted in the detailed listing below. The manuscript would be a little easier to read if it clearly stated ahead of time which versions of the atlas are being used and why.
As stated in the answers to review's comments, we will follow your recommendations in the revised paper.

2) The article does not discuss at all the tidal prediction software which is provided with FES2014. I don't think that the article needs to discuss or provide this in detail, but it needs to be clearer about the nature of the tidal predictions used for computing variance reduction statistics. Do these predictions include all the tidal frequencies mentioned in the manuscript, including the ones which are forced with the ERA-interim in the time-stepped model? Do the predictions include smaller tides computed using inference? If so, do the inference formulas rely exclusively on those tides which were computed with assimilation, or do they use some of the other frequencies? If the comparisons of different models use different constituents, then the manuscript should state this clearly in the tables or figures where the comparisons are shown. Another aspect of the tide predictions needs to be discussed since the atlas is intended for correcting altimetry: How should FES2014 be used when a separate model is used to provide a DAC?
Yes, the tidal predictions include all tidal frequencies mentioned in the manuscript (either from the FES2014 tidal atlas, or from inference for some smaller tides). The tidal prediction software is available on a bitbucket deposit : https://bitbucket.org/cnes_aviso/fes/src/master/. The inference formulae and the list of the tidal constituents that can be computed by inference are listed in the code.
When comparing FES2014 performances to other models in terms of variance reduction, the FES2014 prediction uses all FES2014 waves available within the FES2014 tidal atlas, and thus it includes more tidal frequencies than other models (GOT4.10 and TPXO respectively). This choice was done in order to take into account both the modeling and the omission error of the models. Concerning GOT4.10 and TPXO predictions all tidal waves available in the respective atlases are taken into account and the same inference algorithms are used.
Yes FES2014 tidal correction can be used even if a new model is used for the DAC correction, at the condition that the new DAC solution does not include the S1 and S2 frequencies which are already contained in the tidal model. When a separate model is used to provide a DAC, S1 should not be used in the prediction. See also our reply to reviewer 1's comment #5.

Detailed comments:

Good introduction which lays out the features of the tidal data assimilation: limitations of L2-norm, hence importance of the prior model and outlier rejection, and relative data sparsity depends on dynamics/wavelength.
Good explanation of FES2014a, b, and c releases.

p5,l7-9: "Initially, ... integrated in ..." I don't understand this sentence.
The T-UGOm model has been developed initially has a classical time-stepping code, and the frequency-domain solver (inspired from the CEFMO frequency-domain code used up to FES2004 atlas) is a later addition. As in

many other models, the Flather's method (also called Riemann's invariants method) is often used in regional or coastal configurations to mitigate the prescribed OBCs through a relationship mixing elevation and velocities. However, velocities extracted from a global atlas may not be fully consistent with the regional configuration to run, mostly because of mesh resolution, bathymetry and friction differences, while tidal elevation are less sensitive to those. A frequency-domain simulation, forced at its open boundaries with tidal elevation only, will produce a regional solution with properly "downscaled" velocities at its open limits, that can be re-used for further time-stepping simulations.

p5,l33: Does the discrete system satisfy any conservation laws?
Yes, but in the finite element variational sense (i.e. integral convolution with discretization interpolation functions over the domain). It is a bit tricky compared to usual finite volume (or C-grid) local conservation, but mathematically rigorous.

p5,l35: Thus, there is no lateral eddy viscosity in the model?
There is. We mostly use Smagorinski-tuned Laplacian type of eddy viscosity, but it usually act as a numerical scheme stabilizer, far from what would require a truly physical term. We make use of wave equation formulation to solve for the shallow-water equations, which demands a "diagonalization" of the momentum equations left-hand side. In time-stepping mode, non-diagonal terms (such as viscosity Laplacian, needed for stability reasons) are moved to the right-hand side (at central time in the leapfrog integration for example). It is not directly feasible with the frequency-domain solver, which actually imposes some additional (as for the non-linear terms). As the frequency-domain solver has no stability issue, and as eddy viscosity has a rather small contribution to tidal dynamics, we usually do not perform this step.

p7,l8-9: "even at regional extents, as earlier ... compensation bias." I don't understand this part of the sentence.
The model calibration step (based on semi-empirical trial simulations) aims to set "optimal parameters" (bottom roughness, IWD, etc…) to minimize tidal solution misfits with observations. This calibration helps to define more realistic settings, but also will include some error compensation (such as IWD value not only linked with ocean stratification characteristics, but partly compensating for some local bathymetry error at internal tides generation sites). So it can happen that improving the bathymetry in a limited region of the model will increase models errors, as parameters set for earlier bathymetry error compensation are then irrelevant, and calibration step must be carried out again. Same for change in coastal resolution, LSA atlas, etc… In consequence, we need to keep our model reasonably "light" to allow for a large number of tidal experiments.

p9,l8-9: Are you making a distinction between the tidal loading (the deformation of the earth surface caused by the tidal changes in ocean bottom pressure) and the solid earth tide here (the deformation of the earth surface caused by the gravitational perturbations of the sun and moon)?
Definitely yes, the latter being of course needed for altimetry data corrections, but easily computed from analytical formulas. We concentrate on deformation due to ocean mass re-distribution by the tides.

p9,l30: Thanks for describing this in detail.
The significant, non-tidal ocean sea surface variability around semi-annual and annual frequency will contaminate K1 harmonic analysis because its aliased frequency in TP/Janson (~6 months) and Envisat (~1 year) observations. We use a Glorys-derived SSH harmonic analysis at semi-annual and annual frequency to hopefully remove a part of the non-tidal contamination. The efficiency of the non-tidal ocean signal contamination has been assessed at TP/Jason cross-overs, where the K1 harmonic constants misfits between ascending track and descending track analysis are diminished.

p9,l36: I would have expected that all the harmonic analyses were based on the longest records available, so this section on S2 can probably be deleted.
We might consider following your recommendations in the revised paper.

p10,l19: Please explain this procedure more precisely. Is this equivalent to computing the standard error matrix for the least squares solution, and then recomputing estimates for fewer frequencies when the error correlation was too large? Exactly what criterion was used?
The principle of our separation diagnostic method is more direct. Ideally, i.e. in case of quasi-infinite time series, the harmonic matrix will be quasi-diagonal. The shorter the time series, the larger the cross-

terms/diagonal-terms ratio in the matrix, which reflects the loss in separation efficiency. In the case of a regularly sampled, continuous time series (no data missing), the usual Rayleigh criterion (at least 1 period difference between two different constituents over the time series duration) is equivalent to a maximum ratio of ~0.15 in any row of the harmonic matrix. In the case where 2 constituents show a ratio larger than 0.15, we check whether admittance can be used to infer the one with the lowest astronomical potential or not. If not the case or if at least one is a non-astronomical constituent, we drop it.

p10,l33: This is a little confusing. Are you saying that there are a total of N_{tot}=432 ensemble members, and these are computed from sets of independently perturbed ensemble components? Do you have, N_{tot}=N_{bottom drag} * N_{internal tide} * N_{bathymetry} * N_{LSA} where N_{LSA} = 2, corresponding to the FES99 and FES2012 load tides?
(I see, yes. Made this table while reading later:)
N_{bottom drag} = 8 * 13 = 104
N_{internal tide} = 10 * 7 = 70
N_{bathymetry} = 2 * 18 + 6 = 42
The total number of ensemble members is composed as follows:
N_{tot} = (N_{bottom drag} + N_{internal tide} + N_{bathymetry})* N_{LSA}
With
N_{bottom drag} = 8 * 13 = 104
N_{internal tide} = 10 * 7 = 70
N_{bathymetry} = 2 * 18 + 6 = 42
N_{LSA} = 2 (FES99 and FES2012 load tides)
We will modified the text to clarify this section.

p13,l6: "SpEnOI code is solving the assimilation in the data space" – Are you aware that you could reduce the dimensionality to 432 by solving in the space spanned by the ensemble members, without reducing the quantity of data at all?
We have not investigated this point much, despite it perhaps rings a bell about solution space reduction (G. Egbert et al. publication). As representers are to some extent covariance compute from members, hence not a linear operation, it seems not trivial to me. It would be interesting to discuss this matter with the reviewer#2. Actually, we kept the data representers approach as an heritance from the previous variational data assimilation code (CADOR, used in FES2004), because it was a quick solution (no much change in the software), and second because we wanted to be able to get back to the variational formulation if needed. The main obstacle is not that much the solution of the linear system itself, but the large volume of members vectors to be processed.

p13,l7-22: Can you reduce this text and just state what data you used for assimilation versus validation? I don't understand what data were used.
We will follow your recommendations in the revised paper

p13,l30: Did you use an estimate of the data error to weight the datasets that were assimilated, or did you simply assimilate fewer data in the noisier regions?
Yes, an estimate of the data error was used for the selection of all the assimilated observations. For the altimetry data, this data error is based on the least-squares error estimate (from the harmonic analysis). Error thresholds used to select the altimeter data are gathered in table 4 of the manuscript.
For the tide gauge observations, the error estimates were fixed empirically to 3 mm for the deep ocean stations and to 1 cm for the shelf and coastal stations. The idea was to limit the constraint on the model at the tide gauge stations on the shelf and close to the coast in order to avoid drawing the solution to fit some very local tide features observed by the coastal stations that may be inconsistent with the larger scale tidal patterns that can be accurately solved at the resolution of the model.

p14,l23: Could you please make clear whether "FES2014" refers exclusively to FES2014c in this section, or if you are describing aspects of the FES2014a and b solutions, too.
Clarifications will be made in the revised manuscript.

p14,l29: Earlier you stated that S1 belongs to the DAC solution. Please clarify.

Some users prefer to deal with a mean S1 tide correction rather than performing a DAC correction. So we provide a S1 harmonic atlas to leave the choice to the community to deal with S1 either as a tidal correction or as a DAC correction. I will clarify this point in the revised paper

p14,l40: I am confused about the use of ERA-INTERIM atmoshperic forcing. Wouldn't you want to compute purely gravitationally-forced versions of these, since the atmospherically-forced component would be provided by a separate DAC model? Since the goal of the paper mentions developing the tidal atlas specifically for dealiasing altimetry data, you should be clearer about how this should be done in practice using FES2014c, since there is the potential to unintentionally duplicate corrections from the tidal atlas and the DAC correction as it is usually applied.
As mentioned earlier, we would prefer to leave the atmospherically-forced component in the DAC. Unfortunately, when performing data assimilation, astronomical and atmospheric contributions are definitely mixed in assimilation solution because it is mixed in the data. To avoid duplicated corrections, we consistently filter in the DAC corrections the contributions of (data assimilated) tidal constituents having a significant atmospherically-forced component (S1 and S2).
One could imagine removing the atmospherically-forced component from data using the DAC, but first it is feasible only for data of which original time records are available, and second it would add DAC modeling errors to observations. Finally, purely astronomical tides atlases would be pretty tricky to use by non-altimetric data users.

p15,l30: Could you please remind us about why you are validating FES2014b instead of FES2014c here?
FES2014c is based on FES2014b to which a number of long period tides, computed from mass-conservative equilibrium method, have been added (those long period tides, if not externally provided, are already taken into account in a similar way by the tidal prediction algorithm for the FES2014b prediction). Those long period tides are not considered in this validation section presented here and therefore FES2014b and FES2014c can be equivalently used.
We agree it can be somehow confusing, and we should clarify this in the revised manuscript.

p15,l36: Does "TPXO9" refer to the "TPXO9v2" mentioned on page 14? And, does "TPXO9-atlas" also refer to "TPXO9v2"?
Yes, "TPXO9" refers to "TPXO9v2". We have homogenized the names in the revised version of the paper. Following the naming convention used by OSU, "TPXO9-atlas" is the 1/30-degree solution of TPXO9v2 that we use for the comparison.
We will rephrase the sentence in p15,l37 to make it clearer.

p16: I am unclear on why FES2014a and FES2014b and FES2012 are used in some of these comparisons. I understand if this is simply related to the reporting of comparisons originally conducted for various purposes, but perhaps you could mention the reasons at the beginning of this section.
We agree it can be somehow confusing, and we should clarify this in the revised manuscript.

p17,l31: Can you cite a source for this statement? This certainly depends on depth of the station, stratification, etc.
I will add a source, however it is a common knowledge for people observing or modelling 3D baroclinic tides. Of course IT currents have a complex 3D structure, highly variable in the deep ocean, still they commonly reach much higher values than the barotropic one in the abyssal plain.

p18,l28: "dynamical quasi-coherence of the covariances" – I don't know what this means. Please explain or omit.
As representers (model error covariance) are based on dynamical ensemble statistics, they partially have some true dynamical properties, i.e. the assimilation solution is closer to a possible, particular dynamical solution than if using Gaussian functions as in optimal interpolation instead of representers.
It will be rephrased this in the revised manuscript.

p18,l30: Can you say anything about the results of Fig 21? If they don't show anything new, then perhaps this section can be omitted. Alternately, maybe they basically confirm but slightly alter previous estimates. Or maybe there is something that can be gleaned from comparisons of recent calculations by DeLavergne et al.

Since you previously stated that the currents are not reliable in regions of steep topographic slope(line 13), why would the energy flux divergence or the parameterized baroclinic wave drag be reliable?

Figure 21 is complementary to other works as mentioned in the comment and aims to advertise that barotropic tides energy budgets are available for the FES2014b constituents, including energy fluxes, bottom friction rate of work, etc... We believe it is an interesting point as first, energy budget examination is a very valuable way to understand tidal dynamics and validate tidal solutions, and second, internal tide generation budget remains a very open subject.

About the reliability of currents, the issue mentioned in our paper occurs only in a few places where local resolution was accidentally not properly adjusted to topography slopes constraint (which requests resolution to be proportional to H/grad(H)), and do not minder the overall accuracy of the energy budget estimates.

p19,l26: "After proper, competitive evaluation procedures" – Without explanation, the reader is not going to know what you are referring to. Could you either omit this phrase, provide a citation, or explain this topic further?

English usage/typos:

Language and typos have been corrected in preparation of the revised manuscript.

p5,l5: omit "about a"
p5,l42: "it is known to allow for" –> "that it allows"
p6,l7: "multi-levels" –> "multi-level"
p6,l10: I am more accustomed to seeing "CFL" rather than "CLF" for this abbreviation.
p6,l16: omit "and"
p6,l27: "Go" –> "GB" ?
p7,l18: "SAL" –> "LSA"; and fix "atlas atlases"
p7,l42: Why repeating S1 in the parens?
p7,l38: Can you reorganize this paragraph for clarity? Something like:
"The hydrodynamic solution for the S2 tide differs from the other tides in that the atmospheric forcing is explicitly included. ... The S1 tide originates mostly from the atmospheric forcing, and it was therefore not computed in the hydrodynamic tidal solution. ..."
p8,l16: omit comma at the end
p9,l14: please fix "Then,18); this"
p9,l20: I thought Parcel's theorem was the equality of the sum of squared Fourier components with the variance. Please reword or explain . p9,23: "guaranty" –> "guarantee"
p10,l11-12: omit "to ease the harmonic system solving"
p11,l8: ", 1982" –> "(1982)"
p11,l14: "abuse" –> "abuse of nomenclature"?
p11,l18: "is run" –> "are run"
p11,l21: "tide drag" –> "internal tide drag"?
p12,l6: "sloppy" –> "sloped"
p12,l9: "efficiency is strongly dependent on" –> "depends on"
p12,l13: "(75)" ?
p12,l19: commas after "GEBCO" and "release"
p14,l6: "repartition" –> "distribution"
p14,l16: "it small" –> "its small"
p14,l17: "ration" –> "ratio"
p16,l9: "if" –> "while" or omit "if"
p16,l14: "none data" –> "none of these data" or "no data"
p16,l11: Presumably, you used all the tidal harmonics available from each model in these comparisons, since you mentioned errors of omission previously. You should remind the reader of this here, or later in the Discussion.
p16,l20: Can you redo this comparison of FES2014a with FES2014b, instead?
p17,l40: repetitive of line 34; please consider revising this entire section for brevity.
p18,l15: omit "Somehow,"
p18,l37: capitalize "Love"?
p18,l39: omit "then derivation"?
p19,l23: "others" –> "other"

p19,l35: "coastal details grid flexibility" –> "detailed coastal grid"
p19,l38: omit "generation"
p20,l5: "atlases" –> "atlas"; omit "locally strongly" on l6
p20,l9: "eased" –> "open"
p20,l11: "as well in terms of" –> "for both"

Figures and Tables:

Most of figures are being re-processed to comply with reviewers comments and improve their graphical quality.

p24-25,F2 and F3: The caption mentions panels a, b, and c, but only two panels are shown. Pleas label the panels a, b, and c, and include the Darwin name boldly somewhere on each plot.
This will be fixed in the revised manuscript

F4: capitalize "Maps" in caption. Other captions also need this correction.
This will be fixed in the revised manuscript

F8: The figure shows that the bathymetry of Weddell sea was perturbed, but I thought the text mentions that, after initial experimentation, it was not.
Actually, you're right, and the reason is that ice-shelf seas needs a specific treatment to infer free water column heights from bottom topography and immersed ice thickness, and the number of available databases to perform this operation was found to be too limited to allow for a proper perturbation procedure.

F12: Please clarify how "deep", "shelf", and "coastal" are defined. Is it a depth criterion, or simply whatever the data source happened to label it as.
This will be fixed in the revised manuscript

F13: I cannot distinguish the dark colors at the ends of the colorscale. Maybe represent this data with different size disks, like your previous comparisons in Fig 2? It is difficult to understand the information on this map.
This will be fixed in the revised manuscript

F14: Consider coloring the land and sea differently (white vs light gray). Does blue mean that FES reduces more variance?
Yes, and we will clarify it the revised manuscript.

F15: Is this comparing tide predictions made with the same constituents from each model? If not, you should explain to the reader that FES is providing more constituents, and/or cite Stammer et al, or Zaron and Elipot (2020, https://doi.org/10.1175/JPO-D-20-0089.1) where the comparisons are restricted to common constituents.
No, as mentioned above, the number of constituents differs for each model as the tidal prediction is based on each model tidal atlas, but same tidal prediction software is used. Yes FES2014 atlas includes more waves particularly for non linear tides, which is of great interest in the shallow water regions and makes this model better suited to correct altimeter data in those regions. Notice that the comparison/validation of each individual tidal component is described in the "validation in the frequency domain" section of the manuscript.
This will be clarified in the revised manuscript

F17: Once again, it is important to be sure the reader is aware whether the same constituents are being used, especially since FES2014 is using a number of frequencies with substantial atmospheric forcing. Also – GOT is not really intended for use close to the coastline. Did you extrapolate the values landward, or does this comparison only use locations where GOT grid cells happen to overlap the coastline?
The models used are extrapolated on the coasts in order not to miss any altimeter measurement near the coastlines; this processing is the one performed operationally to provide the tidal model solutions to be used in the altimeter GDRs.
This will be clarified in the revised manuscript

F18: I think this figure can be omitted.
We agree it finally does not add much value to the paper. **This will be fixed this in the revised manuscript**

F20: Can you use a different scaling to make the current ellipses more visible? Consider omitting this figure if you can't revise it. Or, maybe show a few representative comparisons enlarged, instead.
This will be fixed in the revised manuscript

F23: The caption is hard to understand. Could you describe this with something like:
"Map of the SSH variance difference a crossovers using the FES2014a tidal loading versus the GOT4v8ac tidal loading. ..."
This will be fixed in the revised manuscript

p39,T1: fragment at end of caption, "3 Tidal harmonic ..."
This will be fixed in the revised manuscript
p39,T2: Please fix "#DIV/0!". Also the different vertical alignments and number significant figures displayed for values in the table is disconcerting. Also, unlike the other numeric values in the manuscript (cf., Table 3), the "," is used instead of the "." to indicate the decimal point; please revise for consistency.
This will be fixed in the revised manuscript

p40,T4: What criteria are used to distinguish "shelves", "Open ocean", and "Arctic Ocean"? Is the Max error criterion applied to the least-squares error estimate (from the harmonic analysis), or do you estimate the error in some other manner?
The max error is based on the least-squares error estimate. Open and shelf ocean are distinguished following a depth=500m criterion. Arctic Ocean is delimited using a polygon.