Comment on egusphere-2022-1085
Anonymous Referee #2

Referee comment on "Validating the spatial variability of the semidiurnal internal tide in a realistic global ocean simulation with Argo and mooring data" by Gaspard Geoffroy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1085-RC2, 2022

General comments:

This is a very valuable and comprehensive study that compares the semidiurnal tidal variance and autocovariance in a global numerical model and in situ observations. This paper is well written and mostly logically organized. I would recommend publication after the main concern below is addressed as well as the smaller technical comments.

I have some serious concerns about the method employed to compare autocovariance function estimates. After calculating average autocovariance functions, the authors essentially estimate the autocovariance function amplitudes and their associated confidence intervals. Yet, the method employed for calculating these confidence intervals appear to be flawed because it assumes that the amplitude estimates are normally distributed, which is not the case. This is clearly illustrated as an example in Figure 2b that shows confidence intervals crossing the zero value: true amplitude values cannot be less than zero. I suggest for the authors to properly derive error estimates and confidence intervals for the autocovariance amplitude before pursuing the rest of this study. I provide some potential ways of doing so in my detailed comments below. In fact, in Figure 10, as an example, the authors take a better approach by displaying quantiles of the distributions: why not taking that approach from the beginning? I also suggest to replace the term "demodulate" by something more meaningful: perhaps autocovariance envelope or amplitude? As noted below, the method to obtain the "complex demodulate" could be improved by simply computing the analytic transform of the autocovariance functions.

For the validating part of the study, section 3, the current organization of the material does not make sense to me and the conclusions are not clearly laid out. First, I would like to know how the model does in an Eulerian framework, then I would like to know if the Lagrangian framework or method is valid, and third I would like to know the result of comparing Argo and Lagrangian HYCOM particles. As such, I suggest the following reorganization of the material of section 3:
- comparison of HYCOM Eulerian results and mooring (Eulerian) results to assess the
model: what is the conclusion?
- comparison of HYCOM Eulerian and Lagrangian results to assess the method of using Lagrangian data: what is the conclusion on the potential Lagrangian bias?
- Comparison of HYCOM Lagrangian results and Argo (Lagrangian) results: what is the conclusion?

Specific comments and technical corrections:

Abstract: line 1: In the abstract, unless you explain there what you mean by "decorrelate" as you do in the main text, I think that instead of "correlation" you should write "auto-correlation" or "auto-covariance" which are established statistical terms. An abstract should be able to stand alone.

It may already be in the title but perhaps you could rephrase the abstract to provide a summary statement of what you are doing: validating a model by comparing it to in situ observations.

l46: It is not obvious (to me) what the k-space methodology is. I suggest that you rephrase or explain. Does this refer to the method of Zaron (2017)?

l71-72: It is not good practice to refer to a section ahead. Simply explain that the duration was chosen to match the numerical output you are using/comparing?

Section 2.3: Perhaps a reference for HYCOM and that specific simulation is needed. Should you add more details about the use of the Parcels software that would allow readers to replicate your experiment?

l96: Why "mainly"? And can you simply state why you used only 32 days of the model? Data/space constrains?

l101: Why 41644? Does this correspond to a mean geographical density?

l106: The effects of the drift? Do you mean potential Lagrangian biases?

Section 3.1; eq. 1: Could we get here an explanation of this quantity and why it represents the vertical displacement of an isotherm? Perhaps cite Hennon et al. 2014 as
you did in Geoffroy and Nycander 2022? Are you correcting for the float displacement as you did in that paper?

l113-115: which monthly-mean 3D temperature field? Is it from a product for the case of Argo? Please provide more details; I do not understand how you get that gradient for the in situ data.

Figure 1: The Argo segments are shown as dots? How are these segments? Could you plot the assumed rectilinear trajectories of the Argo floats?

I 131: I don't get this: what is a "binned HYCOM particle"? Do you mean that you average the individual autocovariance estimates in Eulerian bins?

I136: Don't you think that in that figure the R_{argo} falls below its CI at ~100h rather than at ~200h?

Eq4 and after: I am not sure that this is the right way to compute the confidence interval for A: what you call the complex demodulate, or rather its square value (A^2), should be distributed like a chi-square variable with 2 degrees of freedom (like a spectral estimate), and not distributed like a Gaussian variable. Thus, confidence intervals as plus or minus two standard errors are likely incorrect. Consider your figure 2b: the CIs suggest that A can take negative values whereas it is clearly a positive quantity. I suggest you revise the derivation of the CI for A and reassess your overall results.

Figure 2b: the CIs for the two curves are superimposed and thus cannot be distinguished; please modify the figure so that the reader can see both.

I156: Considering my remark above that your CIs are likely incorrect, I think you should revisit that statement.

I believe that what you are trying to plot in Figure 2b is the envelope of the autocovariance function. Your method is probably fine but the envelope can be easily obtained by computing the amplitude of the analytic signal of the autocovariance, see Lilly and Gascard (2006) as an example (The analytic signal can be calculated using the Hilbert transform in python with the scipy package or the anatrans.m function of the jLab toolbox for Matlab). One way to get a confidence interval for the amplitude of the analytic transform would be to look at the distribution of all the individual transform amplitudes, lag value by lag value (as you do in Figure 10 later).
"outliers": please use sentence to explain what you mean.

Figure 5 does not look like a scatter plot but a 2D density plot. Is the $R^2$ exactly 1 as written in the plot or is it approximately 1 as stated in the text? I am surprised that it is so close to one. What is it in a domain that is not logarithmic? What is your $R^2$ anyway? The adjective "Pearson" is usually used for the correlation coefficient while the coefficient of determination is the correlation squared for linear regression.

taken as ...": state this earlier to remind the reader.

a bias which means that HYCOM underestimate Argo, correct?

Figure 7b: try the ratio $x/(x+y)$ instead of $\log_{10}(x/y)$ as in Arbic, Elipot et al. 2022. In this way you will not have to use $\log_{10}$ and truncate the scale. The results will look the same but it is a better statistic that is robust to outliers.

The ratio increases approaching the poles? Where is this seen?

Should you conclude the section with some statement?

Section 4.2:

I do not understand what you mean by that. Please explain what is the intrinsic decorrelation. Do you mean Eulerian? In fixed space?

"particle in the Eulerian framework": what do you mean? You average in Eulerian/geographical bins? I think you should use "Eulerian framework" for computing autocovariance from Eulerian time series (model grid and moorings) and "Lagrangian framework" for computing autocovariance from Lagrangian time series (model particles and Argo).

yes indeed because the autocovariance and its amplitude are probably not gaussian distributed!
I251: "the distribution is well centered on the $y = x$": I strongly suggest you revise this assessment. Figure 9a suggests no linear relation between the mooring results and the model results.

I253: "log domain": this figure appears to be on a linear scale?

I270: truely -> truly

Figure 11b: a legend for the various fitted curve would be very helpful.

I293: Why is it 3 times $T_{\text{int}}$?

I306: "Note that ...": this should be moved earlier just after your eq 6.

I 315-319: What are the implications of this comparison for HYCOM? Could you expand? I understand you address this next but a transition sentence at the end of a section would be useful.

Section 4.4:

I323: If your method holds, should you not rather say that the model is biased low?

Data availability: A statement on the HYCOM data availability is missing.