



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-1085-RC1>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on egusphere-2022-1085**

Anonymous Referee #1

---

Referee comment on "Validating the spatial variability in the semidiurnal internal tide in a realistic global ocean simulation with Argo and mooring data" by Gaspard Geoffroy et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-1085-RC1>, 2022

---

The authors have made an interesting analysis of semidiurnal tides in the HYCOM model using the time-lagged Eulerian and Lagrangian autocovariances of vertical isotherm displacement. They compared output from a 32-day-long HYCOM simulation with Argo park mode data and moored thermistor data, and found that the "total internal tide" variance in HYCOM is too small, especially in the far Southern Ocean. They also found that the Eulerian and Lagrangian estimates of the tidal variance at near-zero lag agree very well using HYCOM data, which bolsters this analysis and their previous analysis of Argo data. Finally, they used Caspar-Cohen's technique to estimate the "intrinsic" and "apparent" decorrelation times of the internal tide, and found that the observed tides decorrelate faster than the HYCOM tides. The authors make the interesting observation that the mean (stationary) IT in HYCOM is too large compared to altimetry, but the total IT (stationary + nonstationary) is too small compared to Argo and moorings.

They discussed some reasons for the discrepancies between HYCOM and the observations, but it was unclear if any of their suggestions could explain the quantitative differences. With regard to the too stationary tides in HYCOM, they did not mention the possible roles of missing small-scale mesoscale or submesoscale variability in HYCOM or the deficit of high-frequency wind forcing.

Overall, this is a nice piece of work which I think will be of interest to many readers of Ocean Science. I have many small comments, listed below. While I would say I have no major concerns, my comments could justify some new analyses or revisions of results presented, so I recommend Major Revision.

Comments:

l1: Is "total" needed? Why not omit or say "tidal"?  
Throughout the abstract, "total" is used, but it is not contrasted with "partial" or another "non-total" quantity to understand what distinction is implied by "total".

l11: "beams" -> "waves" or "beams of waves"

l17: Omit "at any given position", since later in the sentence you state that you are referring specifically to "their generation site".

l19: "causes" -> "cause"

l23-l27: I think I understand what the authors' are getting at, but I found the first three sentences confusing. When we look at the plot of an autocovariance, such as is suggested by the first phrase, we would see the envelope of autocovariance decay, and the coherent fraction of the signal will dominate the autocovariance at long lag. It seems like this paragraph may be muddling the ideas of what happens to the autocovariance as a function of increasing lag, versus what happens to the wave energy as a function of increasing propagation distance. I would suggest re-thinking the purpose of this paragraph and re-writing it to more-clearly articulate the point you wish to make.

l42: Once again, "total" is used without distinguishing it properly. It seems like it should be clearly defined above, when the ideas of the coherent and incoherent signals are defined.

l52: Finally, "the autocovariance at short time lags", is identified with the "total variance". Some sort of explanation needs to be provided earlier. But how is tidal variability distinguished from noise and high-frequency ocean variability when looking at the "short time lags"?

l52: "On the other hand" -- I am not clear what is the other part of the contrast. Omit this phrase?

l55: I am not sure "intrinsic" is the right word. An intrinsic quality ought to be one which is unaffected by extrinsic factors. But, the decorrelation is entirely caused by interactions with the propagation medium. Perhaps it is best to stick with the Eulerian vs Lagrangian distinction, and when the autocovariance is discussed, it seems like you need to be clear whether

you are discussing an Eulerian or Lagrangian autocorrelation.

l63: omit "the strength of"

l73: "can vary" by how much?

l81: Did you use exactly the same dataset as in Geoffroy and Nycander (2022)? I would be interested to know how many 32-day records there are, from how many individual drifters. Also, can you remind us exactly what the "data" consist of? Is it time series of isopycnal displacement, inferred from temperature measurements during the part phase, using temperature profiles from the start and end?

l111: Is  $dT/dP$  in the numerator the same as  $d\bar{T}/dP$ ?

l114: "obtained" -> "estimated"

l123: Sorry if I misunderstand what is meant by "unbiased" here, but isn't this a biased estimator when the expected value is taken for fixed  $N$ ?

l140: I do not understand why the sine component is included. The autocovariance is an even function, so any projection onto the sine must be noise, right? Likewise, I don't understand the total error defined in equation (4). And why would a robust estimator (median for  $\tilde{\text{SEM}}$ ) be combined with a non-robust estimator ( $\text{Var } A$ )?

l157: "not significantly different" -- Well, I agree that they do fall within each others' standard errors, but they look significantly different to me. What is the probability of the offset over so many different lags; how many d.o.f. do you think are in these estimates?

l170: Can you explain why you estimated the Eulerian autocovariance along the Lagrangian trajectory? Are you trying to account for the geographic variability of the Eulerian autocovariance?

Fig 3: This is related to the above question: Why are the Eulerian error

bars so small compared to the Lagrangian? Maybe you could spend a little more time explaining how this plot relates to Fig 2. Are the Lagrangian HYCOM curves in Fig 2 identical to those in Fig 3?

Fig 4: Maybe use the same color for the HYCOM curves in each plot? Is the red curve in Fig 3b the same as the black curve in Fig 4b? They seem to both be labelled as demodulates of the HYCOM Eulerian autocovariance, but they seem to have different numeric values ( $R(740hr) < 10m^2$  in Fig 3b, but  $R(740hr) > 10m^2$  in Fig 4b).

l190: "for each particle" -> "for each HYCOM particle" ? But if this paragraph applies to HYCOM, how can there be outliers?

l193: Are the HYCOM Eulerian autocovariances computed all along the trajectory? This would seem to use so many more degrees of freedom compared to the Lagrangian estimates. I am not sure why this is done or why it would be justified.

Paragraph at line 195: This is a very good comparison and a little surprising, to me.

Fig 6: Why are the maps drawn so small?

Fig 7 + 9 + 13 + 14: Please enlarge the maps and panels.

l215: The "fairly constant" ratio is not apparent to me in Fig 7c. Should it be?

l225: Would it be fair to guess that there are also many Argo trajectories that were excluded in the Southern Ocean due to the 0.1m/s drift speed cutoff criterion? I wonder if you would see the difference in HYCOM vs Argo if you made a Fig 8 based on the drift speed?

l227: Once again, "intrinsic" does not seem to be the right word.

l235: Is this an expected property of the Rayleigh distribution for the pdf of the modulated wave amplitude? You might want to look into this in the acoustics or optics literature. I don't believe this has been observed

previously for narrowband ocean internal waves.

I247: Why are you comparing the SCVF<sub>15</sub> statistic? This is a ratio of sample statistics and likely to be very noisy. I don't really know what to make of Fig 9a. With such a small dataset, I would like to see the ratio of total variance (the demodulate amplitude at tau=48hr), instead.

I260: Previously (in the Argo vs HYCOM comparison) you used the ratio of the demodulate amplitude at 48hr lag. Why not use that same quantity for comparison? Oh -- I see it in Table 1.

I262: I am not sure what the "discrepancy" refers to.

I265: SCVF<sup>{15}</sup> -> SCVF\_{15}

I273: I don't think "no impact" is the correct way to characterize the previous results. There is considerable scatter in Fig 5, and Fig 3b shows that the estimates differ. Also, it is unclear to me why you don't try to make the estimates more consistent by extrapolating the demodulate amplitude to zero lag.

I275: "mecanism" -> "mechanism"

I276: Why not just call it "Lagrangian decorrelation" instead of "apparent decorrelation"? If I had been a reviewer on Gaspar-Cohen, I would have made the same suggestion.

I306: "sinusoide" -> "sinusoid"

Table 2: It is interesting that the  $\omega_{AM}$  frequency corresponds to M2-S2 beating, but the amplitude ( $\sigma^2_{AM}$ ) does not.

I330-358: Modes discussion.

I364-377: Bathymetry discussion. Surely the importance of the errors depends

on the horizontal spatial scale of the errors. While this is interesting discussion, it ought to consider the wavenumber spectrum of the error.

I386-401: Stratification.

None of these discussions really deal with the overall quantitative difference of HYCOM vs obs which is about 0.74 (HYCOM/Argo) or 0.51 (HYCOM/Mooring) equatorward of 50 deg. Both datasets have problems in terms of their spatial coverage, but the Argo comparison seems much more meaningful. I am unclear which of the authors' proposed sources of bias could account for the 26% deficit compared to Argo.

I403: "run with" -> "run of"

I416: Shouldn't the factor of 1.5 mentioned here equal the reciprocal of the 0.74 value at I216? Have I misunderstood this?

I422: "stationnary" -> "stationary"; also, I think the "big O" notation should be reserved for asymptotics, and here it is better to say "about" or "approximately". Finally, I don't think "becomes stationary" is an appropriate descriptor; this would be like saying a time series "becomes its mean".

I429: "unaffected" -> "unaffected in the mean"

I339: "supposedly account" -> "supposedly accounts"

I452: latex formatting needs help in the URL.

I454: Zaron's URL has changed to  
<https://ingria.ceoas.oregonstate.edu/~zarone/downloads.html>

References: inconsistent capitalization is used in article titles

I557: "and contributors, T. P."

